Reflections on How Economic Reasoning Can Contribute to the Study of Science

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1. Introduction

In this paper, I discuss a set of issues in the philosophy and sociology of science literatures which economic-style reasoning may help elucidate. My discussion is based upon a particular perspective, one which lies at the core of modern economic theory. For wont of a better term may be referred to as the choice-theoretic approach to socioeconomic modelling.[1]

The tenets this approach takes from mainstream economics are severalfold.[2] First, the behavior of individual actors may be described as outcomes determined by the maximization of individual objective functions. This means in the current context that individuals in a scientific community act in a purposeful fashion, making decisions among available choices in a way consistent with their beliefs, preferences and norms so as to best achieve some sort of payoff. Second, aggregate behavior is either emergent from individual interactions or may be explicitly determined by institutions which are themselves determined by the choices of their members. This requirement imposes a consistency condition between individual motivations and collective outcomes beyond that which is imposed by functionalist or teleological interpretations of aggregate outcomes. From the perspective of the study of science, this second feature in turn means that the behavior of the scientific community as a whole is described exclusively in terms of the

[1]I refer to "choice-theoretic" rather than the more standard "rational choice" models as my analysis does not rely on any particular stance concerning agent rationality.

[2]Elster (1983) takes an essentially equivalent perspective:

> "The basic building block in the social sciences, the elementary unit of explanation, is the individual action guided by some intention." (pg. 20)

union of the individually optimal decisions.[3,4]

The choice-theoretic approach to human behavior supplies a useful complement to both the philosophy and sociology of science literatures by specifying a well posed formal environment in which to study the evolution of scientific theories. The philosophy of science literature (with the possible exception of political theory) is, within philosophy, unique in its attention to positive models of group behavior. Part of the reason why Kuhn's (1970) work on scientific revolutions was so important was its explicit attention to the actual process of scientific practice. Choice-theoretic perspectives are thus a natural methodology in addressing the behavior of the scientific community in order to understand how scientific knowledge is produced and disseminated.

At the outset, it is worth noting two caveats to my discussion. First, my knowledge of the relevant science and science-related literatures is that of an outsider. Hence, a philosopher or sociologist may question either my choice or interpretation of issues in the study of science. Second, this discussion will exclusively focus on those aspects of economic theory with which I am most comfortable. Hence, while it will neglect nonmainstream schools of economics, this should not be interpreted as suggesting these schools have nothing to add beyond the sort of economic reasoning employed here.[5] At the same time, I would certainly defend the relevance of both the general approach as well as the substantive applications which are discussed below.

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[3]Diamond (1988), Goldman and Shaked (1990), Kitcher (1990, 1993), and Brock and Durlauf (1997) are examples of work using this perspective to study scientific activity. The diversity of features of the scientific environments which they study illustrates that choice-theoretic reasoning can encompass extremely different perspectives on scientific activity.

[4]As we shall see, this perspective is compatible with feedbacks from community-level characteristics to individual decisions. What is ruled out is the treatment of the scientific community as a purposeful actor, as implicitly occurs in functionalist explanations, for example.

Section 2 presents some general arguments on how economic reasoning may be applied to science. This discussion sets the stage for the application of such reasoning to specific issues which are prominent in positive descriptions of the scientific enterprise. Section 3 discusses the incommensurability from a choice perspective. Section 4 analyzes progress in science. Section 5 extends this analysis to incorporate dynamic considerations. Section 6 provides conclusions.

2. Economic models of science and the scientific community

This section develops a framework for studying scientific theories which embodies a choice-theoretic perspective. The underlying question which I wish to address is straightforward: how does the distribution of theory choices in a scientific community emerge from individual decisions of scientists which embody their various desires, beliefs, and goals?

i. Why science is not a marketplace

One standard approach in discussions of science has argued that community theory choices emerge through competition in "the marketplace of ideas." This metaphor is most prominent in the Popperian tradition (whose basic ideas are still best described in the original sources such as Popper (1972,1976)) which emphasizes the evolution of theories through the

[5] At the same time, it is important to distinguish the assumption of methodological individualism from other substantive assumptions concerning the environment under study. For example, the school of analytical Marxism (see Roemer (1986) for a survey for this perspective) takes as its goal the reformulation of traditional Marxism in ways which avoid teleological and functional assumptions in favor of a choice-theoretic foundations. Elster (1983) argues that methodological individualism of the type employed here is fundamental to essentially *all* social science modelling.

dynamics of conjecture and refutation. However, this standard metaphor elides as much as it elucidates. Although it is certainly reasonable to regard theories or paradigms as competing for acceptance within the community of scientists, it is far from clear what aspects of this competition are best thought of as taking place in a market. While efforts have been made to argue that scientists compete for citations, funds, prestige, power, etc., none of these desiderata act in ways which are analogous to those objects which are sought or which adjudicate market behavior. To put it most simply, there does not exist, as far as I know, any well-defined model in which the demand and supply of scientific knowledge are equilibrated via any mechanism which resembles a price.[6]

The inappropriate use of market metaphors for the exchange and development of ideas seems to derive from confusion over the relationship between competition in an economist's sense and competition broadly defined. For example, high school track competitions produce payoffs to successful participants without anything resembling a market to adjudicate who are the winners and losers. More germane to the use of market metaphors in the study of science, evolutionary competition is not market competition either.

Why is it important to distinguish market from other types of competition? Because market-based competition, in which the behavior of individual agents is implicitly coordinated by prices, leads to aggregate outcomes whose features depend precisely on this coordinating mechanism. For example, the First Welfare Theorem, which is the formal statement of the conditions under which market interactions lead to efficient outcomes, has no analog for nonmarket contexts. Hence an invisible hand cannot be claimed to apply by analogy to any particular feature of scientific activity.[7]

[6]Mirowski (1996) recognizes the absence of something analogous to prices to achieve coordination among scientists.

ii. **Science as a process of interactions**

Market competition is hardly the basis of all economic analysis; indeed much recent work in economics has focused explicitly on direct, nonmarket interactions.[8] A more promising approach may be developed through the application of interactions-based modelling methods which have developed over the last two decades within economics in response to the need to study economic environments in which agent relationships are not mediated by markets and in which beliefs and preferences are themselves determined endogenously.

A general representation of theory choice in the presence of interactions can be described as follows. Theories are indexed by elements of some set J. In each time period t, scientist i makes a choice of one of these theories, which is denoted as $\omega_{i,t}$. Each scientist makes a theory choice on the basis of solving the maximization problem

$$max_{\omega_{i,t} \in J}$$

$$V(\omega_{i,t}, \underset{\sim}{X}_{i,t}, \mu_i^e(\underset{\sim}{\omega}_{-i,t})) \qquad (1)$$

Here, $V(\cdot, \cdot, \cdot)$ is a function which measures scientist i's payoff or utility from a theory choice. In addition to dependence on the choice, the payoff function additionally is influenced by individual characteristics. In particular, the function contains additional arguments $\underset{\sim}{X}_{i,t}$, which

[7]This is not to say that interesting analyses using market metaphors do not exist. For example, Oomes (1997) studies the issue of how scientists will behave when their goal is to maximize citations.

[8]See Durlauf (1996) for a survey of much of this literature. Also, much of the recent work on evolutionary game theory (Blume (1996), Samuelson (1997) and Vega-Redondo (1997) for excellent recent surveys) involves economic environments in which interactions are not mediated by markets.

represents individual specific characteristics and $\mu_i^e(\underset{\sim}{\omega}_{-i,t})$ which represents the conditional probability measure agent i places on the choices of others at time t. This term embodies individual scientist's beliefs about the choices of his fellow community members and provides the mechanism through which interactions occur.[9]

In order to complete this description, it is necessary to add a rule which describes how an agents' conditional probabilities are formed. Such rules can be generically written as

$$\mu_i^e(\underset{\sim}{\omega}_{-i,t}) = \Gamma(F_{i,t}). \tag{2}$$

Here, $F_{i,t}$ denotes individual i's information set at t and $\Gamma(\cdot)$ is some function which maps information into individual expectations. When individuals are rational in the (economists') standard sense, their beliefs will correspond to the equilibrium conditional expectation of the behavior of others given available information.

One can identify several distinct reasons why the payoff to a scientist from a given theory choice might increase when more scientists are expected to make that choice. First, the beliefs of others may be informative. If individuals with idiosyncratic expertise tend to favor one theory, standard inferential arguments can lead a given scientists to place more weight on that theory than otherwise. Second, there are issues of career-related incentives. Factors ranging from the ability to get funding for research to the ease of publication can incline a given scientist to accept a consensus theory. Third, conformity may occur due to research environment effects. If others tend to follow a given theory, one's productivity in pursuing research associated with that theory may be correspondingly enhanced due

[9]Note that if the $\mu_i^e(\underset{\sim}{\omega}_{-i,t})$ term were not present in the payoff function, this model would be nothing more than the textbook economics version of discrete choice.

to the ability to collaborate, use colleagues as a sounding board for ideas, etc. Fourth, there is ample psychological evidence that conformity in behavior is either hardwired or deeply embedded by our culture. While none of these factors are necessarily dominant in explaining interdependence of the scientific community, all are plausible candidates for the importance of social interactions in individual theory choice. Each of these sources is fully compatible with standard economic reasoning.

As stated, eqs. (1)-(2) have little operational significance, in the sense that most of the properties of community-wide scientific beliefs will depend on the details of the $V(\cdot,\cdot,\cdot)$ and $\Gamma(\cdot)$ functions. What this level of generality does embody, are the ideas that first, theory choice is explicitly comparative,[10,11] second, these comparisons can be represented by a payoff function which embodies the various evaluative criteria which influence individual choice, and third, the behavior of the collection of theory choices for the community as a whole, $\underset{\sim}{\omega}_t$, must be compatible with the individual solutions to eqs. (1)-(2) for all i.

An example of this type of model is provided by Brock and Durlauf (1997).[12] That model provides an analytically tractable framework to study the evolution of theory consensus in a scientific community when individual

[10]This idea is of ancient lineage. For example, Conant (1947) (who is cited approvingly in Williams (1997) in this regard) argues that theories are typically not rejected by scientists regardless of flaws unless a superior alternative is available.

[11]The embedding of theory choice in a comparative framework may well be necessary if one wishes to fully understanding how scientists react to observational evidence. Durlauf (1997) argues that the interpretation of observational weaknesses of a theory is itself dependent on what alternative is available. For example, what might initially appear as an uninteresting anomaly may become a decisive piece of evidence demonstrating the superiority of a new theory.

[12]Brock and Durlauf (1995) provides much of the analytical basis for Brock and Durlauf (1997). See Blume (1993), Brock (1993) and Durlauf (1996) for additional results which are useful in studying interdependent socioeconomic environments.

theory choices are influenced by a combination of both scientific judgment as well as a desire to conform to the beliefs of the scientific community as a whole. When evidentiary support for one theory grows, the model is shown to exhibit highly nonlinear behavior. In particular, the model illustrates how such evidentiary support can lead a scientific community to experience a period of sustained disagreement followed by rapid growth of consensus. This model will be discussed in more detail below.

3. Incommensurability

Incommensurability is an example where the perspective of economics is far different from that of many philosophers. As I understand it, two theories or paradigms are said to be incommensurable when their ontologies differ in such a way that one theory cannot be fully interpreted in the context of the ontology of another.

From the perspective of economic theory, arguments in the philosophy literature about incommensurability are puzzling. As suggested above, economic reasoning naturally conceives of individual scientists choosing one or another theories on the basis of some preference ordering over theories which embodies whatever evaluative criteria a scientist applies. In other words, just as theories address nature as a "corporate whole," to paraphrase Quine (1953), so theories are assessed as corporate wholes whose relative merits are determined through an overall assessment which embodies all of a scientist's evaluative criteria. The lack of ontological translation between theories is irrelevant, so long as a preference ordering which incorporates these criteria can be defined.

Does incommensurability imply that such orderings cannot exist? Certainly not. Consumer theory, in which economic actors maximize utility subject to a budget constraint, has developed a fully coherent theory of choice in which the primitive objects of consumption are not commensurable – indeed this is a context in which one literally compares apples and oranges without difficulty, as the comparisons are only in the context of individual tastes. Similarly, differences in ontology between theories do not imply that there can exist no set of evaluative criteria by which such theories can be ordered.

This argument is an extension of the position taken by Laudan (1996), chapters 2 and 3, that although theories may be underdetermined deductively, they may not be underdetermined "ampliatively" in the sense that nondeductive criteria such as internal and external consistency, explanatory scope, etc., can eliminate underdetermination. I would differ from Laudan primarily in emphasizing that accuracy of correspondence to nature should be treated as one of many criteria for theory evaluation (albeit a critical one). To be clear, this is not to suggest that if one theory may be logically deduced from observation, it would not dominate another which is not. Rather, it means that one theory may better fit a set of observations than another at a particular point in time, yet be rejected at that time because of other criteria. One obvious reason why this is so is that while a theory at one point in time may be less consistent with available observations than another, this will not hold as the two theories are developed over time. Hence, the choice of a theory should be sensitive to a scientist's expectations of its future consistency with observations as well as its contemporaneous success. Such a rejection will of course be facilitated by the belief that refinements of the latter theory will reverse the ordering in observational fit.

The importance of subsuming theory evaluation to a preference ordering which embodies multiple evaluative criteria is of empirical as well as theoretical importance. As noted by Putnam (1996), it is difficult to attribute the overwhelming consensus around theories ranging from quantum mechanics to evolution as stemming from success along a common criterion, be it inference to the best explanation, predictive accuracy, or whatever. Indeed, Putnam's discussion implies that the only possibility for a coherent explanation of the success of different theories is to subsume different evaluative criteria into a preference ordering, which allows the success of a theory along one criterion to weigh against its failure along others.

To the extent that incommensurability arguments have provided one of the major bases by which the concept of scientific progress has been challenged, the choice-theoretic perspective illustrates the flaw in such reasoning. Relative to his preference orderings, progress in science can be defined relative to an individual scientist. If it is the case that the orderings of different scientists exhibit partially similar rankings, some theories will be commonly accepted to represent progress over others.

4. Progress in the presence of "epistemically sullied" agents

As well discussed in Hands (1996), the existence of a set of (at least partially) shared evaluative rankings by members of the scientific community does not have any normative implications for the current state or the evolution of science per se, unless these shared criteria embody some standard such as verisimilitude, superior puzzle-solving ability which can independently be argued to meet some appropriate objective notion of progress.[13]

Further, the embodiment of any given objectively desirable criteria in the judgments of a scientific community is ultimately an empirical question. We know of cases such as Lysenkoism in the Soviet Union, in which

[13]Throughout, I assume that there is an objective reality which science aspires to describe and understand. The assumption of the existence of an objective reality is necessary, as argued by Nagel (1997), in making sense of the very notion of deviations of beliefs from reality.

incentives were set so as to lead scientists to choose (at least publicly) to adhere to theories which by any reasonable measure were errant nonsense. Hence, the question of progress in science is appropriately addressed in the context of the particular world in which we function.

There are in fact two questions involved in the analysis of progress in science. The first is whether, if all scientists are disinterested truth-seekers (or seekers of theories which fulfill whatever additional criteria are regarded as scientifically meritorious), will the scientific community form a consensus in favor of a superior theory regardless of what inferior competitor exists? Second, if in addition to a desire for truth, scientists are "epistemically sullied" (to use Philip Kitcher's (1993) phrase) in that their evaluative criteria involved factors other than this desire, will the scientific consensus nevertheless still be progressive? We address these questions first by discussing under what conditions scientifically superior theories will be chosen over inferior ones at a given time. In the next section we extend the analysis to ask about the long run properties of theory choice.

As is made clear from the framework described in Brock and Durlauf (1997), which studies the average choice behavior of a large community of scientists, the framing of these questions in an economic context indicates that the answer to either question is ambiguous, in that the answer will depend on details of the nature of the scientific community. The intuition of the Brock and Durlauf model is as follows. Individual scientists are assumed to choose theories on the basis of maximizing a special case of eq. (1) where

$$V(\omega_{i,t}, \underset{\sim}{X}_{i,t}, \mu_i^e(\underset{\sim}{\omega}_{-i,t})) =$$

$$h_t\omega_{i,t} + J(\omega_{i,t} - E_{i,t}m_t)^2 + \epsilon_i(\omega_{i,t}) \tag{3}$$

$$E_{i,t}m_t = m_t \tag{4}$$

In this expression, h_t represents a weight which measures the relative strength of scientific support each scientist assigns to theory 1 versus theory 2. The sign of the term indicates whether the term inclines support for theory 1. J represents a conformity weight, the greater J, the greater the desire of an individual scientist to minimize deviations from $E_{i,t}m_t$, the expected average choice in the community. Implicitly, this means that individual expectations exhibit perfect foresight in the sense that the common subjective expected mean corresponds to the realized mean. Finally, $\epsilon_i(\omega_{i,t})$ represents an individual-specific component in assessing theories. Notice that this formulation treats the common payoff h_t as dependent on time. This will occur whenever the evidential support for one theory relative to another is changing in response to research.

This framework allows one to ask under what circumstances a theory which by h_t dominates another is also the consensus choice of the community as a whole. Brock and Durlauf (1997) verifies that this will depend upon the magnitude of h_t, J as well as on the probability distribution of the idiosyncratic terms $\epsilon_i(\omega_{i,t})$. For some specification of these parameters, there will exist multiple self consistent average choice levels, in the sense that at each of these average levels, the distribution of scientists' theory choice is consistent with the beliefs which made those choices individually optimal. For other configurations, the choice level will be unique. Formally, for a given degree of social conformity and a given degree of diversity of idiosyncratic assessments (the distribution of $\epsilon_i(\omega_{i,t})$), there will exist a threshold value H such that if $|h_t| < H$ there will exist multiple self consistent average choice levels, whereas if $|h_t| > H$, then only one self consistent average exists. Intuitively, when the relative evidentiary support for one theory or another is relatively weak, $(|h_t| < H)$, then social factors can dominate scientific ones in determining community theory choice, but when the evidence is sufficiently decisive

($|h_t| > H$), then social factors cannot impede the predominant adoption of a superior theory.

In fact, Brock and Durlauf show more than that. In their framework, so long as a majority of scientists choose that theory with the same sign as h_t, *social factors will facilitate the development of a consensus around the scientifically superior theory.* Hence it is possible for a scientific community of disinterested truthseekers to fail to achieve consensus in favor of a superior theory whereas an epistemically sullied one will do so. This result is quite intuitive. Whenever the conformity effects in a community do not alter the mean away from the scientifically superior theory, then the incentives to conform will only act to bring at least some of the scientists who make inaccurate assessments of the relative merits into the fold.

This basic model also provides some insight into the claim by Bloor (1976) that explanations of scientific belief should be "symmetric" in the sense that the same causal explanation applies regardless of whether the belief is true or false.[14] If by the symmetry thesis one means that the set of determinants of theory choice are the same whether or not the actual theory choice of a particular scientist is true, then the Brock-Durlauf is symmetric. However, if one were to identify the particular causal way in which these different factors enter into individual decisionmaking, or if one were to try to make an assessment of the relative importance of various factors in determining theory choices (a nontrivial exercise given that the different factors determining theory choice enter nonlinearly), then clearly the truth or falsity of a theory matters.[15]

[14]Of course, the Brock-Durlauf model applies to choice between alternative theories, so this discussion is best understood if the model is applied to the case of theory choice where one of the theories is true.

[15]Laudan (1996) provides a related discussion. However, unlike Laudan I would argue that the truth of a theory is relevant for explaining beliefs about because truth, even if ultimately unknowable, will influence evaluative criteria such as predictive accuracy.

This discussion makes two important assumptions. First is that there exists scalar characteristic which measures relative scientific superiority between theories, at least relative to the decision problem each scientist. Second, the discussion assumes that there is no heterogeneity in these scalars, which means that there is agreement on the relative scientific merits. Introduction of heterogeneity does not strengthen the case against progress per se, it merely makes precise mathematical characterization of the conditions slightly more complicated. However, there is a deeper sense in which this heterogeneity might matter. Suppose one does research on a scientific theory which is "better" in a problem solving sense a la Laudan? Successful research will reap especially high returns due to the lack of competition. This possibility is suggestive of some of the issues which arise when one moves to an explicitly dynamic context.

5. Dynamics

The analysis in the previous section was based on asking when a superior theory will dominate an inferior theory at a point in time. However, with respect to assessing whether science is progressive, it is essential to consider the issue of dynamics.

In this respect, there are two defects in the relativist literature. First, there is a tendency to confuse point in time characterizations of the role of social factors with dynamics. Virtually any scientist will acknowledge that, relative to some hypothetical community of epistemically pure researchers, contemporaneous science is such that social factors influence beliefs. However, the failure of science to function independently of social factors has no necessary bearing on the issue of whether science progresses *over time.*

In order to sustain the claim that in the long run, the scientific enterprise is capable of achieving consensus even in the presence of evolving theories, it is necessary to show that the incentives for adoption of a superior theory will not be permanently thwarted by social factors. I am aware of no historical case study in the sociology of science literature which comes even close to making this claim plausible.

Further, it is difficult to countenance the claim that scientific activity can permanently lock into an inferior theory choice, when the theories are sufficiently dissimilar. This can occur in two respects. First, following Brock and Durlauf (1997), when one of these incentives is the desire to work with a scientifically superior theory, the presence of other (social) influences will not imply that these influences will predominate. Rather, they will interact with scientific judgment. In Brock-Durlauf, this means that inferior theories can predominate only if they are "close" to a superior alternative, as measure by h in the previous section. This means that so long as discriminatory evidence between theories accumulates across time, only locally inferior theories can dominate superior ones, so that as long as the body of theories grows, one would express progress.

Second, the sorts of social factors which have been conjectured to produce epistemically sullied agents may themselves lead towards community selection of superior theories. When considering the differences between two theories, differences may (and I would argue normally) be salient with reference to the abilities of these theories to provide predictive and explanatory power, thereby opening up new research, facilitating problem-solving capabilities and hence (possibly) technology, etc. In order for the inferior theory to be stable, it must be the case that the incentives for an individual scientist (or research institution, if it is capable of collective decisionmaking) will never lead the scientist to deviate from the theory choices of others. However, these incentives themselves depend on the relative efficacy of the two theories, as well as the credit which will obtain to a scientist who is the first to pursue various of the new theory's implications. While there is no logical requirement that these incentives render a steady state consensus around an inferior theory unstable, it is certainly plausible, with respect to the particular society in which we function, that individual-level incentives will have this effect.[16] The possibility that the scientific community provides incentives in such a way that scientific progress is facilitated underlies the analysis of both Hull (1988) and Kitcher (1993). The key feature of the environments which they study is that, in the context of science as it is actually practiced, scientifically superior theories create incentives for their adoption.

To be clear, this analysis does not imply that theories will progress according to any particular criterion. Hence a positive approach to scientific knowledge such as that of van Frassan's (1980) which argues against realism, combined with this discussion, would imply that science progresses only in terms of its ability to facilitate prediction and control of nature; indeed it is plausible that the private incentives faced by scientists channel scientific evolution in this direction. A scientific realist would presumably find no conflict between this type of evolution and evolution towards greater verisimilitude in theories.[17] However, to the extent that different evaluative criteria are capable of advancing simultaneously, then the incommensurability of the criteria is consistent with scientific progress, for reasons identical to those which rendered incommensurability questionable in Section 3.[18]

[16]Kitcher (1990) provides good reasons to suppose that the allocation of scientists across different research programs will not be efficient in the sense of maximizing the rate of scientific progress.

[17]It is worth noting that the use of scientific theories in the control as well as prediction of phenomena will strengthen the link between empirical success and realism. Why? Because control typically relies on features of the structure of the phenomenon under analysis rather than its empirical properties. This discussion is standard in economics (especially macroeconomics), where control requires typically the use of structural versus reduced form models. I thank Buz Brock for discussion on this point.

The issue of stability of various steady states in theory choice (or any other decision) is a standard problem in economics. In fact, one of the ways which has proven most fruitful in resolving the selection problem which arises in models with multiple equilibria has been the use of evolutionary stability arguments.[19] Such possibilities are especially salient when one thinks about the evolution of institutions.

In fact, it is ironic that the sociology of science literature has been little affected by recent work on evolutionary game theory. The emergence of the evolutionary game theory literature lies in the same dissatisfaction with neoclassical assumptions about individual rationality and complete markets which has formed the basis of critiques of the use of economic models in analyzing science by Hands (1996) and Mirowski (1996) among others.

At the same time, it is important to recognize that any progressiveness in science due to evolutionary selection arguments is a theoretical possibility rather than a necessary outcome. Thus, this possibility illustrates the incompleteness as opposed to inaccuracy of current claims about the role of social factors in influencing long run scientific progress. Further, Mirowski (1996) argues that the existence of multiple equilibria, a feature which often appears in dynamic games, is itself evidence

[18]The possibility that scientific theories can progress simultaneously along different normative dimensions is important in seeing why choice-theoretic reasoning probably has little relevance to the issue of incommensurable moral values of the type discussed in Berlin (1968) and Gray (1996). Berlin and Gray emphasize that the incommensurability in moral contexts is associated with explicit *conflicts* between values, for example (given by Gray) the conflict between the right to privacy and the right to information about others. While conflicts between scientific values cannot be ruled out *ex ante*, conflicts do not appear to be as intrinsic in this context as in the context of ethics. Of course, if the set of ethical values is finite, then some societal configurations can unambiguously dominate others, but this will, because of the range of conflict, provide little consensus within and across societies.

[19]See Samuelson (1997) for a good survey of the issues revolving around equilibrium selection.

that evolutionary dynamics provide little comfort for those who would appeal to evolutionary arguments to establish scientific progressivity.[20]

However, Mirowski's conclusion is, in my judgment, unwarranted. First, any economic model which possesses multiple equilibria can be augmented with an equilibrium selection mechanism. Such a selection mechanism is not necessarily an *ad hoc* addition, but rather an integral feature of the economic environment under study. For example, arguments that market competition selects for profit maximizing firms rely on clear economic logic. Second, any criticism of a particular model of science on the basis that a class of more abstract models is associated with multiple equilibria is, in terms of relevance to understanding actual science, besides the point. So long as the particular model is specified in such a way as to produce determinate results, the model can be assessed on its own merits.

More generally, the issue for progress in science is not whether a particular sample path for the evolution of theories is unique, or whether the accumulation of knowledge up to some point in time is unique given certain initial conditions. Rather, it is whether those sample paths which are possible outcomes of the actual microstructure of science do or do not contain certain features which can be characterized as exhibiting progress. Claims concerning the folk theorem in game theory or the failure of general equilibrium theory to produce determinate empirical implications in the sense of the Debreu-Sonnenschein-Mantel theorem simply do not speak to this latter consideration.

This distinction between point in time characteristics of the scientific community and long run scientific progress is a manifestation of the danger

[20]Mirowski (1996) concludes:

> "Once one explicitly commits to a specific game theoretic solution concept, then it is child's play to subvert any invisible hand story in science." (pg. 164)

of taking market metaphors for science too literally. As discussed above, the first welfare theorem relates to a broad conception of efficiency in market allocations. Progress in science requires no such conception. Progressiveness relates to whether the theory of relativity is something our society would inevitably have discovered and accepted, not whether it could have been developed prior to 1905, which is the sort of question Pareto-like economic efficiency analyses address. While social factors may impede the rate of scientific progress, they have no necessary implications for whether such progress exists. Even the success of Lysenkoism, which is a clear case of scientific regress, did not represent a steady state for Soviet genetics.

In this respect, suggestions that deviations in the practice of science from the conditions which underlie efficiency conditions in neoclassical economic environments invalidate claims of progressiveness are simply not germane.

6. Conclusions

For a phenomena as multifaceted and diverse as science, economic reasoning is by no means a panacea for understanding. Rather, economic reasoning provides a powerful perspective for dealing with standard problems which have arisen in the study of science. In this essay, I have attempted to illustrate the power of economic reasoning in the context of first, the commensurability of alternative theories and second, in the progressivity of theory choice. Taken together, my suggestion is that an explicit choice-theoretic approach to scientific paradigms can help solidify the case that science is indeed a progressive enterprise.

To be clear, choice-theoretic models do not possess unambiguous implications for the progressiveness of science, or any other aggregate characteristic. The specific nature of social interactions and individual incentives ultimately determines these aggregate characteristics. What theoretical models of scientific theory choice do achieve is a clarification of what empirical factors will ultimately drive the aggregate characteristics. In this regard, economic models of science represent an important complement to the historical studies which underlie the sociology of science literature. If nothing else, models such as Brock-Durlauf suggest that claims questioning the progressiveness of science made by writers starting with Feyerabend (1976) and Bloor (1976) and strongly defended by current philosophers such as Fuller (1988) may, ironically, themselves be as undetermined by the data as those theories which they have diagnosed as suffering this limitation.

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