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Philosophy of Economics *versus* Methodology of Economics

ABSTRACT. As McCloskey noted many years ago, there are two views of methodology: small-m and big-M. In economic model building, small-m methodology is a study of why model builders assume what they assume when building models. Big-M methodology is the view of philosophers of economics who are more concerned with questions such as: What do economists mean by 'realism' or 'realistic' assumptions? Are economic models testable? What is the cognitive status of economic theory? And so on. Since the time when McCloskey was talking about this, the field of small-m economic methodology has been subsequently hijacked by would-be philosophers of economics to the extent that conferences that in the past would have addressed questions of small-m methodology are today devoted to topics of interest only to philosophers, particularly to analytical philosophers who reject Karl Popper's view on economic methodology. What they reject is actually a mistaken characterization created by Imre Lakatos. Contrary to Lakatos, Popper's view is not 'falsificationism'. Popper had denied such a characterization, but this fact is ignored by most philosophers of economics. As a result of their rejection of what is thought to be Popper's view of science, those of us who think Popper's views of explanation are more worthy of discussion than those of analytical philosophers of economics are too often excluded from participation in conferences about the methodology of economics.

KEY WORDS: economic methodology, philosophy of economics, Karl Popper, Imre Lakatos

1. Introduction

Thirty years ago Deirdre McCloskey raised a distinction between what she called small-m methodology and big-M methodology which suggested a difference between what economics methodologists like me talk about and what philosophers of economics talk about. What I have been doing for over fifty years is trying to identify the various answers to the simple question: Why do economic model builders assume what they assume? In contrast, philosophers of economics worry about questions such as: What do economic model builders mean by ‘realism’ or ‘realistic’ assumptions? Are economic models testable? What is the cognitive status of economic theory? And so on.

Needless to say, there is a considerable overlap between these two approaches, but they are different. While small-m methodologists will often be aware of the views of many philosophers, few will have studied philosophy. Similarly, most big-M philosophers of economics rarely know much about economics beyond the level of Economics 101. And when they do look into the writings of economists, they are doing so with an interest compatible with philosophical studies. In North America, this interest is that of analytical philosophers.

2. My early work in small-m methodology

My only philosophy related education involved one undergraduate class in ethics at Bradley University and one graduate seminar in the philosophy of science at the University of Illinois taught by Joseph Agassi. Agassi was one of Karl Popper’s students in the 1950s, but there was little discussion of Popper in our seminar. For my PhD I ended up writing two PhD theses: one on capital theory which was rejected, and one on the testability of economic models which was accepted. The latter was an acceptable alternative topic suggested by my PhD examining committee. Their explicit suggestion, which I followed, was that I should apply what Popper said in his famous 1934 *Logic of Scientific Discovery* to economic model building. I published my accepted PhD thesis as Chapters 2 and 3 of my 1989 book.

The purpose of my accepted thesis was to develop a quantitative measure of testability and apply it to numerous economic models. The question in each case was: how many observations would it take to construct a potential refutation of the model in question?[1] The simplest model I looked at was a two-equation linear Keynesian model involving two observable variables – aggregate national income, and aggregate national consumption – and two parameters. I determined that it would take 6 observations to logically refute this model. Eventually I looked at a model that my thesis supervisor [Brems, 1959] had constructed which had six equations and six observable variables. By my calculations, whenever his model was actually false and we did not know the values of its eight parameters, it still would take 24,283 observations to construct just one refuting observation set! I also looked at a model that had as one of its equations a Cobb-Douglas production function, and depending on the actual value of one of that equation's key parameters, it could require 475,017 observations! Of course, my main conclusion was that economic model builders are not very serious when they say they are concerned about the testability of their models. My paradigm for this criticism was the proclamations of Paul Samuelson, whose PhD thesis was all about producing models that are testable and who I think engaged in this activity to demonstrate to the critics of mathematical model building of his day that model building did not result in only producing tautologies, despite what the critics claimed. I see my work as being small-m methodology simply because I was not interested in the philosophical question of whether or not economic models *should* be testable.

3. The history of economic methodology 1960 to 1980

A highlight in the history of economic methodology occurred at the 1962 American Economics Association meetings. At those meetings there was a session on 'Problems of Methodology' in which six economists and one philosopher participated.[2] The session has become famous mostly for

[1] For example, it would take 3 observations to prove that one is not looking at a plotted straight line.

[2] The session was published in the 1963 Papers and Proceedings, where there are papers by Fritz Machlup, Ernest Nagel, Andreas Papandreou and Sherman Krupp, followed by discussions by Chris Archibald, Herbert Simon and Paul Samuelson.

Paul Samuelson's discussion of Milton Friedman's view of methodology. But for ten years after that one event and its publication, research in the methodology of economics was basically banned from being published in any major economics journal until briefly in 1973. That year a major methodology article was published by my former student, Stanley Wong, in an article about Samuelson's view of Friedman's methodological views.[3] There were four books on economic methodology published, but they were basically ignored by the mainstream of economics.[4] The next major methodology article published was probably my 1979 article in the *Journal of Economic Literature* that presented a critique of most of the criticisms of Friedman's methodological pronouncements as well as presenting my brief critique which identified Friedman's methodology as merely a form of 18th Century instrumentalism. Despite the publication of Wong's article in 1973, the editor of the *American Economic Review* continued to reject articles on methodology, including my subsequently published 1981 article about the maximization hypothesis. It was subsequently published only after a new editor, Robert Clower, took over the editorial duties.

While it does not count as a published article, the last chapter in Mark Blaug's popular 1962 history of economic thought textbook was about methodology.[5] This chapter was eventually expanded and published in 1980 as a book about just economic methodology. Thanks to the success of Blaug's methodology book, publishers rushed to publish methodology books, including my 1982 book and Bruce Caldwell's PhD thesis book. Beyond this point, methodology for a long time became something that the book publishers would at last at least consider.

[3] Apart from being a good methodology article, I suspect it was also published because at the time Wong was a student of Joan Robinson, who was constantly criticizing the editor of the *American Economic Review* for never considering publishing non-mainstream articles like Wong's.

[4] Two of those books had authors that were philosophers [Rosenberg, 1976; Hollis, Nell, 1975], and the others were by economists [Stewart, 1979; Klant, 1979].

[5] This chapter was expanded in the 1968 2nd edition.

4. The first issue of *Methodus*

In 1989 *Methodus*, the first journal devoted to economic methodology,[6] was created by an organization created by Henry Woo, a businessman in Hong Kong: The International Network for Economic Methodology (INEM). From its beginning, this organization had over 80 founding members (the list of founding members is published in every issue). Among the founding members there are very few philosophers. In mid-1994 *Methodus* was renamed to the *Journal of Economic Methodology,* and today it continues publishing methodology. Some think this renaming event was really a hijacking by a group of historians of economic thought and in particular a group opposed to anything dealing with Karl Popper that did not accept the view of Popper's philosophy that was created by Imre Lakatos. It is Lakatos who invented the idea of a Popperian falsificationism and the phony view of Popper of his being a 'naive falsificationist'[7] – the phony view that claims scientists are only interested in testing and refuting theories and models. Imre did not really understand Popper and created this to promote his own role in the philosophy of science. Popper explicitly rejected the Lakatosian falsificationist characterization of his philosophy of science.[8] Nevertheless, it persisted in economic methodology discussions mostly because of Blaug's promotion of that view.[9]

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[6] The first issue of *Economics and Philosophy* was published in 1985.

[7] See Imre's contribution to the discussion in Lakatos and Musgrave [1970]. Interestingly, Imre gave me a copy of that book at the conference where he debated Thomas Kuhn. Earlier, I was with a couple of Popper's former students who were reading out loud Imre's characterization of Popper's view of science. Both found it not only false but extremely humorous.

[8] See Popper's 1982 introduction to his 1983 publication of his previously unpublished *Postscript* where he says "Am I really the man who had naive falsificationism as the linchpin of his thoughts? Is the Kuhnian paradigm true? May I 'legitimately be treated as' a 'naive falsificationist', even though Kuhn admits, after looking at *The Logic of Scientific Discovery*, that, as early as 1934, I was *not* one? … Tests are attempted refutations. All knowledge remains fallible, conjectural. There is no justification, including, of course, no final justification of a refutation. Nevertheless we learn by refutations, i.e., by the elimination of errors, by feedback. In this account there is no room at all for 'naive falsification'" [pp. xxxiv–xxxv] (emphasis in original).

[9] Most of the time when economists and philosophers of economics discuss Popper today, they unfortunately continue to label him a falsificationist thanks to Blaug's misinformed characterization.

Blaug unfortunately learned about Popper only from Lakatos and hence the confusion. For many years I have been arguing that Blaug's view of Popper is fundamentally ignorant. I discussed the two competing views of Popper in Chapter 20 of Boland [1997]. Those views are on the one hand Lakatos' so-called Popperian falsificationism view, and on the other hand the view that most students of Popper promote – namely, the view that Popper advocated the same view of learning that Socrates advocated in Plato's early dialogues. Specifically, Popper says we learn by criticizing and then correcting our knowledge errors. Thus, for Popper, Science as a process of learning is devoted to criticism.

Philosophers who agree with Blaug's view that Popper is a falsificationist use it to reject Popper by claiming that economists do not go about testing and refuting economic theories. They are right when it comes to theories such as neoclassical economic theory or Keynesian macroeconomic theory. But they are completely wrong about the absence of testing and resulting refutations. As I have argued many times, testing and refutations happen regularly in economics. Empirical economic model builders frequently test their models, and when they fail to fit they are rejected. Theoretical model builders are often rejecting models for not fulfilling their aim of building particular models. If these philosophers understood modern economics and the dominance of both empirical and theoretical model building,[10] they would not be rejecting Popper for the absence of testing and refutations.

Conferences have been devoted to discussions of economic methodology – INEM has been holding meetings about methodology roughly every other year, beginning when it held them usually in conjunction with the History of Economics Society meetings. The most recent INEM meetings were held in Cape Town, South Africa in 2015. In between there have been several independent conferences held to discuss specific topics, such as Popper's or Friedman's view of methodology. First there was a 1985 con-

[10] For a discussion of this distinction between these two types of model, see Part I of Boland [2014].

ference on Popper held in Amsterdam.[11] Most of the conference's papers were published in a book which I reviewed in 1990.[12] Despite my well-known publications in economic methodology, for some unknown reason I was deliberately not invited. This was surprising since, by most standards, I was the only Popperian methodologist publishing about economics.

The second methodology conference was held in December 2003 to celebrate the fiftieth anniversary of the publication of Friedman's famous essay on economic methodology, the exact same essay I wrote about in my 1979 *JEL* article discussed above. Despite my 1979 article being one of the most cited methodology articles, again I was not invited to the methodology conference. Most of the papers presented at that conference were subsequently edited and published by a prominent analytical philosopher of economics in a 2009 book which I reviewed for *Economics and Philosophy* in 2010.

I note these two conferences and their published essays for the reason that not only were they conferences explicitly about economic methodology but also as they illustrate a sickness among philosophers and some economists interested in the history of thought and methodology. Why would someone organizing a conference on Popper's legacy in economics not invite someone who is prominently known as a Popperian methodologist? Similarly, why would someone organize a conference about Friedman's famous methodology essay and not invite the author of the most cited article about that essay? Moreover, I raise this not because I happen to be the victim of this sickness but that it illustrates something I have often seen at methodology conference sessions where someone in the audience stands up and criticizes someone who is not present or in one case where papers were presented that criticized someone who was deliberately not invited.[13] I find this behaviour unethical. It also displays a weakness in

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[11] It was held to honour the retirement of Joop Klant who in Europe was considered the prominent proponent of Popper's philosophy of science in economics. The papers were published in de Marchi [1988].

[12] The review of the conference volume appeared in the *Research in the History of Economic Thought and Methodology* 1990–1992, vol. 7.

[13] In the first case, I was chairing the session which was held in Vancouver and scolded that behaviour. In the second case, I was again the victim where a HES session at George

the confidence many philosophers of economics and some historians of economics interested in methodology have in their own views of economic methodology.

5. On the hijacking of economic methodology

In Europe today, almost all of the conferences are dominated by the interest of big-M methodologists. Judging by the usual topics of concern in journals that publish economic methodology discussions, one gets the impression that those interested in the philosophy of economics have hijacked INEM as well as its methodology journal. Nowhere does one find anyone discussing the burning issues of the 1980s, and it was there that one finds a growing discontent with discussions of Popper's philosophy of science, culminating in the 1985 conference discussed above. While economists were never discontented with the discussions of Popper, almost all philosophers of economics reject any discussions of Popper, and particularly in terms of the Lakatosian falsificationist version of Popper's views.[14]

It does not seem to matter how much some of us who do understand Popper repeatedly point out that attempting to test a theory is just one form of criticism – testing is not the main issue in Popper's philosophy of science, particularly when it is applied to economics.[15] For Popper, criticism of conjectured theories and models is science's main business. For Popper, a successful lab experiment produces a refutation, not a confirmation. The point of Popper's perspective is that one is not trying to prove that a theory or model is true (even when it may be), simply because such a proof is impossible. The only thing that can be proven is that a theory is false (when it

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Mason University was held to discuss Friedman's essay and criticisms such as mine but, even though I was attending those HES meetings, I was not invited to respond. And similarly, at the 2003 conference I mentioned above, I was criticized by Blaug even though I again was not invited to respond.

[14] The most prominent philosophers of economics who have this negative and misdirected view of Popper are Alex Rosenberg and Dan Hausman.

[15] See Chapter 8 of Mark Notturno's 1994 collection of Popper's papers, where Popper explains his view of the methodology of economic theory.

is false). In general, Popper rejects the view of methodology that sees its purpose as being to justify any knowledge claim. His followers see his philosophy as being mainly a rejection of the pursuit of justified true belief. Unfortunately, too many analytical philosophers think that the justification of belief is the only purpose for science and its lab experiments.

The primary perspective of the philosophers of economics continues to be that methodology is a normative study, a study concerned mostly with appraising economic models and theories. The primary purpose for appraising models and theories is to choose one based on conventional criteria. Moreover, given their acceptance of the Lakatosian falsificationism view of Popper, these philosophers seem to think that Popper is advocating falsifiability as a criterion to choose between theories and models. As I noted at the beginning, when it comes to discussing the practice of economic methodology, few if any analytical philosophers know much about what economic model builders do beyond what they learned in Economics 101.

My concern here is not that I think we should be spending our time discussing Popper's pronouncement on methodology, but instead that we should be following his lead to explain what economists do and assume by identifying the methodological problem they think they are solving. If we take this approach, we will find that most model builders today are not trying to build realistic models of their economy but trying to solve methodological problems involved in building formal equilibrium models. These methodological problems are mostly problems that arise in the application of specific mathematical techniques depending on whether the model being built is empirical or theoretical.

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