Economic Methodology is Dead - Long Live Economic Methodology: Thirteen Theses on Progress in the New Economic Methodology*

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February 2000

*Paper prepared for a session on "Progress in Economic Methodology Since Helsinki 1989" for the European Society for the History of Economic Thought
I. Introduction

A decade ago my paper for the Helsinki symposium on "The State and Prospects of Economic Methodology" opened by referring to the "explosion in the literature on economic methodology" (Hands, 1990). If the literature of the 1970s and 1980s constituted an explosion, then I am at a loss for words to describe the outpouring of methodological literature that has appeared during the 1990s. A multiple explosion perhaps? Whatever we choose to call it, the literature of the last decade makes the work of the earlier period seem pale in comparison. Not only have we seen continued growth in the volume of methodological research, the representative contribution now seems to exhibit a much greater familiarity with the relevant literature (philosophical, economic, or other). Commensurate with this increase in quantity and quality, there has also been a growth in other indicators of subdisciplinary success: e.g. journals, conferences, and graduate programs. By most obvious measures the last decade has been a period of great progress in the field of economic methodology.

While the field has exhibited progress with respect to these obvious measures, I claim that in one very significant way recent economic methodology has actually been a rather profound failure. Not only has the field failed to exhibit progress, the failure has occurred with respect to the question that was considered to be the most important single question -- the key defining objective -- for economic methodology during most of the twentieth century. That objective was to find a few clearly specified methodological rules for the proper conduct of economic science; these rules, if followed, would guarantee that the resulting economic theory constituted legitimate scientific knowledge. With
respect to this fundamental goal -- the search for simple methodological rules -- economic methodology has not exhibited any progress.

Given the failure to make progress on such an important goal, one might expect the following pages to retract my earlier claims regarding methodological progress and perhaps even provide an examination of how this failure took place. Such an expectation would not be fulfilled. In fact, not only do I still defend my initial "thirteen theses," this paper will add another thirteen more theses regarding progress in the field of economic methodology. How can this be? How can one argue that the discipline has failed to deliver the intellectual goods with respect to its most fundamental goal and yet still argue that it has exhibited substantive progress? The answer to this question comes in two parts. The first concerns the nature of the question that methodologists have failed to answer, and the second involves the progressive broadening that has taken place as a result of this failure.

The failure to find the correct methodological rules for the proper conduct of scientific economics does not reflect negatively on the work of those doing economic methodology; instead, it simply represents one minor aspect of the broad-based failure of the science theory community to discover such methodological rules for the conduct of any science (or science in general). As a result of a number of different developments within the fields of history, philosophy, and sociology of science (some of which are discussed in the following thirteen theses) the practitioners of general science theory have basically abandoned the project of finding a few simple methodological rules that are necessary and sufficient to demarcate science from nonscience (or nonsense, or the nonempirically-verifiable, or the non-cognitively-significant, or any related distinction). From the viewpoint of contemporary science theory that particular rule-seeking project was a mid-twentieth century philosophy of science will-o'-the-wisp; it represented a major investment in philosophical resources that simply didn't pan out. For a few this failure has led to some type

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1 That is of course assuming that only a positive solution - finding such a set of rules - counts as success. If one counts rejecting certain rules as inadequate, then there has been success (we now know that a lot of previously proffered rules are inadequate). In what follows I will make the (non-Popperian) assumption that only positive solutions count as progress toward the objective of finding the proper methodological rules.

2 I say "science theory" to indicate that it is not just the philosophy of science per se, but also more general philosophical work that bears on the study of science, the history of science, the rhetoric of science, and science studies (sociology of science, sociology of scientific knowledge and related approaches).
of radical relativism about scientific knowledge, but for most it has simply changed the subject; in particular it has broadened the range of questions that might be asked and the range of different approaches that might be employed in the investigation of the character of scientific knowledge, the structure of scientific institutions, and the nature of scientific rationality. The failure of economic methodologists to find an acceptable answer to the question of the proper methodological rules simply parallels these developments in science theory more generally. In effect, economic methodologists failed to find such rules because there are none to be found (or to be less absolutist about it: none have been found in the places where they traditionally seemed most likely to appear: successful natural science).

Not only does the failure to deliver the intellectual goods on this key question -- let's call it failure with respect to "rule-progress" -- not reflect badly on the efforts of those writing economic methodology, I claim that it has facilitated a new and quite progressive turn within the broader field of methodological inquiry. The lack of success on the single narrow question of finding the correct methodological rules has expanded our intellectual horizons; new lines of inquiry have been considered; new tools have been employed; and a whole new progressive era has begun. Liberated from the strictly binding constraint of rules-based methodology -- removing those blinders -- we are now free to consider a multiplicity of new and intellectually exciting points of contact between economics and the study of scientific knowledge. Not only are we free to investigate an expanded range of topics, tools, and ideas; we have in fact been successfully doing so during the last decade. If we define the new economic methodology as any inquiry that substantially involves both economics and science theory, then the last decade has exhibited substantial progress in this newly expanded field. The deregulation has been successful; the rule-blinders have fallen away and intellectual innovation abounds. The old economic methodology is dead: long live the new economic methodology.

In the next section I provide "thirteen theses" on progress in the new economic methodology. The number thirteen is based more on symmetry with my earlier paper than with any actual limit to the number of topics that might be discussed. The general argument is that there has been a tremendous outpouring of interesting and important literature that combines economics and science theory in some way. While many authors of this new literature consider themselves to be doing "economic methodology," many others do not -- they
would call their work philosophy of economics, or philosophy of science, or the economics of science, or the social studies of economic science, or a variety of other things -- but regardless of what they choose to call it, it is now possible to talk about progress within this newly expanded field of methodological inquiry. Of course this list, like any such list of progressive moves, will undoubtedly be controversial. There will be objections to certain items on the list as well as arguments for adding others. The list is simply my reading of the most important theses that have come out of the recent literature in the new economic methodology: that is, any work involving the intersection of economics and contemporary science theory.

The following list is not presented in any order of importance, but it does begin with subjects and areas of inquiry that are closest to the traditional rules-based methodological literature and then moves systematically into less traditional (and perhaps more controversial) areas. Finally, like my original Helsinki list, the thirteenth item is the least generally accepted; it is an assertion of my own that I am floating for more general approval.

II. Thirteen Theses

1. The economic methodology of John Stuart Mill (and the broader Millian tradition) is both more complex and more relevant to the actual practice of economic theorizing than previously believed. It now seems clear that Mill's a priorist approach to economic methodology is more than just a fifth-wheel (or a 6th book) loosely attached to his general empiricist epistemology, and that his analysis of concepts such as tendency laws, ceteris paribus, and inexactness continue to be relevant to methodological debates in economics and elsewhere. While Mill's program provided the intellectual backdrop for most of the methodological writings of the late 19th and early 20th centuries, it was abandoned mid-century with the rise of logical positivist and Popperian philosophy of science. The many tribulations of these two philosophical programs, as well as the fact that Mill's philosophy can simultaneously sustain both an empiricist epistemology and the practical disunity/plurality of science, has helped motivate a reexamination of the Millian methodological tradition.

3 All thirteen, and many more, are discussed in more detail in my forthcoming book Reflection Without Rules.
Some methodologists are also attracted to the Millian framework because it seems to be capable of providing a more effective methodological rationalization of the actual theoretical practice of mainstream economists than other philosophical approaches (particularly falsificationism). The author who is most responsible for the Millian revival is Daniel Hausman (1981, 1992, 1995, 1996, ...), but Nancy Cartwright (1989, 1994a, 1994b, 1995, 1999, ...) and many others have also contributed.

2. The Popperian tradition is much more complex than simply "falsificationism" and many different components of the Popperian philosophical tradition have been involved in, and are relevant to, debates within economic methodology. I mentioned "situational analysis" in my 1990 paper and what I said there still holds, but the issue is far more complex than simply trying to reconcile Popper's situational analysis view of social science with his falsificationist philosophy of natural science. There is also the issue of the relationship between economics and the more general "critical rationalist" reading of Popperian philosophy, the tensions introduced by the failure of Popper's version of verisimilitude, Popper's commitment to deductivist explanations, and a number of other concerns. The substantial differences between the Popperian falsificationism of Mark Blaug (1992, ...) or Terence Hutchison (1938, 1988, ...) and the Popperian-but-nonfalsificationist views of Klappholz and Agassi (1959) or Larry Boland (1982, 1997, ...) are certainly more clear now than they were a decade ago. A few of the works that emphasize these developments include Blaug (1994), Boland (1994), Caldwell (1991, 1994), Hands (1985, 1991a, 1991b, 1996), Latsis (1983), and Popper (1994).

3. Positivism has also come unstuck in time; it is now generally accepted that it is not (and never was) the monolithic and homogeneous philosophical position that it was packaged up to be when it was first imported into methodological debates in the human sciences during the interwar period. There were many different positions within the Vienna Circle and later logical empiricists modified the program still more. Many methodological debates within economics where both sides claimed the empiricist mantel - for instance certain debates between neoclassicals and American Institutionalists - become much more understandable once we recognize the diversity that exists within "positivist empiricism." The changing view of positivism also helps contextualize Samuelson's operationalism
and its relationship to various behaviorist moves within the field of psychology during the same period. It is also clear that (even early) positivism was not a philosophical position independent of (or prior to) particular theoretical commitments regarding political economy; Otto Neurath in particular was involved with the positivist movement in part (perhaps even primarily) because of the support it provided for his own particular version of political economy (see Cartwright, Cat, Fleck, and Uebel 1996; Cat, Cartwright, and Chang 1996; Uebel 1992 and others).

4. **Realism is also a many splendoured thing.** During the heyday of the Received View the discussion of "realism" within economic methodology simply mirrored the narrow two-sided debate over the "scientific realist" versus the "instrumentalist" interpretation of scientific theories within the logical empiricist philosophical literature. Not so in recent years. Those writing on economic methodology have cut the philosophical umbilical cord to the Received View and the many different faces of "realism" are now emerging within the methodological literature. As a result of this and other changes, "ontology" is no longer considered to be a dirty word among those writing on (even mainstream) economic methodology. "Realism" is now distinguished from "realisticness" (Mäki 1989, ...); the question of realism in economics is now perceived as a question that is essentially independent of the physics-based debates over realism in the philosophy of science (Hausman 1999, ... Mäki 1996, 1998b, 1999, ...); various brands of realism have been uncovered in the work of many figures and schools in the history of economic thought (Lawson 1994b, 1994d, ... Mäki 1990a, 1990b, 1997, 1998a, ... Pratten 1993, 1996, 1997, 1998 and others); economics is now being employed in the discussion of various versions of realism by philosophers of natural science (Cartwright 1989, 1999, ... Giere 1999); and the literature on "critical realism" (a philosophy of social science that draws on the anti-empiricist program of Roy Bhaskar) now represents an influential and rapidly growing perspective within the methodological literature (Fleetwood 1999; Lawson (1994a, 1994c, 1995, 1997a, 1997b, 1998, ... and others).

5. **Abstraction, Idealization and Modeling are important issues in economic methodology and the use of and/or appropriateness of those concepts also (like realism) has little to do with how the terms have been defined and debated within the philosophy of physical science.** Economists systematically employ
abstractions, idealizations, and models; and understanding the role of these
different concepts is extremely important to understanding the cognitive activity
of economic theorizing, but there is much more to it than borrowing what
philosophers of physics have said about these concepts and "applying" them to
economics. Some of the many contributions to this growing and diverse
literature include Balzer and Hamminga (1989), Hamminga and De Marchi

6. The current disarray within the philosophy of natural science has
undermined the previous "shelf of scientific philosophy" view of economic
methodology. The process of weaning economic methodologists from the shelf of
scientific philosophy -- the view that methodologists simply take ideas off the
shelf of scientific philosophy (what "they" say good science is) -- has been
ongoing for many years, but it has accelerated as a result of the current malaise
within the philosophy of natural science. The shelf no longer has anything on it,
or at least it doesn't have anything on it that has proved to be very reliable in the
area of application for which it was originally designed (natural science).
Economic methodology is cut off, and as a consequence those writing in the field
are forced to fend for themselves. Leaving the shelf behind certainly need not
imply abandoning the ideas and argumentation of disciplinary philosophy, but it
does mean giving up the shelf that seemed to be conveniently provided by mid-
twentieth century Received View philosophy of science (Hands 1994a).

7. The literature on the Rhetoric of Economics has continued to expand, both
in terms of the number of rhetorical studies and also in terms of its overall
breadth of focus. Rhetoric was also listed as the 7th thesis in my 1990 paper, but
at that point the literature consisted almost exclusively of first generation work
by Klamer, McCloskey, and those directly influenced by their work (Klamer,
McCloskey, and Solow 1988; Nelson, Megill, and McCloskey 1987; Samuels 1990;
McCloskey 1983, 1985; ...). In the intervening years that literature has continued
to grow (McCloskey 1994, 1996, ... and others) and it has now been joined by a
second generation of rhetorical literature that extends the original framework in
a variety of different ways (for example many of the papers in Favretti, Sandri
and Scanzieri 1999 and Garnett 1999). The literature on the rhetoric of economics
has also influenced, and been influenced by, the recent literature on the rhetoric
of the natural sciences (Gross and Keith 1997 and others).
8. While there is a lot of controversy within contemporary science theory, there are a few points of relative consensus -- anti-foundationalism, naturalism, and the social nature of science -- and these ideas have spilled over into economic methodology. In simplest terms anti-foundationalism is the abandonment of the idea that absolute foundations can be found for human knowledge; naturalism is the idea that one should employ the best current science in the general investigation of scientific knowledge; and the social nature of science is the point that science is fundamentally a collective endeavor engaged in by scientific communities and that consideration of its sociality matters to understanding scientific knowledge. These ideas have all filtered into the methodological debates within economics and taken root in a number of different ways. Gone is the empiricist-foundationalism that was once the generally accepted backdrop for all methodological discussion. It is now considered acceptable to start from various scientific positions - biology or cognitive psychology for instance - when thinking about "knowledge" in general and economic knowledge in particular. The economics profession, like all organizations of scientific practitioners, is a social organization and much (for some, all) of what is produced by this institution is a result of its sociality. References here run the gauntlet of recent methodological work.

9. The science studies literature (sociology of science, sociology of scientific knowledge, actor-network theory, social constructivism,...) has interacted with recent economic methodology in at least two important ways. First, many of these sociological approaches characterize the behavior of individual scientists and/ or the structure of scientific institutions in ways that owe much to economic theory. In some cases the relevant theory is rational choice theory and mainstream microeconomics, while in others it is a heterodox economic theory such as Marxism, but either way the underlying social theory for many of those writing within science studies amounts to some form of economics. See for example Hands (1994b, 1997a), Mäki (1992a, 1993), Mirowski and Sent (1999) and others. Second, many of these science studies approaches have been employed in the investigation of various topics within the history of economic thought. These historical studies have increasingly replaced the Popperian and Lakatosian "rational reconstructions" of the previous decade. Such sociologically-inspired histories are more diverse and less didactic than the earlier philosophy-based

10. The Economics of Science has recently come into its own as an independent and rapidly growing framework for the study of scientific knowledge and institutions. The economics of science often competes with various sociological approaches to science and scientific knowledge. Some of these economic models extend standard arguments about economic efficiency to the "cognitive efficiency" of various scientific activities and institutions, while others focus less on epistemology and restrict the economic analysis to the behavior of scientists (without appraising the cognitive value of their output). Some of the recent literature on the economics of science employs the same type of mathematical modeling and econometrics that one would expect to find in research produced in any other area of contemporary economics, while others employ economic ideas and argumentation in a more informal way. Of course if the economics of science is applied to the science of economics it raises issues like "reflexivity" that did not exist in the methodological literature based on the shelf of scientific philosophy. See Diamond (1996), Sent (1999), Stephan (1996), Wible 1998), Zamora Bonilla (1999), and others for a discussion of this recent literature and its methodological implications.

11. In addition to the sociologists of science (#9) and the economists (#10) who are applying economics to questions about scientific knowledge, there are also a number of philosophers of science who have recently begun to explicitly employ economic concepts in their investigation of scientific knowledge. Increasingly philosophers of science are using economic tools such as rational choice theory and game theory as a resource in the investigation of traditional philosophical questions about scientific knowledge. Often the argument is that, contrary to many of those in science studies, the claim that scientists act in their own rational professional or self-interest and do not obey any specific rules for the proper conduct of scientific activity, need not (following economic theory) undermine the cognitive value of the products produced by the scientific community. If self-interested individual behavior can (as if by an "invisible
hand") generate economic efficiency, then why couldn't the self-or-
professionally-interested behavior of scientists also (given the right institutions)
produce epistemic efficiency? One way to think about this project is to think of
economics - rather than biology or cognitive science - being employed as the
"naturalizing base" for a naturalistic inquiry into scientific knowledge (perhaps
even economics). Some examples of this philosophical literature include
Goldman and Cox (1996), Goldman and Shaked (1991), Goldman (1999, ...), and
(1997) for a few critical responses.

12. Related to, but separable from, point #8 above, the general tendency
among those writing in economic methodology -- like the tendency of those
writing in science theory more generally -- seems to be to find a comfortable
middle ground between relativism and foundationalism. It is clear that
traditional foundationalist-inspired philosophy of science is no longer acceptable
-- as I said, naturalism, anti-foundationalism, and the sociality of science are the
givens of contemporary science theory -- but at the same time very few are
willing to accept a radical relativist (or postmodernist) view of science as the only
alternative. Scientific knowledge is something different from mere opinion, and
it is more than opinions held by one specific (scientific) form of life, and yet it is
also not epistemically privileged in the way that (or to the degree that) it was
considered to be by foundationalist philosophy of science. This search, the search
for a middle ground between foundationalism and relativism, seems to be the
main task of contemporary science theory and also much of the new economic
methodology. As with #8 above the references here would need to include most
of recent science theory and its application to the study of economics.

13. Not only is the shelf of scientific philosophy gone, it never really existed.
The supposition of the "shelf" is that the metascience on the shelf is independent
of political economy. This is of course what gives the shelf its appeal; it is
epistemically above the economic fray; the items on the shelf are there on the
basis of either pure epistemological reasoning and/ or what actually happens in
the most successful natural sciences. In either case what appears on the
philosophical shelf is "out there," free of economic bias and contamination, and
thus a good independent mediator in debates regarding the scientific status of
various economic theories. Wrong. As the recent work on Neurath makes
exceeding clear (see references in #3 above) positivism was thoroughly intertwined with debates about political economy and the role of science in economic and social life. So too, for Popper, who had difficulty with his own falsificationism at least in part because in conflicted with the practice of legitimate social science. And then there is Logical Empiricism and the cold war, and of course pragmatism is intertwined with the Dewey's political economy, and on and on. The economics was always in the stuff on the shelf; it was never out there and independent; it was always in here and intertwined. How one views the stabilization of the epistemic order is (and always has been) deeply intertwined with how one views the stabilization of the economic order.

III. Conclusion

While I am little uncomfortable using the term "new economic methodology" ("the new ... " always sounds so presumptuous), I need a term that captures all of the many different projects discussed above -- all of the many different ways in which people are investigating the multiplicity of possible interactions between economics and contemporary science theory -- and yet is also a term that distinguishes this recent work from the more narrow rules-based and shelf-guided economic methodology of an earlier period. I welcome other suggestions, but for now I will simply call it the new economic methodology. While I am calling it "new," I would also like to note that it is not this recent turn, but rather rules-based methodology that is in fact the historical aberration. The broader view is certainly more consistent with what most of those writing about the method of mainstream economics were doing prior to the1930s (and heterodox methodologists throughout the century) than the narrow search for exclusionary rules borrowed from philosophy of natural science. Is it a coincidence that the rise of rules-based methodology coincided with the stabilization of the discipline around the neoclassical mainstream? I doubt it, but that is a subject for another time.

Hopefully I have persuaded the reader that a lot of interesting and important (and even progressive) work is being done in this expanded field of methodological inquiry. I want to emphasize that while I am personally quite pleased about these recent developments -- I have never been enthusiastic about narrow rules and most of my methodological writings have focused on the
failure of various attempts to find such (particularly falsificationist) rules -- I am not making an argument about what we "ought" or "should" do in the field. It is already done. The change has been made; the corner has been turned; I am just reporting. Given the failure of the search for simple rules and the wide range of new inquiries being undertaken along the unstable and overlapping boundary between economics and contemporary science theory, economic methodologists seem to have basically two choices. We can continue to define economic methodology as the shelf-based hunt for narrow methodological rules and be willing to accept that the field is therefore dead, or we can redefine economic methodology to encompass this broader and more progressive (and actually far more interesting) field of inquiry. I obviously vote for the latter.
References


Cartwright, Nancy; Cat, Jordi; Fleck, Lola; and Uebel, Thomas (1996), Between Science and Politics: The Philosophy of Otto Neurath. Cambridge: Cambridge University Press.


Favretti, Rema Rossini; Sandri, Giorgio; and Scanzieri, Roberto (eds.) (1999), Incommensurability and Translation: Kuhnian Perspectives on Scientific Communication and Theory Change. Cheltenham: Edward Elgar.


McCloskey, D. N. (1996), The Vices of Economists - The Virtues of the Bourgeoisie. Amsterdam: Amsterdam University Press.


