Limits to Science or Limits to Epistemology?

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1. Introduction

A recent spate of writings has argued that in a fundamental sense, science is reaching limits.[1] The basic claims of this work are twofold. First, it is argued that for many areas of inquiry, notably physics, chemistry, and biology, science has provided an essentially accurate view of nature. The theories of evolution, relativity, and quantum mechanics are taken as primary examples of scientific success. Further advances on these successful theories about nature will be derivative of these existing broad theories. Second, it is argued that in those areas where scientific inquiry cannot claim great empirical successes, fundamental advances are impossible. Unverifiability may occur either because of the lack of distinct empirical predictions of the theories or because of the nonexistence of feasible experiments to allow discrimination. Cognitive and social sciences are often cited as examples of disciplines doomed to permanent failure.

This paper argues that the claims about limits to science are essentially unpersuasive. First, I claim that there is no basis for concluding that a successful science is at the end of fundamental breakthroughs. Second, I argue that the claim that social science, as an example of a supposedly unsuccessful science, is incapable of producing successful models,

[1]The best known of these is Horgan (1996) who launches a broad attack on the possibility of future fundamental breakthroughs. Other recent writings include Ziman (1996) who argues that funding limitations will prevent the development of sufficiently strong empirical evidence to provide the impetus to fundamental advances and McGinn (1993) who argues that consciousness and the nature of the mind are phenomena which are insuperably difficult for humans to comprehend. If correct, this argument would apply to scientific investigations as well as philosophical ones. Concerns over the end of scientific discoveries appear to resurface periodically. See Rescher (1978) for a survey (and criticism) of related concerns two decades ago and Feynman (1965) for similar thoughts.

is unsupported. Here, I focus on economics given my professional training, but the arguments apply more broadly. In short, it appears more plausible that there are fundamental barriers to our ability to identify bounds on science than that we have reached such barriers, so that it is the epistemology of science rather than science *per se* which may face fundamental limits.[2]

2. Verification and Limits

The argument that the successes of current biological and physical theories are such that future fundamental changes in these theories will not occur, rests on the view that there is a well-defined metric by which one can measure the accuracy of a theory against the whole of nature. Unless one wishes to claim that the current body of theory already explains all of nature, one needs an explanation of the discrepancies theory and observation which is compatible with the limits argument. One possibility is that all reconciliation of discrepancies is beyond human ken. Alternatively, at least some of these discrepancies can be resolved through inessential adjustments of the theory under question.[3] This requirement, however, would in turn imply that at a given moment in time, a determination could be made. However, the resolution of discrepancies cannot be known *ex ante*.

A good example of this inability is the behavior of Mercury's orbit which represents in retrospect a critical corroboration of the General Theory

[2]Chalmers (1994) provides a superb survey of the philosophy of science.

[3]Notice that this argument does not require that observations be defined independent of theory, and so does not suffer from the criticisms of Quine (1953). However, as there is no disagreement in economics on the meaning of observations (as opposed to their explanation), I will take the meaning of observation statements to be constant across the range of theories under consideration.

of Relativity as an alternative to Newton's Theory of Gravitation. An irregularity in the orbit relative to Newton was well known for many years preceding Einstein's work. However, the inability of classical theory to explain it was regarded as an "anomaly" (to use Kuhn's (1970) term) rather than as evidence of a deep flaw in the theory. A similar anomaly existed with respect to the orbit of Uranus. Astronomers conjectured that the anomaly could be explained by a planet beyond Uranus, which led to the discovery of Neptune. Similarly, it was believed that an as-yet undiscovered planet interior to Mercury, designated as Vulcan, would explain the aberration.

In retrospect, of course, this belief proved to be wrong. However, the relevant issue in identifying a steady state in our theoretical conceptions is whether one can know at the current time whether all discrepancies between theory and observation are anomalies. The lesson of the aberration in Mercury's orbit is that knowledge of the meaning and implications of a empirical failure of current theory, however, is impossible, *in absence of the new theory which will supplant the old.*

At an abstract level, this claim is nothing more than the recognition that the interpretation of the failure or success of a theory is a function of the body of alternative theories. Putnam (1974) makes essentially this claim in his analysis of the evolution of science. Putnam argues that no theory is testable in isolation; rather, theories are rendered testable by the addition of "auxiliary statements" which render the theory's predictions observable.[4] Hence any empirical result can be reconciled with a rejection of the auxiliary statements rather than the theory itself. Of course, the plausibility of rejecting an auxiliary statement will depend upon the background of background science in which the rejection must occur; for

[4]Putnam's discussion of the process of theory corroboration bears close relation to Quine's (1953) argument that theories are undetermined by observations.

example, one will not reject the reliability of instruments without good reason. The upshot of Putnam's argument, however, is that there are no *a priori* implications of discrepancies between theory and observation for the prospects of a fundamental shift from one theory to another.

The implication of this argument is simple. So long as we are in a state in which at least some observations are inconsistent with current theories, there is no basis for assigning probabilities to the directions along which a reconciliation will occur.[5]

3. The Evolution of Observations

The argument that discrepancies between existing theories and facts imply that it is impossible to assess the probability of fundamental theoretical shifts is bolstered by considering the evolution of the facts themselves. Discussions of limits to science have tended to treat the set of extant observations as static, when in fact this set evolves through the process of research and often due to the evolution of theory. When Einstein published the General Theory of Relativity, he proposed three ways to empirically distinguish his theory from Newton's, of which two – the bending of light around the sun and the slowing of clocks in a gravity field – were not related to any known observations. (The third related to Mercury's orbit which, as discussed above, was already known to be anomalous relative to Newton's theory.) In fact, Eddington's famous study of the solar eclipse in 1919 was undertaken specifically to test one of these implications of the General Theory.[6]

This feedback from theory to the class of observations, however,

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[5]See Cranston (1953) for the related argument that the nature of new inventions is unpredictable.

[6]This account follows Pagels (1982).

introduces a second dimension along which forecasts of limits to science become uninterpretable. Observations which are uninteresting from the perspective of corroboration of one theory may be important from the perspective of another. In economics, the relationship between the variance of the unpredictable component of the Phillips curve and the slope of the inflation/output tradeoff is irrelevant to neo-Keynesian theories but is essential to new classical theories, and so was not investigated until Robert Lucas's seminal work on expectations and economic fluctuations (cf. Lucas (1972,1973)). Therefore, even if it is true that current theories well approximate current fact, this is a statement which holds only relative to the set of empirical phenomena which current theories identify as salient. The unpredictability of new theories thus implies the unpredictability of new salient phenomena.

4. The Determinants of Scientific Revolutions and Limits

A further source of the inability to predict the end of scientific revolutions stems from the intrinsic unpredictability of what constitutes a fundamental shift in theory. This issue is different from the claim (certainly true by analogy to the law of iterated expectations) that if we could predict the shift, we would have implemented it already. Rather, I argue that we cannot predict whether a shift will occur. I make this claim on the basis that fundamental theoretical shifts represent changes in the primitives (or, following Quine (1967), natural kinds) from which theories are constructed.

One way to think about this is the following. Competing research programs may be thought of as combinations of natural kinds. A current research program may be thought of as assigning a probability of one to a class of models each of which is consistent with one set of natural kinds. This roughly corresponds either to Kuhn's idea of a "paradigm" or Lakatos'

(1970) idea of the "hard core" of a research program. An alternative research program is one which assigns positive probability to an alternative class of models, some part of which is not measurable with respect to the original class. Put differently, claims about the improbability of future scientific revolutions are equivalent to efforts to define probabilities in absence of knowledge of the state space of events. The latter is mathematically meaningless. Notice that in the formulation, the two research programs are incommensurable in Kuhn's (1970) sense.

However, there is a second sense dimension along which the primitives of a theory are fundamental. The previous example implicitly assumed that once the natural kinds of a theory were defined, one could work out all the relationships between them. However, the history of science tells us that this is far from the case. The choice of natural kinds plays a critical role in determining the sorts of questions which are answerable in practice. Hence, even if the natural kinds of one theory can be translated into those of another, the ability to successfully theorize may not be preserved.

One can go further and argue that theories differ not only with respect to primitives but also with respect to the rules by which primitives are combined. By analogy, languages differ with respect to syntax as well as vocabulary. Combinations of certain observational outcomes and theoretical outcomes might therefore be easy to construct in one theory and difficult or impossible in another due to the differences in combination rules. This is certainly true in economics, where behavioral assumptions represent rules which delimit the ways in which individual agents are conceptualized as interacting.

Quantum mechanics is a good illustration of the importance of new primitives in leading to fundamental breakthroughs. The fundamental conceptual breakthroughs of the 1920's had to do with a reconceptualization of the nature of reality at a subatomic level. Until that time, particles were typically conceptualized as objects whose physical properties could be analogized to macroscopic objects, hence the quandary associated with whether light was a particle or wave. An essential feature of quantum mechanics was the recognition that the thought categories inherited from macroscopic behavior were simply incorrect at a sufficiently microscopic level. Particles were reconceptualized in terms of properties which were inconsistent with "folk physics." This profound shift in theoretical vocabulary represents, I believe, the meaning of Richard Feynman's famous remark that "nobody understands quantum mechanics" (1965, p. 129).

This belief that the natural kinds which dominate common sense thinking may be poor scientific guides underlies the school of eliminative materialism in cognitive science. Eliminative materialists such as P. M. Churchland (1979) and P. S. Churchland (1986) argue that the categories of folk psychology such as belief and emotion, are not reducible to brain states because these categories are not natural kinds with reference to a successful scientific theory of the mind. They therefore argue that neuroscience will not only provide a fully materialist model of the mind, but will lead to a new set of natural kinds with which to characterize psychology. The example of quantum mechanics lends plausibility to their view. Further, the development of quantum mechanics suggests that claims of limits to human cognitive capacity will forever preclude an understanding of the mind (McGinn (1993)), are not credible due to the potential of future vocabulary shifts of a similar kind.[7]

A similar phenomenon occurred in economics over the last three decades. In the 1960's, it was common to take aggregate economic phenomena such as the Phillips Curve to be primitives in macroeconomic theories. A key consequence of the rise of new classical macroeconomics has

[7]See McIntyre (1993) makes the related argument that changes in the vocabulary of a theory can simplify the characterization of a system, which is used to counter the claim, (also discussed below), that the complexity of social phenomena precludes the discovery of social laws.

been a change in the primitive constituents of aggregate economic models: while Keynesian models employed aggregate structural relationships as primitives, in new classical models individual agents are the primitives so that all aggregate relationships are emergent. This change in perspective in turn has led to many new formulations of economic questions, such as the impact of government policies where policies are conceived as state dependent policy rules rather than as a sequence of static choices. In this latter case, it should be recognized that the introduction of particular *ad hoc* expectations into the old-style Phillips curve models would have rendered them observationally equivalent to their new classical counterparts. This is precisely the sort of theoretical advance which is extremely difficult to recognize within the syntactic rules of the older Keynesian theory and was only be recognized once the new classical theories were developed.

Thus, to the extent that fundamental shifts in theory are either accompanied or caused by fundamental shifts in the vocabulary of scientific discourse, the assignment of probabilities to the process of theory change is ill-defined. Different theoretical frameworks, whether interpreted along the particulars of Lakatosian hard cores or Kuhnian paradigms, embody particular vocabularies and syntaxes which render meaningful predictions about existence or nonexistence of alternative new languages impossible.

5. Social Science

Unlike the natural sciences, arguments that the social sciences have little prospect of providing major advances have not been based upon the success of current theories. Rather, the arguments have more often been based on the claim that social sciences are incapable of exhibiting predictive laws of comparable accuracy to those found in the natural sciences.[8,9]

The claim that the predictive power of the social sciences has been far exceeded by that of the natural sciences is undoubtedly true, and I find it as a practical matter inconceivable that this will change, given the standard for predictability which philosophers have typically employed. For example, Searle (1984) bases his rejection of the possibility of laws of social science on the inability of any model to predict an individual's voting behavior perfectly. As a practical matter this argument is clearly correct because of the high dimensionality of the factors which would need to be accurately measured and whose impact on one individual's voting behavior would need to be known with certainty.

However, this type of argument is irrelevant to the potential for meaningful economic laws. The goal of prediction in economics most naturally concerns the regularities which exist across the behavior of many agents. One is little interested in the output of a single worker; rather, one is interested in aggregate GDP. By analogy, no physicist would claim that the body of theory on subatomic particles will ever provide perfect or even accurate prediction of the topography of the earth. At the same time, intrinsic unpredictability of individuals has no implication for the unpredictability of aggregates. In the physical sciences, statistical mechanical models of phenomena such as magnets and glasses have precisely this property.[10]

[8]By predictive law, I mean a formal model of economic phenomena, which combined with some set of initial conditions and auxiliary assumptions, produces observational predications. Alternative predictions are made on the basis of altering the initial conditions and auxiliary assumptions. Of course, the distinction between initial conditions and the substance of a theory is ambiguous at an abstract level but is operationally irrelevant, at least when it comes to economics theory.

[9]Here, I ignore the argument made by Rosenberg (1983) that economics is not a science and therefore cannot produce predictive laws because its practitioners are unaffected by empirical shortcomings as I have yet to see an example of this claim which reflects any understanding of economics as it is actually practiced. I do not regard it as coincidental that accounts such as that of Hausman (1992) which reflect an understanding of economics also conclude that economics is a legitimate science.

Further, equating scientific success with general predictive accuracy is at best a limited view of the science in general, and of the social sciences in particular.[11,12] Social sciences possess two additional goals which are independent of general predictive accuracy. First, the social sciences seek to explain behavior. Second, the social sciences seek to understand how various purposive interventions will alter socioeconomic configurations.

The major schools of economic thought have embodied world views that have been of enormous interpretive value. Regardless of one's views of Marxism as an ideology, there is no doubt that our understanding of economics, sociology and history has been profoundly influenced by Marxist perspectives. Berlin (1978), hardly a follower, defends the interpretive contribution of Marx from the perspective of historiography, in particular arguing that Marx revolutionized the study of history by introducing considerations of economic factors as they impinge on political ones. Similarly, Keynesian economics provided an essential clarification of the role of incomplete markets and price stickiness in the determination of aggregate output. The role of these factors has informed the formulation of its rival new classical macroeconomics. Similarly, the emphasis on the role of expectations in influencing individual behavior is now an integral part of

[10]See Anderson (1972) for a general discussion of this issue.

[11]Putnam (1994) argues that there is no single goal which explains the development of all scientific theories, and that particular theories succeed relative to different goals. My argument is that each scientific enterprise embodies multiple, and possibly conflicting, goals.

[12]This claim is inconsistent with Friedman's (1953) emphasis on predictability as the primary goal of economic analysis. In my view, Friedman's essay, by ignoring the distinction between prediction and explanation, articulates an impoverished view of the intent of most research. The importance of explanation does not, however, invalidate Friedman's claim that the falsity of model assumptions is consistent with a model's being useful. Following Gibbard and Varian (1978), false assumptions lead to useful explanatory models when either the assumptions are approximately correct, or when the purpose of the model is to produce a "caricature" which elucidates a particular set of economic principles.

neo-Keynesian economics.

The essential role of collective decisionmaking at either the government or private sector levels in the economy makes the development of models which explain the consequences of these decisions integral to the social science enterprise. In fact, this goal of social science indicates that the standard of predictive accuracy is too vague, as one is really asking here about the predictive accuracy of social science models with respect to the effects of changes in policy. There is no logical reason why a model cannot simultaneously exhibit low general predictive accuracy while at the same time exhibiting high predictive accuracy with respect to changes in particular variables. The theory of evolution for example, has essentially no predictive power with reference to the particular distribution of species types on Earth. At the same time, as discussed in Nesse and Williams (1994), evolutionary theory can provide precise predictions about the effects of different medical interventions against viruses, conditional on this distribution.

It is easy to characterize cases in which economic science has progressed in its ability to inform government policy. Among macroeconomic examples, I would include the following. First, Keynesian economics led to the development of a theory of countercyclical stabilization policies.[13] Countercyclical stabilization policies have made the prospect of a recurrence of a 1930's type depression negligible if not zero and certainly the postwar environment has not experienced any disruption which even approximates the 1930's. Second, the rational expectations revolution has led to a reformulation of policy analysis from a consideration of the consequences of isolated static policy choices to an analysis of the effects of different policy regimes. For example, the analysis of intertemporal

[13] Of my examples, this is the one most likely to create controversy among economists, although I believe a large majority would agree with my claim.

maximization predicts for that temporary income tax cuts should have small effects on consumption whereas temporary investment tax credits should have large investment effects. This seemingly paradoxical result can be understood once one recognizes that from the perspective of most consumers, a temporary tax change has little effect on lifetime wealth and therefore little consequence for the consumption of nondurables for a forward looking agent who seeks to maintain a high level of consumption for all periods of life. On the other hand, alterations in the timing of durable investment purchases make sense in light of tax differences since alterations in the timing of durables purchases to take advantage of tax breaks will have little effect of profits. Both predictions are borne out empirically (Okun (1971) and Auerbach and Hassett (1992) respectively). Among microeconomic examples, economic theory suggests for example that rent controls can hurt the poor by reducing the supply of available housing, a phenomenon that is widely recognized.

Even if the goal of economics is the development of successful general predictive laws, the possibility of major advances is still very much open, as most of the arguments which have been made against the possibility of accurate forecasts are suspect. To justify this claim, I review some of the major arguments against economics as capable of generating laws, and see whether the arguments apply equally well to natural sciences where well understood laws do exist. In this discussion, I will implicitly assume that any laws in economics are, in Hausman's (1993) term "inexact," by which I mean that the predictive power of a given law for a given phenomenon will is normally incomplete. This leaves unresolved questions such how well a model must predict in order for it to be considered a success and how to interpret predictive failures, but such questions plague virtually all science and are immaterial to the present discussion.

First, it has been argued (Hayek (1967)) that the range of factors which determine economic outcomes is too large to permit identification of a useful predictive model. This type of argument is not decisive, however, on several grounds. The presence of an arbitrarily large number of omitted variables is no necessary obstacle to highly accurate prediction. By itself, the absence of any deterministic relationship is thus by itself irrelevant. Further, in aggregate behavior, we are typically interested in the role of different factors that alter the conditional probabilities of various outcomes. So long as some sort of law of large numbers applies to the averages of a group, the existence of many omitted idiosyncratic factors will be irrelevant.[14]

Hayek recognizes the power of statistical arguments against his position, and asserts that while general regularities may be identifiable, the sorts of complex patterns which characterize economic phenomena may not be. This argument is not generally correct. The many results in complex systems theory tell us that complex patterns of behavior can be generated by tightly parameterized models. An updated version of Hayek's argument might, however, run along the following lines. The sorts of complex systems which can exhibit rich pattern formation (e.g. statistical mechanics models such as ferromagnets or spin glasses) are typically nonergodic and hence exhibit average features that depend upon the system-specific realization of shocks. Such shocks vary from economic environment to environment and hence function as a set of parameters that cannot be identified.

However, nonergodicity per se does not represent an insuperable barrier to predictive, as opposed to deterministic economic laws. Specific formulations of nonergodic economic models typically tightly constrain the class of possible equilibrium configurations (see Arthur (1989), Blume

[14]At the same time, there exist deep questions about how to evaluate the empirical fit of economic models. For example, deep questions exist as to what level of microeconomic structure must be known before general equilibrium models can be rendered potentially falsifiable (see Sonnenschein (1972) and Brown and Matzkin (1996)). For partial equilibrium models, there is a vast literature on the identification of structural economic models from data; an important recent contribution is Manski (1995).

(1993), Brock and Durlauf (1995), Krugman (1996) for examples). Furthermore, these models can provide exact predictions of the consequences of policy interventions. Similar considerations apply to nonergodic models in the physical sciences. Nonergodic statistical mechanics models form the basis for the theory of magnetism, yet are perfectly capable of prediction (e.g. heating a magnet will eliminate the magnetization.) In fact, models such as those studied by Blume and Brock and Durlauf are mathematically equivalent to well understood physics models of statistical mechanics and so have inherited their identifiability.

Second, it has been claimed (Searle (1984)) that the fact that social phenomena are functions of the intentions of individuals implies the nonexistence of lawlike regularities. This argument is based on the idea that an object such as money is anything which a society collectively treats as money, there can be no law of monetary phenomena. This argument is irrelevant to the practice of social science. Social science seeks laws which predict one phenomena conditioning on other social phenomena or aggregate phenomena. For example, economic laws are sought which relate interest rate behavior to monetary policy or trading patterns to geographical factors. Searle's argument at best establishes nonexistence of any bridge law which allows a reduction of social laws to natural ones. This lack of reducibility is not unique to social phenomena. As Anderson (1972) argues, the properties of condensed matter typically cannot be reduced to the laws of subatomic physics. Searle's argument is suggestive of the autonomy of laws in the social science, not of their nonexistence.[15]

To the extent that Searle's objection is germane, it would require that the self-referential nature of social phenomena renders them independent of law-like behavior, which simply does not follow.[16] Even if

[15]See Wolfe (1993) for a recent defense of the autonomy of the social sciences along similar lines.

[16]See Kincaid (1990) for additional criticisms of Searle (1984).

self-referential behavior produces nonergodicity, this has no relationship to the possibility of conditional predictive accuracy. Nonergodicity implies that different sample path realizations of the economy will produce different aggregate statistics. It does not imply that along a sample path, prediction is impossible. In fact, economics is filled with examples of models in which expectations are at least partially self fulfilling, none of which is incapable of standard statistical treatments.

Third, Fay (1983) has argued that the discovery of social science laws is made impossible by the open-ended nature of social systems; Popper (1957) makes related arguments. By open-ended, Fay means that the properties of social systems are unpredictable because of the presence of external effects which continually change the equilibrium properties of the system. These changes, in turn, occur, because of the role of beliefs in determining social configurations.

"human actions and institutions are...unpredictable in so far as they change as a result of people's coming to think of themselves, each other, and the natural world in novel ways...Social institutions and practices, as well as the beliefs and desires of the members of particular social groups are continually in a state of flux and evolution which will always appear to be indeterminate to those who wish to study them." (pg. 103)

Fay's argument differs from the previous ones in emphasizing the role of nonstationarity in social phenomena. This argument is successful in identifying a fundamental difference between social and natural phenomena which is relevant to prediction. To the extent that the data generating process characterizing the economy is changing across time, the possibility of statistical identification is correspondingly diminished. Formulated in these terms, however, it is clear that Fay's argument is a claim about the predictive horizon for social laws, rather than a demonstration of the nonexistence of such laws. The sort of nonstationarity with which Fay is concerned implies that the predictive horizon of social science models may be short. I have little doubt this is true for technical change, for example. On the other hand, changes in the data generating process are in principle statistically identifiable, and little evidence exists in the postwar macroeconomic data for the sorts of nonstationarity which would render Fay's objection fatal to the prospect of successful social science prediction.

Notice as well that there is a way to formalize many of the objections to the identification of economic laws. Let $\underset{\sim}{Y}_t$ denote the vector of endogenous variables which a theory purports to explain and predict at time t, $\underset{\sim}{X}_t$ the set of observable exogenous variables which influence the endogenous variables and $\underset{\sim}{\epsilon}_t$ the vector of unobservable variables which influence the prediction of the theory.[17] A theory is then formalized as a mapping $\Phi(\cdot)$ of the form

$$\underset{\sim}{Y}_t = \Phi(\underset{\sim}{X}_t, \underset{\sim}{Y}_t, \underset{\sim}{\epsilon}_t)$$

Econometric analysis of a model focuses on the identification of $\Phi(\cdot)$. Each of the critiques discussed can be mapped into a statistical question concerning how data can reveal $\Phi(\cdot)$. Hayek's argument on complexity can be reinterpreted as the statement that the number of elements of $\underset{\sim}{X}_t$ is so vast that the identification of Φ from a available data is impossible. Searle's concern about the social nature of economic phenomena is interpretable as the argument that the interaction of each of the endogenous variables with one another (expressed here as the presence of $\underset{\sim}{Y}_t$ as an argument of $\Phi(\cdot)$) creates an indeterminacy in the system. (Searle might be alternatively interpreted as worrying about the identification of models in which expectations of $\underset{\sim}{Y}_t$ affect realizations of $\underset{\sim}{Y}_t$). Fay's argument concerning the openendedness of social systems is a claim that $\Phi(\cdot)$ is itself time varying.

[17] In economics, predetermined variables typically include both those variables which the theory of interest does not attempt to explain as well as past values of the endogenous variables.

Each of these concerns has in fact been the subject of study within the econometrics and statistics literatures.

From the perspective of those literatures, none of these objections has been shown to be generically insuperable in way in which these objections have been made. This is not to say that cases do not exist where the prospect of successful identification of social science laws in impossible. There exist well studied cases in which theories with very different implications are observationally indistinguishable due to some of the factors which have been alleged to militate against the existence of any social science laws whatsoever. For example, Durlauf and Johnson (1995) show that how broad classes of neoclassical and new growth models are observationally equivalent when analyzed at the level of cross-country behavior, using a formalized variant of argument of Hayek's claim that the historical record is too sparse to permit identification of relevant differences between the two theories.

To be sure, none of this discussion implies that limits to the discovery of social laws do not exist. In particular cases, limits no doubt can be found. It is worth recalling that the deepest result on limits to knowledge which has yet been discovered – Gödel's incompleteness theorem – exists relative to a particular system, namely first order arithmetic. The common defect of arguments against the existence of social science laws is to think that strong conclusions concerning the existence of such laws can be made on a generic and *a priori* basis.

6. Conclusions

The basic point of this paper may be summarized in the statement that it is more plausible to believe that our ability is limited in identifying generic limits to scientific breakthroughs than it is to believe that such generic limits have been reached. For the sciences in general, the implications of existing discrepancies between current theories and observations cannot be known. Further, for the social sciences, the standard arguments that have been made to deny the possible discovery of new scientific laws appear to be either spurious or irrelevant. I therefore conclude that reports of the end of science, natural or social, have been greatly exaggerated.

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