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THE TREND OF METHODOLOGICAL THINKING

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In this paper (*) I examine the past, the present, and the future of methodological work in economics. My thesis is that, despite appearances to the contrary, there exists a trend to methodological thinking these days.

My theme will be developed slowly. To organize my comments, I will undertake what is usually a pretty boring exercise, and that is to present a categorization scheme. A distinction will be drawn between economic *practice*, that is, what economists actually do, and the *methodological writings* of economists. The distinction is important because one frequently hears the complaint that methodologists have not paid enough attention to the practice of economists, that by doing things like focusing on the writings of philosophers we have been led seriously astray. The distinction will be used to defend the thesis that a discernable trend in methodological thinking exists today.

1. *The Practice of Economics*

There are many ways to describe the practice of economics. One could look at what economists do in their various roles as teachers, researchers, policy-advisors, journal editors, referees, text-book writers. One could study the mores, folkways and norms of the profession. One could examine the logic of economic arguments, the rationality of our institutions, or the

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ıetoric of our discourse. The particular vehicle employed below is derived om a crude, vaguely Kuhnian, kind of sociologism. What is important in hat follows is not the particular description of the practice of economics, hich in this case is woefully incomplete. The significant point is to disıguish sharply between the practice of economics and the practice of ethodology.

In describing the practice of economists, I will distinguish among ıree types of *research* practice. All will be labeled science, though I freely lmit that I am unable to define exactly what science is, though (like porɔgraphy) I know it when I see it.

First there is *grand science*. Grand science takes place when new odels and techniques are discovered. It is currently highly formalized; its :actitioners are located at the top research departments; it is work at the ontiers. It is risky business to try to do grand science, one never knows if ıe's work will be granted sustained prominence or simply be viewed as a ıssing novelty. We have no instant rationality about such exercises; identiing grand science is a retrospective activity. A plausible if obvious way of lentifying it is to look at the contributions of past and prospective Nobel rize winners. Interestingly, much of this work has been interdisciplinary, ith mathematics being the discipline of choice to be mixed with economs. If it is hard for us to recognize that this *is* interdisciplinary work, that nly goes to show how complete the formalistic revolution has become. ıdeed, if one were to ask the question of most economists, "Is it possible ɪr one to make a contribution to economic theory without using mathnatics?" they would have a hard time taking it seriously. Of course, this is ıst the sort of questions that methodologists love to ask.

The second category is *normal science*, and this category contains the ıst majority of the research done by economists. In normal science, the eld and subfield in which the work is done is pretty well established. The :chniques used are those learned in graduate school, which are perfected ıd extended in subsequent work, or picked up in seminars or from more :cently trained colleagues. Progress in normal science is often techniqueriven. For example, one might apply a new modeling or econometric :chnique that has been developed by others to an old problem. Among mpirical economists, the discovery and mining of a new data set, or the iscovery of a new proxy for an old variable, or even the simple addition of new significant variable to an already well established relationship, constiıtes an advance. It should be evident that normal science stands in a erivative relationship to grand science.

The third category is *alternative science*, and those who engage in it ıallenge some aspect of mainstream practice. Some practitioners of alterative science, like Austrians or Institutionalists or Post-Keynesians, are identified with a well-defined rival school. Others make their name by proposing an alternative for a particular construct: Leibenstein's (1966) x-efficiency theory or Simon's (1976) distinction between substantive and procedural rationality are examples of this phenomenon. If a number of followers of such leaders emerge, a kind of *normal alternative science* may develop. Journals are established, specific research problems are defined and addressed, and a few university departments may even become known as centers of alternative science activity. The leaders of such groups often do pretty well in the profession; they are the mavericks whose existence allow mainstream economists to feel good about their open-mindedness. Their followers usually sweat tenure. Occasionally, though, some of the leaders cross the bridge and make it to mainstream status. Two examples are Milton Friedman and James Buchanan.

Having described the practice of economics, we may now turn our attention towards the practice of methodology. Before moving to that task, however, we can ask: What do economists in the three groups above think about methodology? The answer to this question is revealing.

The mainstream *normal* scientist cares the least about methodology. To the extent that the term "methodology" even comes up in conversation, he interprets it as meaning method, or technique, as in, "What methodology did you use to correct for auto-correlated disturbances in your model?". And remember, normal science makes up the bulk of the research done by economists. If the past predicts the future, most economists won't be interested in methodology.

Practitioners of *grand* science, on the other hand, occasionally *are* interested in methodology, particularly when, as their reputations are established, they are asked their opinions concerning the best way to practice the science. This is why the methodological pronouncements of practitioners of grand science are invariably heavily prescriptive. Of course, some of them end up finding methodology rather distasteful and say nasty or condescending things about it. In any case, even when practitioners of grand science do write about methodology, they approach the field more as a hobby than anything else. They do not consider it serious scientific work.

Because they are outsiders who seek to change things, practitioners of *alternative* science usually are obsessed with methodological issues. This is another reason why so many mainstream economists steer clear of methodology.

There are other groups within the profession some of whose members are interested in methodology. Because they are constantly in pain, graduate students sometimes turn to methodology to try to convince themselves that their masochism has a purpose. Unless they become alternative scientists, they soon recognize that studying methodology makes their burden heavier rather than lighter. Another group is comprised of teachers of economics, those who work at smaller teaching colleges where the research demands are not so heavy. There are probably two reasons why some members of this groups become interested in methodology. The first is that, because they are not constantly engaged in doing research they actually have time to read, and to read in areas outside of their specialization. The second is that they are teachers, and good teachers usually reflect on the material they teach, and such reflection can lead one to an interest in methodology. Though I will not pursue the point, I will add in closing that economists in the various groups I have described often harbor the same beliefs about the history of economic thought as they do about methodology.

2. *The Old Methodology, When Positivism Was Regnant*

Having provided categories for describing the *practice* of economics, let us now turn towards an examination of *methodological writings*. We take as our subject the old methodology, McCloskey's (1985, pp. 24-26) methodology with a capital M, which might well be defined (though we will see, there are problems with this definition) as the philosophically-based appraisal of economic theories. Again, I will use three categories. As luck would have it, the three categories *mirror* the categories used to describe the *practice* of economics.

Thus, first we have *grand methodology*, which takes place when a practicing economist, usually a prominent one, lays out a few key methodological principles (or, to use the jargon, lays out a few criteria of theory appraisal), and then shows that following these principles leads one to accept a certain group of theoretical constructs and to reject its rivals. The most famous example is Friedman (1953), whose defense of predictive adequacy and simplicity provided a rationale both for accepting the competition-monopoly dichotomy and the quantity theory of money, and for rejecting the theory of monopolistic competition and Keynesian macro models. A second lesser-known example is contained in Hayek's 1933 book, *Monetary Theory and the Trade Cycle*. In that work he lays out three methodological principles which, if followed, would eliminate all alternative business cycle theories except for the Mises-Wicksell model he endorsed. It would seem that the trick of such efforts is to show the *objectivity* and *universality* of the initial methodological principles. Interestingly, in neither of the two examples just cited was there any attempt to do this. Nor have economists shown any concern about this state of affairs. Indeed, modified versions of both Friedman and Hayek's positions are widely accepted today. (Friedman's position was recently updated in Melvin Reder's (1982) brilliant description of the "tight prior"; Hayek's arguments have been cited by rational expectations theorists). It is no wonder that philosophers, in their more honest moments, find economics a very curious enterprise.

The second category for methodology, analogous to normal science, is the large *secondary literature* that has sprung up in response to the seminal works in methodology. To make a contribution here, one takes a particular position and criticizes it. There are many ways that such criticism may be undertaken. One may question the internal consistency of an argument, or the consistency between the professed methodology and the actual practice of the economist whose work is analyzed, or the compatibility of his position with the reigning philosophy of science. The literature here is large; indeed, writing a commentary on Friedman or Samuelson is almost a rite of passage for those interested in making a contribution in this area. Just as normal science is derived from grand science, this secondary literature in methodology is also derivative. The difference between them is that while normal science seeks to extend the work of grand science, this secondary literature is almost always *critical* of the grand methodological pronouncements it takes as its subject. This leads to some strange results. For example, one reason that Larry Boland's (1979) paper, "A Critique of Friedman's Critics", aroused such passion is that he reversed the usual procedure. When he declared that "*Every* critic of Friedman's essay has been wrong" (1979, p. 503), he was attacking the secondary literature, which was unprecedented. Many of his readers mistakenly transposed Boland's argument: they drew the faculty inference that a critique of Friedman's critics must also be a defense of Friedman. Actually, Boland's paper is one of the most subtle attacks on Friedman in the literature.

The final category is, of course, *alternative methodology*. One must be ambitious to practice alternative methodology. The first step is to provide a thoroughgoing critique of the mainstream. To do so, one must learn a lot about epistemology, because the point is to show that the standard approach to economics does not provide us with sufficient knowledge about some crucial aspect of phenomenal reality. But this implies that one actually knows what aspects of phenomenal reality are crucial, which gets one into metaphysics. But even this is not enough, because one still has to decide what to criticize. Is it the actual practice of mainstream economists or their methodological pronouncements which should serve as the target? (For the record, usually it is some mixture, with mathematical formalism being the target for practice and positivism being the target for methodology.) The next step is to provide an alternative vision of what constitutes economic reality and of the best procedures for investigating it. Given the immensity of these tasks, it is not surprising that alternative science practitioners are

usually associated with well-defined schools of thought: Austrian, Institutionalist, Marxist. It must be added that the work of those not associated with schools (two that come to mind are Nicholas Georgescu-Roegen (1971) and Kenneth Boulding (1978)) is often extremely hard to assess.

3. *The Death of Positivism Warrants a New Approach to Methodology*

Having completed our description of the old methodology, we may now focus on the current scene. The new methodology is perhaps best viewed as an attempt to come to grips with the implications of the death of positivism. It is essential to emphasize, however, that the demise of positivism has had its primary effect on the *methodological* literature. It has had almost no effect on the research *practice* of economists – neither normal science nor grand science has been much effected. Only practitioners of alternative science, like the Austrians or Post-Keynesians, have noticed that anything has changed. This is because it is only practitioners of alternative science who pay much attention to methodology.

Why hasn't the decline of positivism had any effect on the practice of economics? The answer is simple. Though one result of the positivist era was to cause economists to think of themselves as "real scientists", *their actual practice had little to do with positivism*. This fact is emerging over and over again in the new methodological literature. Two examples will illustrate this claim; they are easily multiplied. Neil de Marchi (1988) recently showed that the attempt by economists at the LSE in the early 1960's to apply Popper's prescriptions to economics never got off the ground. In analysing the use of the term "ad hoc" by economists, Wade Hands (1988) made an interesting discovery. Philosophers disdain changes in a theory which are designed solely to protect it from empirical falsification: such changes are what they mean by an "ad hoc" theory change. Economists, on the other hand, tend to describe as ad hoc any theory which is not grounded on rational, maximizing behavior. In other words, in order to do "scientific economics", one must begin from the (either tautological or empirically false, depending on its formulation) assumption that all agents are maximizers. Such a prescription finds no support in positivist philosophy of science. It is no wonder that the death of positivism has gone unnoticed by most economists: like other sciences, we never followed its prescriptions in our actual practices.

What effect has the collapse of positivism had on the writings of methodologists? To answer this, it is germane to point out that the goal of positivism was to provide various models of what constituted legitimate scientific practice. Equally important, positivism was not challenged and eclipsed by some alternative vision of what constitutes legitimate scientific practice, rather, it collapsed from within. To be sure, philosophers of science have proposed alternatives in the intervening years. But none of these has replaced positivism. In fact, a number of philosophers now believe that the question, "What is science?" when asked at such a high level of generality, is a pseudoquestion. They would rather ask, "How can we characterize the specific practice undertaken in specific sciences?".

The implications for the methodological literature should be obvious. Practitioners of grand methodology, those who would identify a handful of methodological criteria of correct scientific procedure, no longer have an outside authority, positivist philosophy of science, to point to in support of their choice of criteria. Practitioners of alternative science on the other hand no longer have to spend so much time beating up on positivism; their efforts can be directed at the more constructive task of building alternative paradigms. Methodology has entered, for the time being at least, a more pluralistic age.

That's the good news. (At least to me it's good news.) The bad news is that a pluralistic environment is very unsettling. One encounters a lot of competing claims, with no apparent means to assess them. Everyone agrees that we are better off without the Puritanical rigidity of positivism, but is a methodology of free love that much better? The question on the methodologist's agenda is: What lies beyond positivism? And since I mentioned the title of my (1982) book, I should point out that the title was intended to be both a declarative statement (as in, "We *are* beyond positivism.") and a question (as in, "So where do we go now?").

4. *Two Trends in the New Methodology*

I will now try to pull together my musings on what the new methodology looks like. My thesis, again, is that a discernable trend is emerging. The trend has both a negative and a positive heuristic; that is, there are some topics that most agree are not worth too much time and attention, and other topics that are.

The negative heuristic might be stated: *Methodologists should no longer seek for some ultimate set of criteria of theory choice.* I have made this point often, and will not repeat the arguments for it here. Because my thesis is that a commonality of views is emerging, I will mention how this thought has been expressed by other practitioners of the new methodology.

The first economist to make the point explicitly was Larry Boland, who stated in an article published in 1970 that, "methodology, in attempting to solve the choice problem, is pursuing an uninteresting (because unsolvable) problem". He made the point again in his (1985) review of Mark Blaug's methodology book, in which he argued that the chapters on various economic research programs were the weakest in an otherwise admirable book. Don McCloskey (1985) made the point when he castigated Methodology with a big M. Big M Methodology is nothing other than what I have called grand methodology, that is, the statement of a few methodological principles that are meant to define legitimate scientific practice. Philosopher Dan Hausman (1988) made the same point in his paper at the 1986 American Economic Association meetings, in which he argued that economists have for too long in their methodological writings been concerned almost exclusively with questions of theory choice, and that it is time that other questions be taken up.

Because the negative heuristic is so easily misunderstood, I will digress briefly in order to state what it *does not* imply.

First of all, the negative heuristic does not imply that we have nothing to learn from philosophers. We have learned a tremendous amount already from the philosophy of science. We should bear in mind, too, that it is the philosophers who are now insisting that we turn our attention towards the actual practice of scientists in the various disciplines. Our mistake in the past was to try to adopt within economics, in an uncritical fashion, various heavily prescriptive models of legitimate scientific practice, models which were based on an idealization of the practice of physicists. Philosophers are now specializing in the study of economics. We would be throwing the philosophical baby out with the positivist bathwater if we were to adopt as a working principle that we have nothing to learn from philosophers.

My second point about the negative heuristic is even more important. When advocates of the new methodology say that a concern with questions of theory choice should no longer be at the top of the methodologist's agenda, it does not mean that all standards disappear. *Criticism* is an essential part of the new agenda. I have made this point repeatedly in my own work (1982; 1986; 1988). Larry Boland has also emphasized the importance of criticism, with the significant qualification that criticism is best viewed as an aid to *understanding*, rather than as, say, a criterion of demarcation (1987). Don McCloskey (1985) makes the same point when he notes that the purpose of a literary criticism of an economic text is to make its arguments more *understandable*, to see how it uses the traditions of its genre, how it achieves its intent, what metaphors it uses and with what effect.

And this, by no accident, brings us to the positive heuristic: *The purpose of the new methodology is to help us to understand better what the practice of economics is all about.* The old methodology shed virtually no light on the actual *practice* of economists. As Don McCloskey (1983, p. 493) has put it, the actual rhetoric of economists, as opposed to their official rhetoric, is "honorable but unexamined". (I will add parenthetically that it has been unexamined, but that it may turn out to be something less than honorable once we do look at it!)

A few examples of this type of work will be provided below, examples that draw from three different traditions: rhetoric, sociology, and philosophy. You may ask why a tradition is necessary. The answer is simple. In order to understand economics, we must first describe it, and a description is always from a point of view. It must also be emphasized that these traditions are complementary rather than competitive. It is a shameful waste of resources to spend time debating which approach is best. What we should be doing is providing rhetorical, and sociological, and philosophical studies, which may *then* be criticized for how well they accomplish their given tasks. This point, of course, is simply the application of our negative heuristic at the meta-methodological level.

The rhetoric approach is exemplified in the work of Don McCloskey (1985 and citations therein). The primary focus of this work up until now has been the rhetorical analysis of texts, where "texts" is taken to mean major articles or books. McCloskey has also made the point that the old methodology, the "official rhetoric", doesn't shed much light on the ways that economists actually argue and persuade. Proposed work will highlight how economists change their rhetorical devices to suit the audiences before them (one argues differently in front of introductory students, say, as opposed to when one is in front of a dissertation committee), how economists read, and how economists use the narrative form. The emphasis throughout is to show economists that in our everyday practice we use rhetoric all the time.

A.W. Coats is the major spokesman for the sociology approach. He has performed a number of sociological studies on such topics as turn of the century British economic thought (1967), the role of economists in government (1981), and Anglo-American differences (1980). Most recently (1986) he has examined the work of Richard Whitley, a sociologist who has provided what may prove to be an exceptionally fruitful framework for analysing the sciences. It is interesting to ask why the sociological investigation of economics has not gone farther than it has. Is it because economists truly believe that nothing can be learned from the other social sciences? One question that a sociologist of the future should answer someday is whether the professionalization of economics is responsible for decreased novelty in the work of economists. Or, to put it more broadly and more bluntly: What have the trappings of science cost us?

Two examples of the philosophical approach can be cited. The first is Larry Boland's (1982) *The Foundations of Economic Method*, in which he argues that the same hidden agenda underlies both orthodox and avante-garde research approaches in economics. The second is a series of books by

philosopher Alexander Rosenberg. It is fascinating to trace Rosenberg's odyssey since the publication of *Microeconomic Laws* in 1976. He began by claiming that certain law-like statements exist in economics, so that economics is an empirical science. Later, he changed his mind and characterized economics as an exercise in applied mathematics (1983). In the interim he went to a study of sociobiology (1980), and ultimately to a study of biology proper (1985), presumably in an attempt to discover the mechanisms and structures that are foundational for a study of the social sciences. Now he is coming back to economics. This is at best a caricature of Rosenberg's journey, principally because there is no time to do it justice. But the point is that an extended philosophical analysis of economic science is an extremely difficult task, and that implies that there is still much work to be done.

These examples are intended to illustrate the thesis that in the current diversity in methodology there is a common theme: to uncover, and thereby better understand, the practice of mainstream economics. It is likely that this sort of exercise will be characteristic of mainstream methodological work in the future.

What sorts of obstacles might the mainstream methodologist of the future confront? Three will be listed here, but others surely will emerge as the discipline moves forward.

First, the attempt to describe actual practice will always be hindered by the theorist's dilemma that practice will always be richer than any attempt to describe it. The second obstacle, the identification of which we owe to Polanyi, is that all practice involves a tacit component which is by its nature not amenable to description. Finally, we must remember the insights of Kuhn, Feyerabend and Shackle: the world is full of unanticipated novelty. Even if after a number of years of hard work we are able to obtain a passable description of the practice of economics, what happens if economics changes? What if behavioral economists convince us that the foundational notion of maximizing behavior can and should be replaced; what if game theorists convince us that a theoretical analysis of institutional change is within our grasp? If this occurs, our carefully constructed descriptions of practice will rapidly become obsolete, or, to put it more kindly, will become "of historical interest".

If the goal of mainstream methodology is to describe the practice of economists, what will alternative methodology look like? The answer is that it will look the same as it does today. The purpose of alternative methodology is to attempt to *change*, rather than simply to *describe*, mainstream practice; its goal is prescriptive rather than descriptive. Two examples of alternative methodology are the efforts by Don Lavoie and Jack High (1986) to create a hermeneutical, or interpretive, economic science, and Uskali Mäki's (1988) attempt to create a new methodological approach, one which meets the strictures of the doctrine of scientific realism. Note that the new mainstream methodology, where the focus is on description, might also cause the practice of economics to change. But its influence will be much less direct. To borrow a metaphor: sometimes the writings of literary critics cause books to be written in a new way, but changing practice is not the goal of such criticism. We criticize to *understand*.

I will close with a personal note. The question I most want an answer to is this: How is it that economists were able to fool themselves for so long about what they were doing? In his new book, Phil Mirowski (forthcoming) has gone a long way towards answering this question: his basic claim is that economists suffer from physics envy. In future work, I hope to discover the role that methodological writings played in promoting that disease, and to find out why its opponents were for so long unsuccessful in fighting against it.

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Summary JEL 036

THE TREND OF METHODOLOGICAL THINKING

An analogy between the practice of economic science and the practice of economic methodology is drawn. Just as there exists grand science, normal science and alternative science, there exists grand methodology, a methodological secondary literature, and alternative methodology. The death of positivism warrants the examination of new topics within these three categories in methodology. In particular, the search for some ultimate criteria of theory choice has for now been abandoned, and closer attention is being paid to the actual practice of economics. The goal is an enhanced critical understanding of practice.

Riassunto JEL 036

LE TENDENZE DEL PENSIERO METODOLOGICO

In questo lavoro si stabilisce un'analogia fra la pratica della scienza economica e la pratica della metodologia economica; così come esistono la grande scienza, la scienza normale e la scienza alternativa, esistono pure una grande metodologia, una letteratura metodologica secondaria ed una metodologia alternativa. La morte del positivismo ha aperto la strada all'esame di nuovi argomenti all'interno di queste tre categorie del discorso metodologico. In particolare, è stata per il momento abbandonata la ricerca di criteri ultimi su cui fondare la scelta fra teorie; si presta invece una maggiore attenzione alla prassi effettiva degli economisti, al fine di accrescerne la comprensione critica.