

METHODOLOGY OF ECONOMICS: COMPLAINTS AND GUIDELINES*

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Now that we have been persuaded to join those who think that methodological work in general is legitimate and even useful (Caldwell 1990) and also that such work has recently indicated progress (Hands 1990), it is time for a few complaints about the situation in economic methodology. In the process, I hope to clarify some of my own reasons for thinking that methodological research is and has been both useful and progressive. (For other fresh accounts of the state of economic methodology, see Caldwell 1989, Hausman 1989, Salanti 1989.)

In a sense, there should be no pressing need to demonstrate the legitimacy of economic methodology as a field of specialization. Namely, it would seem to me that such legitimacy has already been established beyond any reasonable doubt. There now exists an international community of scholars devoting themselves to fundamental questions related to the character of economics as a scientific discipline; the group is growing in numbers and maintains a network of communication; conferences, symposia, and sessions in larger conferences specializing in methodological topics of economics are organized; outlets for publication such as *Economics and Philosophy* and *Research in the History of*

Economic Thought and Methodology have been founded; and most important of all, research in economic methodology is being done in increasing quantity and with improving quality, illuminating perennial issues that have bothered the economics profession. Anybody familiar with all this should have no doubts about the legitimacy of this subfield of study. Thus, the task may be that of informing those who remain unfamiliar with this state of affairs.

Still, there is no reason for exaggerated satisfaction within the subfield of economic methodology. There is a lot to be hoped for, and nothing is more natural for methodologists than critical self-reflection.

2. Four questions

There are four questions to which the methodology or metatheory of economics has to seek answers.

- A. What is economics not like?
(The negative descriptive question)
- B. What is economics like?
(The positive descriptive question)
- C. What is rational economics like?
(The rationality question)
- D. What should economics be like?
(The prescriptive question)

By 'economics' I mean the structure and dynamics of the complex consisting of economic concepts, theories, models, and tech-

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niques, as well as of economists, their actions, interactions, and the aims and beliefs involved therein. Questions A and B concern this complex as it actually is or is not. Question C is about economics as it ideally is. Question D is close to C, but not identical with it. An answer to D tells economists to pursue the ideal that is set by an answer to C. Answers to C may be retrospective, while answers to D are prospective. Question A can be answered in a piecemeal way; every correct statement about qualities that economics does not have can be added to a gradually accumulating storage that increasingly limits the set of statements that can truthfully be claimed as answers to question B. A correct partial answer to A may be a correct answer to C.

The purpose of the brief notes to follow is to express my opinions regarding the extent to which these four questions have been adequately answered or left unanswered by those interested in the character of economic science, and also to give some hints as to how we should proceed in searching for adequate answers.

3. Knowledge of economics

It has been and will be maintained by some practicing economists that methodology as a field of specialization is not needed for answering questions A to D, since economists allegedly already know what economics is like (and, by implication, what it is not like) and what it should be like, if there is anything to be hoped for in the prevailing situation. (This position is one of the topics of Bruce Caldwell's paper for this symposium.) In a sense economists holding this view are right, in another sense they are wrong. To see this, let us make a distinction between practical and discursive knowledge.

Good practicing economists have *practical knowledge* of economics in the sense that they know *how to do it*; they have the required know-how concerning the way success in research will be achieved. However, practicing economists typically do not have adequate *discursive knowledge* of economics in that they usually are unable to provide a systematically articulated theory of the *what and why* of economics: what it is like and why it is the way it is. It is here that a specialist on methodology

is needed. It is the task of the methodologist to provide the conceptual tools that help go beyond the mere practical know-how to a theoretical understanding of the foundations of the discipline.

Often when practicing economists try to give a discursive account of their discipline, they commit grave errors. The case of Paul Samuelson is certainly one of the most illuminating examples of this. His phenomenalist and operationist claims about what economics is and should be like cannot but cause amusement nowadays. It is also possible that many present-day economists are not much different from those biochemists who, when asked, reported themselves to be true advocates of Popperian falsificationism, even though their own research practices had little if anything to do with the Popperian rules of how to do science (Mulkay and Gilbert 1981).

The very notion of economists' views of what they are doing qua economists needs to be further analyzed. There are different levels or contexts in which such views appear. *First*, economists may have more or less firm *beliefs* of what their discipline is like, irrespective of whether they are explicitly articulated or not, whether or to whom they report about such beliefs. *Second*, in their rhetorical discourse directed to their colleagues working in the same field and published in their specialised books and articles, economists may make claims about what they (think they) are doing as practitioners of their discipline. *Third*, economists may talk to other economists about what it is that they are doing, off the record so to speak. That is, such communication takes place informally, either privately (such as over a cup of tea with a colleague) or publicly (such as in seminars or conference sessions). *Fourth*, economists may direct their metatheoretical statements to wider audiences (such as their students, other social scientists, or those responsible for the funding of economic research).

It can be seen that the distance between these levels and actual research practice varies. It is also easy to imagine that there may be incongruities between the views related to different levels or contexts. Such incongruities are one expression of flaws in attempts by practitioners at discursive knowledge of the nature of economic science. Specialists in methodology have the task of formulating a

theoretical account of economics exempt of such flaws. It should be recognized that the pursuit of such an account requires adding a *fifth* level to the list above: the way economics in fact is practiced and structured, irrespective of the ways economists say or believe it is.

4. *The Popperian dominance*

I maintain that the mainstream of recent research and discussion on the methodology of economics has been largely dominated by a restricted set of themes, categories, problems, and questions. I will call this situation the *Popperian dominance*. I do not argue that the Popperian view of good science has dominated the scene, i.e., that most methodologists of economics have been advocates of Karl Popper's ideas. In fact committed Popperians seem to have been in the minority most of the time. My point is that *epistemological questions related to rational theory choice or rational theory development, formulated in the dynamic but anti-inductivist and asocial framework of Karl Popper or Imre Lakatos, have dominated the field*. In other words, a limited slice of the whole of Popperian philosophy has dominated the major themes of interest in economic methodology. Of course, the dominance has not been absolute, but there seems to have been a dominance nonetheless.

Recent works that most clearly indicate the Popperian dominance include Mark Blaug's *The Methodology of Economics* (1980), Terence Hutchison's *The Politics and Philosophy of Economics* (1981), Lawrence Boland's *The Foundations of Economic Method* (1982), Johannes Klant's *The Rules of the Game* (1984), Roy Weintraub's *General Equilibrium Analysis* (1985), and others that are based on Popperian or Lakatosian commitments, possibly with modifications. The Popperian dominance is also manifest in the analytical and critical, uncommitted work on the applicability of Popper's or Lakatos's ideas to economics, some of which have been gathered in such anthologies as *Method and Appraisal in Economics* (1976), edited by Spiro Latsis, and *The Popperian Legacy in Economics*, edited by Neil de Marchi (1988). Much of the excellent work by Bruce Caldwell and Wade Hands be-

longs to this category (e.g., Caldwell 1982, 1984, 1988; Hands 1985a, 1985b, 1988).

There may be good reasons for the Popperian dominance in the methodology of economics. One of them is the popularity of Popperian ideas among practicing economists themselves, or at least in the introductory chapters of economics textbooks. An analysis and criticism of the methodological views held by practitioners is, of course, a most natural devotion for methodologists.

As an implication of the suggestion made above, a qualification is needed here. To the extent that economists hold Popperian *beliefs* about what it is that they in fact do or should be doing, those beliefs can be supposed to have a role in making economists behave the way they do, even though those beliefs were utterly wrong. If they do have such a role, it should of course be carefully studied. Mere Popperian rhetorical discourse directed to outsiders (views at level four above) is not what I have in mind here, even though this is perhaps the most obvious place where Popperian ideas appear; genuine Popperian beliefs are required. The problem with all this, of course, is that we do not have any systematic studies which would show whether economists have such beliefs and what their role might be.

In any case, I will argue that, as a consequence of the Popperian dominance, some questions concerning the nature of economics have been answered in a flawed way, while many other important problems have not even been posed. Naturally, then, metatheoretical concepts needed for dealing adequately with such problems have not been developed. It is to be admitted that methodologists of economics working under the Popperian dominance have contributed to answering question A. Indeed, we are now in a better position to tell what economics is not: for instance, it is not a process of bold conjectures followed by severe tests and refutations. Unfortunately, good answers to the remaining three questions have not been provided at an equal pace; our understanding of what economics actually or rationally is or should be has not grown to the same extent. As a consequence of the Popperian dominance, we now do have a better understanding of what economics is not like; progress in this regard has occurred. In addition, we also have wishful prescriptions — attempted answers to

question D based on Popperian norms — with no foundation in an adequate answer to the positive descriptive question.

5. *Errors and omissions*

I argue that the Popperian dominance has directed the attention of economic methodologists to an unnecessarily limited set of topics and problematic positions. My complaints are of two kinds. First, some positions adopted under the Popperian dominance seem to be simply mistaken. Second, it can be argued that, relative to a number of highly important foundational problems, the Popperian questions have received an amount of attention that is out of proportion in respect to their relevance, i.e., that there has been a kind of misallocation of intellectual resources in economic methodology.

As an example of what I regard as a mistake, let us take Lawrence Boland's celebrated account of Milton Friedman's alleged instrumentalism and of the alleged conventionalism involved in neoclassical economics in general, published first in the *Journal of Economic Literature* in 1979 and then in his 1982 book. Among the Popperian presuppositions of Boland's account are logicism (the conceptual tools used are those of simple quantificational logic) and anti-inductivism (the problem of inductive generalization is regarded as insoluble). Boland argues that since general statements in neoclassical theories, the so-called assumptions included, cannot be inductively proved as true by deriving them from statements of singular facts, neoclassical economists lose their interest in truth and adopt conventionalist and instrumentalist views instead. Conventionalist and instrumentalist conceptions, ignoring considerations of the truth of economic theory, are on this account merely ways to circumvent the problem of induction. (Boland 1979, 1982.)

My own opinion is that the impossibility of proving general statements inductively as true has nothing to do with neoclassical economists' possible lack of interest in the issue of the truth of assumptions. Instead, the reason seems to be the simple fact that those economists are rather sure, mainly on the basis of their everyday experience of human be-

haviour and the workings of the economy, that their assumptions are not and cannot be quite true. Friedman's obscure methodological argument is just a way of making economists feel comfortable with this situation.¹

In passing, I should say that in light of this and other such disagreements, it is extremely important to have the qualification »some form» in front of »instrumentalism» in thesis 6 of Wade Hands's (1990) list. Note, however, that it can be argued that it is possible to interpret passages of Friedman's essay also in consistently *realist*, that is, non-instrumentalist, terms (Mäki 1990a). The problem with the essay is that it does not provide a single, internally coherent methodological view, i.e., there is no such thing as »Friedman's methodology» that can be read in his 1953 essay (see also Mäki 1986). Therefore, I am a little bit puzzled by the possibility that the formulation of Hands's thesis 6 is intended to imply the idea that we should pursue (merely) coherent interpretations of Friedman's essay.

In general, Popperian methodology seems unable to provide sufficiently sophisticated tools for dealing with the traditional issue related to what I have suggested (in Mäki 1989) should be called the realisticness (rather than »realism») of assumptions in economics. To put it in terms that were popular among economists debating this issue in the 1950's and 60's, Popper and Lakatos share the view that theories are to be tested by their observational implications. This may partly explain why very little interesting work has been done on the topic of the realisticness of the »assumptions» of economic theories in recent years by methodologists. This is a serious point, since the issue related to the quality and function of assumptions is one that seems to interest practicing economists as well as others perhaps more than any other methodological issue. New and more powerful frameworks are needed for progress to occur here. The popularized Popperian idea of scientific theory as consisting of simple universally quantified statements gives a distorted picture of economic theories; these theories typically involve

¹ I first expressed this criticism to Boland himself in a panel discussion at George Mason University in May 1985. At that time he did not accept it. (See Caldwell 1987, pp. 224–226, and Mäki 1987.)

a complicated system of existential and modal statements, as well as of isolations, simplifications, and idealizations, the accuracy of which in regard to economic reality cannot easily be experimentally controlled. Neither has the slightly richer Lakatosian notion of theory as a conjunction of hard core and protective belt statements proved helpful in understanding these features of economic theories. The construction of new metatheoretical frameworks has to acknowledge the peculiarities characteristic of economics. Such frameworks might help save economists from uncritically adhering to simplistic versions of Friedman's argument. (For my own attempts to construe a framework for analyzing the realisticness issue, see, e.g., Mäki 1989, 1990a, 1990d.)

It is easy to point out other omissions in recent economic methodology due to the Popperian dominance. One of them is that once induction is denied logical legitimacy, the role and structure of actually practiced inductive reasoning in economics have been, if not completely neglected, at least examined insufficiently. More generally, it can be argued that Popperian falsificationism is unable to deal with the problem of testability in economics — not only the problem of verifiability and confirmability of theories, but also of their falsifiability (Hausman 1988).

Another omission is perhaps partly an implication of the Popperian definition of »metaphysics» as the unfalsifiable residual related to scientific theories. The complaint here is that the falsificationist framework provides no tools for analyzing the character of the presuppositions poured into the epistemologically homogenous category of »metaphysics». Metaphysical issues proper, i.e., issues belonging to metaphysics in the classical, non-Popperian sense, such as the nature and role of universals and essentialist notions in economics, individualism vs. holism and their alternatives, the structure of causal relations and the existence and character of economic laws (e.g., whether some sort of necessity is involved), folk psychology vs. materialist reductionism in conceptions of economic agents, and other such topics, have attracted insufficient attention. Take the relationship of these issues to the invisible-hand argument, probably the most important theoretical idea ever created in economics. There is no doubt that

we do *not* have a good understanding of what kind of individualism the invisible-hand argument presupposes, whether and what kind of causation and lawlikeness is involved in the notion, and whether it is indifferent with respect to the issue of folk psychology. (For an attempt at an analysis of the metaphysical structure of the Austrian theory of money, see Mäki 1990d.)

In general, I would say that the study of the metaphysical foundations of economics has suffered from the Popperian dominance. Without plenty of such study, adequate answers to question B (as well as to C and D) are not forthcoming.

A major omission that is related to the emphatic prescriptivism of Popperian methodology is its being concerned with questions C and D to the exclusion of question B. As a consequence, we know very little of what is really going on in economics. We have learned that economists do not obey Popperian prescriptions; indeed, this is to be characterized as genuine progress in economic methodology. However, we are still largely in the dark in regard to question B insofar as economists' behaviour is concerned. In particular, the potential contributions of the as yet non-existent sociology of economics could help us here (see, however, Earl 1983, Whitley 1983, Coats 1984, Mäki 1990c). Unfortunately, from the Popperian point of view, the sociology of science is concerned merely with the suspiciously irrational side of science. The toil and trouble of engaging in empirical and theoretical work on the realities of economics is therefore easily replaced by wishful prescriptions as to how to behave rationally as an economist.

6. *Reactions and alternatives*

Not all work on economic methodology has been suppressed by the Popperian dominance (and not all work inspired by the Popperian tradition has been equally omissive of some of the more relevant issues like those of metaphysics mentioned in the previous section; see, e.g., Latsis 1972, Boland 1982). A few brief remarks on explicitly non-Popperian work can be provided.

Those of us who declare that we have to pay more attention to what economics in fact

is like, i.e., to question B, are quite right. This has been a recurring theme of those who espouse the rhetoric of economics approach, a welcome initiative in metatheoretical studies (McCloskey 1983, 1985, 1988; Klamer 1984, 1988; Klamer, McCloskey, and Solow 1988). Three suggestions related to these endeavours should be kept in mind, though. First, even though we have been provided with several insights into the rhetorical realities of economics, we still lack a systematic theory of economic rhetoric. A well-formulated theoretical framework should also be pursued. (See Mäki 1988.) Second, the set of perspectives and approaches to the realities of economics should be understood widely enough. Mere rhetoric of communication is not sufficient. For instance, the metaphysical foundations and social conditions of the discipline have also to be examined systematically. Third, the descriptive question has to be understood so as not to exclude the normative dimension altogether, against a fashionable current.

Indeed, there seems to be something like a crisis of normative methodology in the field, implying a suspicious attitude towards questions C and D. I would like to suggest that the crisis is, for the most part, the crisis of the Popperian dominance. One manifestation of this is the reaction of McCloskey and Klamer, which has tended to take the form of a denial of methodology. Among the premises of this position seem to be, first, the equation of methodology with Popperian methodology and, second, the equation of methodology with prescriptive methodology. Neither of these equations makes much sense. In an important sense, however, they are unconscious reflections of the Popperian dominance.

It is my opinion that questions C and D should not be abandoned completely, as against this recent trend in economic methodology. Instead, methodologists should approach them armed with newly acquired information about the realities of economics. In other words, normative considerations should be based, among other things, on foundations consisting of detailed descriptive and explanatory knowledge of the past and present of economics, its achievements and failures, its theories and research practices, and their metaphysical foundations and social conditions, as well as on knowledge of the capacities of the human mind in various so-

cial settings with various objectives.

As representative of the kind of research that is needed for establishing the necessary foundations, I would mention Daniel Hausman's study on the explanatory import of alternative theories of capital (Hausman 1981) and Bert Hamminga's work on the pragmatics of the structure and evolution of the neoclassical theory of international trade (Hamminga 1983). Both of these studies outline a metatheoretical framework in close contact to a special case in economics. Furthermore, Philip Mirowski's investigations into the physical analogies implicit in economic theorising may help elucidate some details in the metaphysical foundations of economic theories and research practices (Mirowski 1989). There is something to be learnt also from his study by Abraham Hirsch and Neil de Marchi on the methodology of Milton Friedman as a practicing economist (Hirsch and de Marchi 1990). Even though I welcome the recent introduction of hermeneutics to the battlefield of economic methodology, I am a little skeptical of attempts to create hermeneutical foundations for economics just by borrowing some loose and general philosophical insights and declaring their general validity, with no careful specification for the basic notion of hermeneutic understanding in the context of economics. It is hoped that progress will take place also here in the future. (For these attempts, see Lavoie 1990; for a critical account, see Mäki 1990b).

7. Conclusion

To sum up, I submit the following claims:

- * We have some rather good results produced by methodological work that can be used for answering the negative descriptive question A (it seems that the most uncontroversial theses on Wade Hands's list are of this kind).

- * We have very few good answers to the positive descriptive question B.

- * We have hardly any good answers to the rationality question C and the prescriptive question D — partly because we do not know how to seek the answers.

This is not to argue that people responsible for the recent upheaval of the methodology of economics have done bad work. The prob-

lem is rather that of the amount of available resources employed for methodological research, on the one hand, and its allocation between alternative themes and problems, on the other. First, although the number of competent methodologists of economics has grown rapidly in recent years, the numbers involved are still insufficient for producing reliable results on a wide spectrum of topics. Second, the available intellectual resources have been allocated so as to generate an unfortunate concentration of efforts on questions some of which are of relatively lesser importance.²

To summarize what I find the most important omissions in recent work on economic methodology, I would mention the problem of realism, modes of non-deductive reasoning, metaphysical foundations, social conditions, and grounded norms in economic theorizing.

In order to reconstitute the relevance of the methodology of economics, new metatheoretical frameworks have to be developed. Otherwise, questions B, C, and D will remain unanswered in an adequate way. It should also be understood that such frameworks can most probably not be adopted directly from general philosophies of science. Economics seems to have many peculiarities of its own. We should be sensitive to those peculiarities.

² As can easily be seen, the point concerning the misallocation of intellectual resources is very similar to the criticism often launched against formal work within orthodox mainstream economics, based on considerations of relevance. Part of the explanation for such a concentration or alleged misallocation of resources may also be similar in both cases: the established (Walrasian or Popperian) framework that provides an easily accessible and tractable set of categories, questions, and standards for acceptable answers succeeds in attracting a major portion of scholars in both cases, irrespective of its relevance to studying the economy or economics.

In the case of Popperian methodology, this explanation has to be supplemented by reference to the popularity of loose Popperian views among many practicing economists. In other words, many economists claim to have discursive knowledge of what economics is or should be like. This alone makes the analysis and criticism of Popperian ideas completely legitimate. The irony here may be that the Popperian views held by economists have been marketed to them, to a large extent, by those writing on economic methodology.

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