Style vs Substance in Economic Methodology

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Until recently, economic methodology was never a fruitful research field for an aspiring economist trying to establish a career. The reasons were simple. Methodology was supposed to be written only by successful economists in their retirement years, sharing their wisdom to tell us about the methods they used to succeed. Young, beginning economists were always discouraged on the grounds that they did not know enough, and besides, talk is cheap. Thus in the 1960s, when I began, not only did one face the problem of finding something to discuss from a methodological viewpoint that would interest an ordinary economist, one had to overcome the sociology of the profession. In the last ten or twelve years, things *appear* to have dramatically changed. Today, almost anyone can hire a hall and put on a methodology play. It seems that today the only decision facing the would-be methodologist is one that involves the selection of the playwright whose script will be performed.

The situation facing an aspiring methodologist today is so radically different from the one I faced in the 1960s that communication between me and this new generation is usually very difficult. One would think that given all the current discussion of Karl Popper's views of the philosophy of science that communication ought to be easy but a communication obstacle persists. In this paper I will attempt to explain why this obstacle persists both for me and for anyone who studied Popper's views of the philosophy of science in the early 1960s or earlier. Moreover, I will explain why this obstacle might not be easily overcome.

Economic methodology in the 1960s

First let me begin by describing my situation in the 1960s. Against the expressed wishes of my undergraduate teachers I decided that I would specialize in the study of economic methodology. I do not remember exactly why I became interested in methodology. I do recall that, as a student reading economics, I felt that my understanding did not begin until I was told the aim of the writer and why the writer was proceeding this way rather than that way. But I think my interest in methodology started before I began my career in economics. I remember my decision to drop out of the engineering school in my final year and then my attempts to decide on something else to study. I shopped around – economics, philosophy, art school, music school, and so on. Each of these were considered because I had taken classes from these departments. In the process I remember reaching the 'profound' conclusion that despite the wide diversity of these areas, the teachers seemed to be saying the same thing while communicating it in different ways – 'Boy' discovers Conventionalism.

My interest in methodology was rather mundane. At the time I had not heard of Karl Popper or any other philosopher of science. My fellow students and I were very lucky to have some excellent undergraduate economics teachers who were more interested in helping us learn than in the usual posturing to convince us that they were important members of the modern economics establishment or that they were up on the latest fads. Above all, we were encouraged to take a critical approach to understanding. I am not sure I understood the virtues of that approach at the time since I thought the study of economics was going to be very easy. After all, since economics is mostly concerned with maximization and its consequences, it was easy to see that my task would be simply one of learning to apply my understanding of mathematics to economics. As I eventually learned, such a task is more likely to involve boring exercises in puzzle solving than learning new ideas.

Given my extensive mathematics background, my teachers encouraged me to pursue a graduate degree in the newly growing field of mathematical economics (which in 1962 was the current fad for new graduate students, particularly at 'developing' economics departments). With the help of a government-funded fellowship, I did this. My graduate program permitted a few electives and for rather obscure reasons I signed up for a seminar chaired by a Dr Agassi. Now it seemed I would have a chance to learn more about ‘methodology’ or what I later came to know as Conventionalism. Of course, everything I heard or read I interpreted as some variant of the Conventionalism that I had previously ‘discovered’.

After having my first PhD thesis rejected for being ‘too philosophical’, I was asked to write a completely different thesis. The thesis that I was requested to produce was an attempt to apply what I saw as Popper’s philosophy of science to economic model building. Why this project was not seen to be ‘too philosophical’ is a mystery to me. Nevertheless, armed with my freshly accepted PhD thesis I entered the economics profession – as a mathematical economist!

As a beginner in 1965 and without any support from my economics colleagues I saw my task as one of applying philosophy of science (viz. Popper’s) to economic theory and model building. (You would think I would have learned from my previous mistaken view that the study of economics is no more than applied engineering mathematics, but obviously I did not.) Apart from the questions which I examined in my thesis [see Boland 1989, Chapters 2 and 3], I found very little in modern economics that would lend itself to direct application of philosophy. However, things got much easier as I learned more economics. For the most part, I worked alone as there were few if any economists in my generation who were interested in the study of economic methodology. Things changed in the late 1970s and now there are gobs of people writing papers on economic methodology. Unfortunately, little of the methodology published over the last two decades has moved beyond the naive Conventionalism that I ‘discovered’ in the early 1960s.

In the remainder of this paper I wish to critically examine the logic of the situation which has been facing the would-be methodologists of the new generation during the last decade. I think there is something peculiar about the logic of the situation which seems to necessitate the aforementioned communications failure. In this regard, the question of interest is why in the last ten or twelve years has the new generation made no progress beyond promoting, as the latest fads, variants of Conventionalism? Put another way, why does there seem to be more style than substance in the methodological writings of the new generation of economic methodologists?

A theater for the new generation of methodologists

Despite the sparseness of methodology in the 1960s and 1970s, this emphasis on style rather than substance is particularly troublesome. Many of the current generation of methodologists seem to think that the field of economic methodology has always been as thriving and active as it appears today. Moreover, unlike the 1960s and 1970s where there were few if any outlets for methodology papers, the new generation have numerous journals in which to publish. There are two reasons for this change of scenery. The most obvious is the publication of Mark Blaug’s 1980 methodology book. The less obvious but probably of more direct importance is the influence of Warren Samuels who in 1979 as editor of an economics journal set about deliberately publishing a ‘symposium’ on methodology. Subsequently, Samuels (with the help of Mark Perlman) went on to help the History of Economics Society set aside some of their yearly sessions to the exclusive domain of economic methodology.

In some ways it is unfortunate that methodology has found room to flourish only with the good graces of the historians of economic thought. While historians of thought find the study of methodology useful, their perspective seriously distorts the study of methodology. Seldom is independent thinking encouraged, instead one is encouraged to search through philosophy literature to find authoritative quotations (unfortunately, sometimes without concern for context). While the obvious primary source for authorities is the philosophy of science, which philosopher is considered to be the current authority is always a matter of fad. Consequently, the last ten or so years has seen the passage of several fads, each of limited duration. Let me quickly review the sequence of fads in the 1960s.

A brief overview of the decade of the new generation

The new generation began with an outright Conventionalism that was often illuminated with a Kuhnian spotlight. While there was an occasional reference to 'paradigms', the overriding concern was for algorithmic theory choice. However, owing to Blaug's book, the new generation quickly changed to a consideration of Lakatos and his tools for 'appraising' the *progress* of economics. Concern for progress is, of course, the hobby-horse of almost all historians of economic thought. There is no obvious reason for a methodologist to be concerned about 'progress'. Before the ink was dry on the 'appraisals' of such things as general equilibrium analysis or macroeconomics, the party was crashed by Donald McCloskey and his anti-methodology methodology which he labelled 'the rhetoric of economics'. There was much gnashing of teeth among methodologists [e.g. Caldwell and Coats 1984]. Lurking behind the scenes was a plea for more sociological studies of economics. In the audience we still could see some learned observers patiently biding their time.

With each change of scenery, and each appearance of a new script, I do not wish to suggest that there has been a change in the dramatic company of players. In some (and perhaps most) cases the players have not changed, only the fads have changed.

Before I examine the leading players and their performances, let me review some of the available scripts and associated stage props.

The available playwrights, scripts and theatrical props

The leading playwrights are, as I have noted, a historian of science (Thomas Kuhn), a philosopher of mathematics (Imre Lakatos) and an economic historian (Donald McCloskey). I do not mention Popper here since almost nobody is seriously trying to follow one of his scripts. While there is much discussion of Popperian methodology or 'Popperian falsificationism', what is actually promoted or vilified is the caricature of Popper created by Lakatos.

Kuhnian paradigms

One can spot a Kuhnian script by simply noting the use of the stage props 'paradigm', 'normal science', 'scientific revolutions' and so on. Again, the use of such code words reflects the academic interests of historians of thought since who else would be concerned with these concepts rather than questions of methodology per se. In other words, unless one is writing a history of economics, these concepts lack substance. One could of course easily characterize mainstream economics as a discipline adhering to the neoclassical paradigm, namely a discipline which builds market-based models that employ the maximization hypothesis. Proceeding in this way, one would then note that neoclassical economists never question the realism of this assumption but instead proceed as if they are puzzle solving. The puzzle in question is how to build a model using the maximization assumption and still explain apparent non-maximizing behaviour (e.g. marriage, capital punishment, etc.). There is no question of being able to solve the puzzle, it is only a matter of discovering the missing pieces.

Kuhnian methodology papers are seldom about puzzle solving. Instead they are about revolutions and whether or not an alleged revolution occurred. For example, 'Was there a marginalist revolution?', 'Was there a Keynesian revolution?' and so on. The only puzzle here is whether the historian of thought can identify a change in the prevailing paradigm. Economists who promoted the application of Kuhn's view to economics rarely grasped its sociology of knowledge underpinnings and in particular its dependence on psychologism (viz. that according to Kuhn [1970] science differs from other social activity merely because scientists have a scientific mentality).

Lakatosian hard core

Following a 1974 conference volume about applying Lakatos' alleged philosophy of research programmes to economics [Spiro Latsis 1976], much of the work during the late 1970s and early 1980s was devoted to advocating a shift from Kuhnian scripts to scripts authorized by followers of Lakatos. There are two essential aspects of 'Lakatosian' plays. One involves either Popper-bashing or Popper-promoting and the other involves excessive concern for the 'growth of knowledge' and, in particular, the question of how or whether economics has made progress since Adam Smith.

Among economic methodologists, Popper-bashing is usually the result of people not reading Popper but instead reading Lakatos and his accounts of Popper's discussion of science. In particular, it is the result of Lakatos' notion of the 'falsificationist' methodology that is attributed to Popper. This 'falsificationism' is very much a version of Conventionalism, one which uses falsifiability as a Conventionalist criterion to choose the 'best' theory or model. This attribution leads to two types of complaints. On the one hand, some argue that Popper's views are worthless because economics is impossible to falsify [e.g. Dan Hausman 1988]. On the other hand, it is argued that economists do not practice what they preach; that is, they preach falsificationism (e.g. they preach that testability matters) yet they do not engage in falsificationist activity (viz. in conjectures and refutations) [e.g. Blaug 1980]. Many of the proponents of the latter argument see themselves as Popperians but, unfortunately, their implied falsificationist prescriptions have little to do with Popper and everything to do with Lakatos.

The most common characterization of Popper among these followers of Lakatos is that Popper is claiming that the 'growth of knowledge' requires a commitment to falsificationism in the form of conjectures and refutations. It is a view that sees falsification (rather than criticism or 'critical rationalism') as the essential part of a dialectic that determines the evolution of a science. Again, this emphasis on the history of scientific knowledge is the central theme of all plays inspired by Lakatos and performed by historians of economic thought.

Lakatosian plays are characterized by stage props such as 'the hard core', 'the protective belt', 'progressive and degenerative scientific research programmes', 'novel facts', 'ad hoc auxiliary assumptions', 'the Duhem-Quine thesis', 'internal vs external histories' and below all the code words, it is essential to have excessive footnotes. To perform a Lakatos play one merely has to solve the following puzzle: Can discipline X be characterized (or modelled) as a Lakatosian scientific research programme? The solution to such a puzzle will identify the aspects of discipline X that constitute the 'hard core', the 'productive belt', the 'auxiliary assumptions', the 'research programme', etc. [e.g. Roy Weintraub 1985]. For the most part, this is a tedious exercise. But what is most important is the extent to which such an exercise is so appealing to economists. This is not entirely surprising, though. After all, neoclassical economics proceeds in very much the same puzzle solving way. Where the director of a Lakatos play looks for the 'hard core', the neoclassical model builder looks for the 'utility function' which is to be maximized. Where the director looks for a 'protective belt', the neoclassical model builder looks for the 'cost function' that constrains the maximization process. In other words, a Lakatos puzzle is an analytically trained economist's dream since he or she is not required to look at the world in a different way.

McCloskey's rhetoric

For years one characteristic of economic methodology literature has been its pretentiousness. And again, its demeanor is due to its history of thought orientation. To an ordinary economist, methodologists are viewed as would-be high priests who think they alone can decide whether someone's work constitutes progress. The ordinary economist considers high-sounding methodology to be worthless or at most unhelpful. It was to this audience of ordinary economists that Milton Friedman's famous 1953 methodology essay was directed – his essay is primarily a diatribe against the prescriptions and proscriptions of the followers of 1930s Logical Positivism. Its popularity is due entirely to its anti-methodology message. McCloskey [1983] exploits the same cynicism by promoting his own anti-methodology methodology. McCloskey refers to methodology with a capital M to emphasize that methodologists see themselves as publishing Big Ideas. He complains about economists who as amateurish methodologists dismiss various ideas as 'meaningless' or 'unscientific'. He sees such labels as evidence of attempts to practice the tenets of (logical) positivism which he calls 'modernism'. In its place, McCloskey argues that we should instead be concerned with the 'rhetoric of economics'. The distinguishing stage props in a McCloskey script are quotations from Richard Rorty or Northrop Frye or Stanley Fish, references to 'literary criticism', 'sprachethik', 'metaphors', 'discourse', etc. Not much is done with these except to establish a certain negative style. The typical rhetorical play has no more substance than a 'White-Hall comedy' although some of the details and one-liners can be entertaining with the right audience.

The company of players

There are surprisingly only a handful of major inhabitants of the company of economics players. In addition to Blaug and McCloskey, there is Terence Hutchison (who is credited with being the first to introduce Popper to economists), Bob Coats (who has carved out a niche for himself by writing about the history and sociology of the economics profession), Wade Hands (who like me started in mathematical economics), Bruce Caldwell (whose very popular book was developed from his PhD thesis), Arjo Klamer (who sees himself as an anthropologist specializing in the study of the economics tribe) and Uskali Mäki (a philosopher who teaches economics as well as philosophy). There are other players (including some philosophers who think economics might be a good place to ply their trade) but these are the ones I will discuss.

Of all the players mentioned, only Hutchison seems to understand Popper even though when he introduced Popper [Hutchison 1938] he was merely promoting testability as an alternative to the verifiability of the 1930s positivists. For the most part, Hutchison has been patiently allowing the various movements of the troops without interfering too much [cf. Hutchison 1988]. Blaug has not changed his tune much since he endorsed the Lakatos caricature of Popper [Blaug 1975] and McCloskey offers nothing of interest to methodologists. Klamer seems to be stuck in a rut but this is somewhat understandable in that he never took methodology very seriously in the first place [e.g. Klamer 1984, 1988]. Coats only occasionally ventures into methodology and when he does, it is usually to promote a sociological perspective [e.g. 1984]. Mäki has brought his philosophical skills to bear on many of the controversies [e.g. 1988, 1990] but unfortunately it is too easy for an ordinary economist to see his work as one of drawing exceedingly fine distinctions that only a philosopher could appreciate.

This leaves the two most prolific of the new generation, Wade Hands and Bruce Caldwell. Wade began with a review of the Lakatos conference volume [Hands 1979] in which he endorsed to a limited extent the introduction of Lakatos into economic methodology. The uneasy endorsement of Lakatos is extended in his review of Blaug's methodology book. But given Wade's familiarity with the work of Noretta Koertge, he recognizes that one must not confuse Lakatos with Popper [Hands 1984a] yet Wade seems to take the opportunity to bash Popper or at least the allegedly falsificationist Popper. The Popper-bashing is also pursued in the same year in Wade's discussion of 'crucial counterexamples in the growth of economic knowledge' [Hands 1984b]. Later Hands exercises his membership in the Popper-bashing club by focusing on 'ad hocness in economics' [Hands 1988]. Despite recognizing the inequality between the views of Lakatos and Popper, Wade seemed intent on pursuing these Lakatosian themes. In 1985 he gives some 'second thoughts on Lakatos' and a 'new look' to Popper and economic methodology [Hands 1985a, 1985b]. Wade asserts that Lakatos' methodology is 'ill-suited' to economics yet it 'can still provide valuable guidance' (i.e. second thoughts merely mean a more limited endorsement). Karl Popper is thrashed for crimes which are due more to Lakatos' caricature than to Popper himself. It seems that Wade cannot entirely give up the Lakatosian viewpoint.

Rather than view methodology as a tool to study the history of economic thought, Caldwell has been more interested in the history of economic methodology, itself. Thus, his approach to methodology is rather unique. Nevertheless, Caldwell began with a limited endorsement of the Conventionalist theory-choice oriented view of methodology [Tarascio and Caldwell 1979]. That is, Bruce started with the same naive Conventionalist view as I did twenty years earlier. While I still do not think Bruce has progressed very far beyond his version of Conventionalism (which he initially called 'pluralism'), he has made more progress than any other methodologist of his generation. Now most important, I do not want anyone to think my criticism of Bruce constitutes disrespect. To the contrary, he is virtually the only methodologist of his generation who has progressed to the point of confronting the limitations of the Lakatosian version of Popper. Still, his view of Popper can be most perplexing.

In a comment on Wade's review of Blaug, Caldwell [1984] seems to jump on the Popper-bashing bandwagon by seeing 'falsificationism' as an alternative solution to the conventionalist's theory-choice problem. Actually, his comment is just an elaboration of a chapter from his 1979 PhD thesis. Apart from making public his membership in the Popper-bashing club, Caldwell's PhD thesis [and its reincarnation as Caldwell 1982] is a fairly good general history of economic methodology. At a conference which Mäki held in Helsinki, Bruce presented his overall views of economic methodology [Caldwell 1990]. The Big Question was 'does methodology matter?' and the focus of the conference (apparently due to Mäki) was 'realism'. And while seizing the opportunity to promote his version of Conventionalism (which he is now calling 'critical pluralism'), Bruce too engages in a little more Popper-bashing such as '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'. Does 'Popperian thought' ever do this? I think I smell Lakatos here.

The most perplexing contribution by Caldwell is his recent article which purports to be 'Clarifying Popper' [Caldwell 1991]. Bruce kindly sent me an early draft of this article. I responded by asking a few obvious questions, not the least of which was why he thought *Popper* needed clarifying. But perhaps I am not being fair since he may merely be clarifying the mistaken image of Popper that is due to the overly zealous followers of Lakatos. Yet, even though explicitly recognizing that Popper does not use the word 'falsification*ism*' [p. 2, footnote 1], Bruce insists on using it to characterize Popper. The odor of Lakatos simply will not go away, will it?

Both Hands and Caldwell see a major contradiction between 'falsificationism' and 'situational analysis' which are seen as two essential aspects of a Popperian 'position'. Wade and Bruce appear to be saying that this is Popper's problem even though readers familiar with Popper's writings can see that this is at best a problem of accepting the Lakatosian view of Popper as true. While Hands sees this as an unavoidable fault of Popper, Caldwell sees this apparent contradiction as a methodology-choice problem. 'Popperians' must give up either 'falsificationism' or 'situational analysis' or both. Blaug is accused of giving up 'situational analysis'. Surprisingly, Caldwell claims to solve this 'dilemma' – he is saying that all we have to do is recognize that both 'falsificationism' and 'situational analysis' are merely two elements in the more general methodology which he calls 'critical rationalism' [1991, pp. 22-7]. And about this Caldwell says 'I call this my own solution simply because Popper has never acknowledged that a tension exists between falsificationism and situational logic, and has never portrayed critical rationalism as providing a resolution of the conflict' [p. 22]. Bruce says this while explicitly recognizing Popper's rejection of the term 'falsificationism'. My olfactory nerves are getting a real workout now.

Towards a post-Popper economic methodology

With all of the fuss and the plays about Popper you would think he is a major figure in economic methodology. Nothing could be further from the truth. Despite Hutchison's early efforts [1938] and Richard Lipsey's brief flirtation [1963], the promotion of falsificationism in economics is due mostly to Paul Samuelson's success at developing mathematical economics. It just happens that while Samuelson claimed his purpose was to develop 'operationally meaningful' propositions in economics, his definition of an 'operationally meaningful' proposition was that they must be refutable [1947/65]. The only time Popper is promoted by ordinary economists is when someone thinks Popper offers philosophical authority for what is really Samuelson's methodological pronouncements. Surprisingly, there does not seem to be much Samuelson-bashing when it comes to the discussion of methodology. Instead, it is the Popper that Lipsey called upon that has been the target of so much abuse.

I think it is time for economic methodologists to move beyond childish Popper bashing. Popper is here to stay even though few economic methodologists have learned much from him. It is not enough to have a critical attitude, one must be fair. Criticism begins and ends with understanding. Appealing to philosophical authorities is an insufficient substitute for understanding. It is more important to understand what economists think they are doing than being able to perform according to the latest philosophical fads.

There are alternative ways to produce methodological research than to see it as a subdiscipline of the history of economic thought. While there are good reasons for considering the methodological aspects of the subjects of historical research [e.g. Mirowski 1984 and Schabas 1990], a better task for the methodologist is to critically examine some of the less grandiose subdisciplines of economic theory. Three methodologists come to mind. While they approach methodology in very different ways, Philip Mongin [1986], Richard Langlois [1990] and James Wible [1984-85, 1990, 1991] have produced critical work worthy of consideration although their work is unlikely to appeal to those methodologists who constitute the prime audience for the latest rendition of a Lakatosian play. Rooting out the methodological presuppositions of ordinary economists can be very revealing and who knows, such efforts might lead to the improvement of economic theory.

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