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DOES METHODOLOGY MATTER? HOW SHOULD IT BE PRACTICED?*

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In the first section five criticisms of methodological work are presented and analyzed. It is argued that, though many of the objections contain elements of truth, the study of methodology is still a worthwhile activity. In the next section three alternative approaches to the practice of methodology are critically evaluated. In the conclusion the author discusses the future direction of his own work.

1. Does methodology matter? Five objections answered

At the 1989 History of Economics Society meetings in Richmond, Virginia there was a session entitled, »Should Methodology Matter to the Economist or to the Historian of Economics?» Some of the participants answered in the negative. As an observer I was disappointed in the session, not because the study of methodology was attacked, but because the attack was such an anemic one. The major worry seemed to be that many economists think that methodological study is a waste of time. One panelist even suggested that it would be alright to keep doing methodological investigations as long as we called them by another name so as not to offend our fellow economists.

Lest there be any doubt, it should be stated at the outset that, at least in the U.S., most economists are indifferent towards methodology, and many of the rest are openly hostile to it. Indeed, *explaining* these attitudes is a methodological topic worthy of further study. But their existence, at least for now, should be taken as a given.

In this first section I will survey some of the more common objections raised by economists against the study of methodology, then try to answer them. My goal is to show the fair-minded economist that methodological study need not necessarily be a misallocation of scarce intellectual resources.

Though others exist, five of the most commonly encountered objections to methodology are:

1. *One does not learn how to »do» economics by studying methodology, one learns it by doing economics.* A knowledge of methodology is neither a necessary nor a sufficient condition for becoming a good economist. Given that time is a scarce resource, students would better spend theirs mastering economic theory, econometrics, and the sundry applied fields rather than confusing themselves debating the imponderable questions of methodology.

2. *Methodologists pretentiously and arrogantly try to tell economists how to do economics.* Most methodological prescriptions are based on philosophical notions about how best to do science. But what do philosophers

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know about economics? The models philosophers develop are not formulated with economics in mind and, indeed, often have little contact with the issues which most concern economists.

Sometimes this argument reduces to a simple questioning of the relevance of studying philosophy. But it can also be encountered in a more sophisticated form, in which the arguments of various philosophers and historians of science (e.g., Kuhn, Feyerabend, Rorty) are used against »positivism» or Popperian thought or the very idea of methodology itself.

3. *Methodological debates between opposing research traditions in economics are sterile: they never reach any conclusive results.* This argument is the obverse of the one above. Methodology is now found deficient because it *fails* to provide grounds for deciding among competing claims. This position was probably first articulated by Schumpeter in reference to the *methodenstreit.*

4. *Only »fringe» groups in the profession are interested in methodology.* Mainstream economists busy themselves with the practice of economics. The only ones who care about methodology are critics of the mainstream, be they Marxists or Austrians or Institutionalists or Post Keynesians or what have you. Such groups care about methodology because they have an axe to grind. Real economists do economics while the malcontents talk about how to do it.

5. *I know what economics is, so methodology is superfluous.* This argument is doubtless the most fundamental one. If one thinks that it is not difficult to know »good economics» when one sees it, then the study of methodology must seem like an enormous waste of time.

The arguments above are presented in an exaggerated form, but they are not extreme caricatures. Each contains an element of truth, though each can also be answered.

1'. It is true that one does not learn how to do economics by studying methodology. If anything, the situation is the reverse: one must know economics thoroughly before one can hope to make a contribution to the methodological literature. Let us grant the obvious point that the best way to learn how to do economics is by studying economics. There are still two problems with this complaint against methodology.

a. For the argument to have much force, economics should be a monolithic, homogeneous discipline. If there is broad agreement about what the content of an economist's education should be, then methodology will appear to be an esoteric and unnecessary field of study. It becomes more interesting and relevant when there exists disagreement over what consitutes the proper content, scope and methods of economics. Economists differ, of course, in their perceptions concerning the homogeneity of the discipline. I suspect that one reason why many economists in the U.S. do not have much use for methodology is that they perceive the discipline to be a settled one. How that view got established is clearly a topic worthy of further investigation.

b. More fundamentally, the above argument misunderstands the *goal* of methodological study. The goal is not to teach one how to do economics. Rather, it is to help one to think more clearly about what it means to do economics, to understand better what the practice of economics, in all its diversity, is about. What sorts of questions do economists focus their attention on? Which do they ignore, or consider unimportant? What are the peculiarities of their approaches, and what accounts for them? What is the deep structure of economics? How do the practices of theorists and empirical economists differ, and how are they alike? How does economics compare to other sciences, both social and physical? These are the sorts of questions which methodologists ask.

I have elsewhere described in greater detail some suggestions for how to do methodological work, a meta-methodological program which I originally called methodological pluralism but which is probably better dubbed critical pluralism (e.g., 1982, 1984, 1988a, 1989). I will not repeat this account, except to make the following two points. First, critical pluralism broadens the definition of methodology to include the study of things like rhetoric, sociology, and history. Some may quibble with me over terminology, but I hope that all will agree that such fields are best viewed as complements rather than substitutes in the quest to understand economics better. Second, to see that studying methodology is *not* the same thing as studying economics, one can compare the travails of a methodologist to those of, say, an academic theologian or a sociologist of religion. The analogy is a simple one. One does not study theology or the sociology of religion to become more religious. One does it to understand religious phenomena better.

2'. The second argument, that methodologists pretentiously and arrogantly tell economists what to do, is probably best viewed as a backlash against a specific and concrete episode in the history of ideas. For the first half of the twentieth century, various narrowly empiricist and heavily prescriptivist visions of what constitutes science dominated the philosophy of science. These ideas, often in perverse and idiosyncratic forms, percolated down into the methodological literature in economics. Sometimes the borrowings were explicitly acknowledged by the economist (as in the case of Machlup), and sometimes the expropriation was an unconscious one (as in the case of Friedman). Sometimes the economist was a theorist trying to show how scientific economics is (as in the case of Samuelson), at other times the point was to show how economics could become more scientific by following the prescriptions of the philosophers (as in the cases of Hutchison or Blaug).

There are a number of reasons why methodology took this path. Most fundamentally, economics like many of the other social sciences was very eager to establish its scientific credentials. As Phil Mirowski (1990) recently showed, one way of accomplishing this goal was to adapt the models of energy physics to the dismal science. Mouthing the rhetoric of positivism was another. Don McCloskey (1985) is exactly right in his characterization of the result that this had on how economists communicate: we owe many of our stilted language games to the quest for scientific credibility. Our indignation may be tempered somewhat when it is recalled that the authors of the doctrines were men whose experiences with Fascism and totalitarianism were direct and personal. For these men the ability to demarcate science from dogma clearly and unambiguously was more than the solution to a technical problem of philosophy.

Most now agree that the positivist program in its many variants has failed, and that the interpretations of the works of philosophers like Popper and Lakatos which have made their way into economics have little to offer us. (These are dominant themes in Caldwell, 1982, as well as in much of the recent literature, e.g., Hands, 1985a, b; deMarchi, ed. 1988; Caldwell, 1988b). If this account is correct, then it is no longer appropriate to accuse methodology of being arrogant and pretentious. The appropriate question is: What direction should methodology take given the collapse of positivism? That question is taken up in the next section.

3'. It is true that many methodological debates are sterile, and that progress in the field seems always to come very slowly.

But there *has* been progress, especially in the last ten years or so. In his contribution to this symposium, Wade Hands lists thirteen theses which he claims would be supported by a majority of contemporary methodologists. (As an aside, I agree with all but the last of Hands' thirteen theses. I do not agree that the explanatory-predictive distinction provides the key for distinguishing microeconomics from macroeconomics. I think I know what he is getting at, though, and it may be that our disagreement is terminological rather than substantive.) More generally, there are now a number of scholars who are interested in methodology (broadly defined) not with the hope of attacking or defending a particular research program, but because they wish to better comprehend the discipline. There are many problems which remain to be solved, and many solutions which are in the process of being proposed. Methodology currently is an exciting and vibrant area in which to work.

As to the sterility of certain debates, surely it is a methodological topic of the first importance to discover *why* it is that communication between rival groups sometimes breaks down so badly. Is it because of incommensurable metaphysical, or epistemological, or ethical, or methodological frameworks? Can the true differences separating such groups be discovered? Are they capable of articulation? Is it even reasonable to have faith in rational discourse about such matters?

4'. Heterodox groups usually *are* more interested in methodology than are members of the »neoclassical orthodoxy» (which usually is left undefined), and for the reason stated: methodology provides another set of arguments for why the mainstream is wrong.

But methodology is not the exclusive province of the fringes of the profession. Mainstream economists make methodological decisions and arguments all the time, they just don't always recognize them as such.

When, say, a George Stigler or a Paul Samuelson dismisses a Marxist or Institutionalist critic as being unscientific, he is making a methodological claim. Less dramatically, decisions to employ specific modeling and testing strategies quite often are founded, at least in part, on methodological considerations. Methodology, like interpretation, is ubiquitous. To deny it is to engage in obscurantism.

5'. One might accept that methodology, though not something which helps one to become an economist, might help one to understand economics better. One might accept further that the practice of methodology need not commit one to giving arrogant advice or to engaging in sterile debates. One might even accept the ubiquity of methodological discourse. But even if all of these points are granted, one might still question the necessity of methodology. For one could still argue that all of the important questions are settled in everyone's mind except the philosopher's; that any economist worth his salt can tell good economics from bad economics. If this is so, why do we need to study methodology?

Perhaps the most effective way to rebut this argument is to request that the speaker provide a description of »good scientific practice.» The typical answer (which can be found in the first chapter of many undergraduate economics textbooks) might look like this: Science uses theory. Theories, because they are abstractions, are unrealistic. We decide which theory is best by looking at their predictions: the best theories make predictions which are widely confirmed. If the speaker is sociologically inclined, he might also make reference to the scientific community. If so, he would add that good science is what a community of specialists who are perceived as experts say it is.

Having set the trap, there are two ways to attack this characterization. One is to demonstrate that certain fields generally regarded as scientific do not meet the criteria outlined above. For example, one can show that biology, though excellent at explaining how random variation and selection lead to the evolution of species, is unable to predict the evolutionary path that any particular species will take. But it is more fun to show how areas of inquiry not generally considered to be scientific can be fit into the model of »good scientific practice.» Consider, then, the similarities between economics and astrology.

Both of these disciplines make predictions. Many of the predictions made by each are unfalsifiable. Of those which are falsifiable, many have been falsified. Indeed, it is possible to define the term »prediction» in such a way that their predictive records are roughly comparable.

Both base their predictions on a theory. In both cases the theory is »unrealistic,» something that critics of each frequently point out. In both cases the theory is sufficiently complex that it takes experts in the field to understand it.

In both cases just such a community of specialists has formed. These experts are usually willing to explain the theory to the uninitiated for a fee. In the non-specialist population, each group has its critics. But there is also a fairly substantial portion of the non-specialist population which »believes in» the prognostications of each group. It might even plausibly be asserted that astrology has a larger and more faithful following than does economics. Ron and Nancy Reagan are a nice example: he doesn't believe in the predictions of economists, whereas she does believe in those of astrologers.

What conclusions do we draw from this? Is there no difference between economics and astrology? Are we trying to draw the distinction in the wrong way? Is there no such a thing as science? If there is, how do we demarcate it? Or is demarcation less important than other questions? These are the sorts of questions with which methodology grapples. It is understandable that some economists choose not to spend their time investigating them. But it is wrong to think that all the questions have been answered. There is still plenty of work to be done.

2. How should methodology be practiced? Philosophy, non-philosophy, and anti-philosophy

Positivism once provided the foundations for methodological work in economics. But positivism has been in eclipse within the philosophy of science for decades. Does positivism have a successor? Among those who answer yes, the heir presumptive appears to be some variant of realism. Among those who answer no, many would simply replace the philosophy of science with some other discipline to understand better the nature of scientific inquiry. But there is also a more radical group which seeks to extinguish epistemological discourse. Each position has implications for how to pursue methodological work in economics.

1. There are many varieties of realism: common-sense and scientific realisms; metaphysical, ontological, and semantic realisms; normative and descriptive realisms (Mäki, 1989a). If one were to describe a »representative realist,» the (in many ways unrealistic!) representation of his most basic beliefs might look as follows. The world exists; it is structured, multi-layered and complex; the properties and mechanisms which govern the structures are in principle discoverable; and it is the goal of science to try to discover them. Scientific theories refer to really existing entities, properties, mechanisms and structures; theories attempt to represent them; theories may be judged as true or false according to how well they correspond, in some sense, to what exists in the world.

Realists provide enriched and at times novel analyses of the nature of categories like necessity, causal power, essence, and universals. There are alternative models of the nature of scientific explanation, of the interpretation of theories, of the nature of truth. Most realist philosophers of science have concentrated on the physical sciences: that much within philosophy has *not* changed. The best general introduction for social scientists is probably Christopher Lloyd's (1986) *Explanation in Social History*. Lloyd compares realism with other approaches within philosophy, then shows how it is able to resolve or avoid many of the problems which proved so lethal for its positivist predecessors. He then argues for a realist (he calls it a »structurist») approach to the questions of human action and institutional change within the social sciences. The book combines a superb critical survey with a finely-crafted substantive proposal for change, and carries a complete bibliography. He is not kind towards what he dubs »economistic» explanations. But then, what social scientist who is not an economist is?

Realism will be difficult to apply in economics for two reasons. The first is that it will take a massive effort to introduce the large, complicated, and unfamiliar realist literature to economists. Realism is not like Popperian thought: it is not easily accessible, it does not provide simple formulas for demarcation, it does not quickly translate into a set of methodological rules. Luckily for the profession the work has begun. In a series of papers, Uskali Mäki (1989a, b, 1990a, b) has introduced a number of realist categories of analysis into economics. In my opinion, this is some of the most exciting research currently underway in methodology. It is potentially far-reaching in its implications. Even more encouraging, others are now beginning to provide realist analyses (e.g., Lawson, 1989, 1990).

The second obstacle is more troublesome. A potent and long-lingering variant of instrumentalism is well-entrenched in economics. Part of its influence is due to Friedman's famous essay, part is due to the types of theories which dominate standard neoclassical analysis, and part is due to the nature of some empirical work within economics. In my opinion, it is the last which creates the most problems for realism: much valuable empirical work in economics is not and cannot be concerned with the search for causal mechanisms. More generally, to the extent that the instrumentalism in economics is well-founded, a problem exists for realism because the two doctrines are usually viewed as incompatible with one another. If one is to make the case for realism, this tension will have to be overcome.

I think that the tension can be overcome, and have suggested one possible line of attack in which the central theses of economics are given a realist interpretation, while the models are given an instrumentalist one (Caldwell, 1990). Mäki (1989a) has taken another route by coining the awkward but precise terms »realisticness» and »unrealisticness,» which are predicates attributable to representations. Economists can still be realists, even though they employ representations which are in various ways »unrealistic.» Recently Mäki (1989b, 1990b) has expanded on the ways in which a representation might be unrealistic: e.g., besides hopeless falsity, there is simplification, exaggeration, isolation, approximation, and understatement.

Having said all this, the crucial tasks facing those who would reconstruct economics along realist lines are to establish just what kinds of economic entities, structures, and processes are said to exist, to explain convincingly how our unrealistic theories represent them, and to show how the empirical studies which so many economists undertake can be made consistent with realist prescriptions about the search for causal mechanisms. The success or failure of these endeavors will determine the success or failure of the realist programme within the dismal science.

2. Disciplines other than philosophy are available to those who would understand better the practice of economics. Perhaps the most obvious of these is the sociology of science. It is difficult to understand why so little work has been done in this area. There are, of course, the oft-repeated departmental rankings, as well as the occasional humorous depiction of the norms of the profession (e.g., Leijonhufvud, 1973). Of a more serious nature are the attitudinal surveys which have recently been undertaken (e.g., Frey, Pommerehne, et al., 1984; Colander and Klamer, 1987). There have been exhortations to do more sociology of economics, and frameworks for such study have been recommended (Coats, 1985, 1986). But much awaits the sociologist of economics.

There has been more activity in what might be called the hermeneutic or interpretive approach to economics. The most prominent contribution is the path-breaking work of Donald McCloskey (1985) on the rhetoric of economic argumentation. Another important advance are the oral history projects executed by Arjo Klamer (1983, 1989) in which leading economists are interviewed on matters both disciplinary and personal. A third development is the nascent attempt by a few economists at George Mason University to provide hermeneutic foundations for Austrian economics (e.g., Lavoie, 1989).

Are such approaches best viewed as substitutes for or complements to more traditional philosophical investigations? To be sure, there exists a long-standing feud between philosophers and hermeneuticians over which mode of understanding should be granted primacy. Economic methodology would do best, I think, to avoid such internecine conflicts. There is enough work to be done within each of the traditions without spending time debating which one is the most insightful. Fortunately, this conciliatory view has other advocates in the literature (e.g., Lloyd, 1986, p. 103 contains numerous references; see also Mäki, 1988), and with luck it will become dominant.

3. There is, however, another view, one which denies that the traditional philosophical (and particularly epistemological) approaches have anything of value to offer to those who seek a better understanding of the scientific enterprise. This view has entered the methodological literature, taking the form of an argument against methodology itself.

Elements of the argument can be found in the early chapters of McCloskey (1985), where the influences of Richard Rorty and Wayne Booth are evident. It appears that there is a strong and weak version of the McCloskey position. The strong version disparages capital-M Methodology as overbearing and attacks the pursuit of capital-T Truth as chimerical. The weak version is the pluralistic plea to pay more attention to the nuances of language and to the workings of language communities. It is the strong version which many methodologists find objectionable.

McCloskey's sometime antipathy towards methodology pales in comparison to recent work by Roy Weintraub (1989), for whom there is no weak version. For a long time Imre Lakatos was Weintraub's philosopher of choice, and given his interest in general equilibrium theory, the choice is peculiar but not completely inexplicable: Lakatos had penned *Proofs and Refutations,* after all. Weintraub has now fallen under the influence of Stanley Fish, who took over the English department at Duke a few years ago. Fish's theory of the interpretation of »texts» can be applied to any discipline. Fish is an impressive debater, an irrepressible self-promoter, and most important, an honest scholar. He notes that his own position, if followed to its logical conclusion, will forever change the way one perceives one's discipline. But he also honestly admits that it is a position which has no consequences for the discipline itself. I suspect that the latter point is Fish's way of acknowledging that his program is essentially a skeptical one. In any case, it seems to me that Weintraub's position leads directly to skepticism.

Some may respond: So what? What is so bad about skepticism? And it must be admitted that skepticism is not altogether unattractive. It is very appealing to the iconoclast, it is notoriously difficult to refute, and it can even be made to appear modest. But it also undercuts the bases of argument. Indeed, if the skeptic is right, then his own argument makes no sense. The consistent skeptic says nothing, or else admits that his discourse is only meant to entertain.

Let us leave Weintraub's skepticism aside, then, and return to the more reasonable position of McCloskey. McCloskey must have been shocked to encounter such strong resistance from methodologists over the claims he made in the early chapters of his book. It surely must have irritated him to have so much attention paid to the early chapters when the true meat of the rhetoric approach was found in the case studies which came afterwards. Furthermore, McCloskey had singlehandedly advanced the cause of »methodology» (again, broadly defined) by getting the attention of the larger economic audience, an audience other methodologists had found elusive. So why were those with a prior interest in methodology so unappreciative?

As one of his critics, I will admit that I felt ambivalent about attacking him. The rhetoric approach is clearly an important and viable contribution, one fully capable of sustaining its own research program. But the opposition to it from methodologists like me was inevitable, given the anti-epistemological foundations which were chosen for its grounding. The point is a simple one. One need not deny the goals of philosophy to advance the cause of rhetoric; one can take a non-philosophical approach without adhering to an anti-philosophical one. Methodologists, rhetoricians, and sociologists would do better to learn from one another rather than to fight over turf.

3. *Concluding comments*

When he asked me to prepare a paper for this symposium on methodology, Uskali Mäki requested that I conclude with a few words on my future research agenda. I will immodestly comply with his request.

I want to do a full-length study of the development of Friedrich Hayek's methodological thought. Hayek is an interesting figure for a number of reasons. He lived a long life and wrote prolifically, so there is plenty of data to analyze. More will be added soon as the materials in the Hayek Archives at the Hoover Institution in Stanford, California become available. The archival work is being undertaken by the philosopher Bill Bartley, who will serve as the biographer for both Hayek and Karl Popper. Hayek exhibits in all of his writings a (typically Austrian) fascination for methodological argumentation. His ideas on methodology appear to have changed through time, and it will be intriguing to piece together the causes and consequences of their evolution. Hayek knew most of the other major players on the methodological stage. He attended together with the likes of Fritz Machlup and Oskar Morgenstern the Mises seminar in Vienna in the 1920's. Lionel Robbins brought him to the LSE in the early 1930's, and he performed the same service for Popper some dozen years later. While at the LSE he engaged in debates with Keynes on monetary theory, with Knight on capital theory, and with the market socialists on the concept of state planning. He met Milton Friedman when the latter attended meetings of the Mount Pelerin Society, which Hayek founded. The list can be extended. Hayek's fortunes waxed, waned, and waxed anew over his long career. Once viewed as Keynes' principle competitor, Hayek lost influence in the later 1930's, and was castigated for the polemical tone of his 1944 book, *The Road to Serfdom.* Things got worse in the next two decades when he was not criticized but simply ignored. (There is no worse fate for a purveyor of ideas.) All this changed with the awarding of the Nobel Prize in 1974. About the same time, a revival of interest in Austrian ideas began which continues to this day. And Hayek continues to provoke, as is clear by the response to his latest offering, *The Fatal Conceit.*

By tracing the evolution of the thought of this singular individual and individualist, I hope to obtain a window on the world of twentieth century economics, and in particular of the role played by methodological argumentation within that world.

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