Methodological criticism vs. ideology and hypocrisy

Lawrence A. Boland, FRSC
Simon Fraser University

There was a time when any university-educated economist would be well-versed in philosophy of science and methodology. Usually in the 1940s and 50s, the accepted views of methodology were versions of logical positivism. But, little of the philosophy literature had anything much to say about economics. Occasionally, at the annual American Economic Association meetings, there was a session devoted to the discussion of methodology. But even this limited discussion seemed to reach a concluding climax during one particular session at the end of 1963. The only question seemingly at issue was which side were you on, Milton Friedman's – as expressed in his famous 1953 essay – or Paul Samuelson's – as expressed in his seemingly victorious critique of that article presented at the 1963 AEA meetings. As a result, if one were interested in studying economic methodology – but one was not interested in the grumblings and gossip surrounding Friedman's essay – there really was not much to read about economic methodology.

Of course, in the 1960s, when I began studying economic methodology, one could always find a cursory discussion of methodology in the opening chapter of most – but not all – textbooks. Those that did include such a chapter might make some reference to Friedman – but it was usually as an example of a 'positivist'. The one thing in common to all discussions of economic methodology was the ubiquitous positive vs. normative distinction.

Those of us who were being trained to be the vanguard of the new 'mathematical economics' were inspired and motivated by Samuelson's seeming expertise in methodology – particularly his demonstrated expertise in his famous Ph.D. thesis, *The Foundations of Economic Analysis*. Clearly, anyone interested in 'theory' rather than 'applied economics' would be on Samuelson's side. And so, we – budding 'pure' theorists – were all trained to dismiss Friedman's view as some form of positivism or logical positivism having little to do with any of the 'scientifically meaningful' theories that we were interested in.

Looking back, all this seems confused since it now seems that Milton's famous essay is really a rejection of the demands of the logical positivist philosophers of science. And Samuelson's use of the term 'scientifically meaningful' sounds an awful lot like the rhetoric of those same positivist philosophers.

It is easy to understand why we might have thought that Friedman was advocating some form of positivism (logical or otherwise) since the word 'positive' was in the title! Having chosen Samuelson side – the one which rejected Milton's view of methodology – I never saw a need to actually read Milton's essay. Moreover, following the party line, I continued to refer to Friedman as a logical positivist or more generally, as in a 1970 article I published in the journal *Philosophy of Science*, I saw him as just another 'conventionalist' in competition with other conventionalists such as Samuelson. At issue in the competition were acceptable methodological theory-choice criteria: I saw Milton as a promoter of simplicity (which would be appropriate for applied economics) and Samuelson as a promoter of generality (which would usually be seen to be a goal of mathematical analysis). And, in a 1971 article in the same philosophy journal, I discussed theory-choice criteria more generally and I did explicitly explain 'Instrumentalism' but without reference to Milton or his essay! Again, I thought we must see methodology as a mediator between whether to pursue 'applied' or 'pure' theory. It would seem then that I had identified Friedman with 'applied theory'. For me, any applied theorist was like a television repairman (someone who might believe there are little men in the tubes or transistors). And since the truth of the repairman's understanding of physics does not matter so long as he fixes the broken television – and since Friedman reportedly said that the truth of the applied theorist's assumptions do not matter – I was implicitly making the connection between Friedman's views and what the philosopher, Karl Popper, called 'instrumentalism'. But, since to this point I had not actually read Milton's essay, I held to the party line that Friedman was a positivist.

In the fall of 1971 I helped Stanley Wong write his paper about Samuelson's views of methodology (which was subsequently published in the 1973 *American Economic Review*). I explained to him that Friedman's essay could be interpreted as an exact form of the instrumentalism that Popper had often criticized. Of course, obeying the party line – and foregoing any pretence of scholarship – I still had not bothered to read Friedman's essay.

A brief history of my 1979 *JEL* article[1]

So, if it was so easy for me to avoid reading Milton essay, how did I come to write my infamous *JEL* article about the popular critiques of his famous essay? Well, in the summer of 1975, I overheard a colleague (a loud proponent of mathematical economics) explaining *his* view of why Friedman's methodology was all wrong. I quickly realized that what I was hearing was merely the same critique that Samuelson had published twelve years earlier. Apparently, this inspired me to think I could teach my colleague some methodology. By this time I acquired some appreciation for scholarship and so began reading Friedman's essay. I was shocked by what I found. Of course, there was the expected but vague reference to the distinction between positive and normative economics (credited to John Neville Keynes), but there was nothing in Milton's essay that could be considered a clear version of positivism or even logical positivism. As I noted earlier, what I found was that his essay was more an argument *against* positivist methodologists. Shocking, indeed!

Since I was teaching a methodology seminar that semester, I wrote up my paper and presented it to my seminar. And, in order to get some professional feedback and criticism, I tried twice to get it on the program of the meetings of the Canadian Economics Association. It was rejected both times. This was understandable since methodology papers are rarely accepted for the CEA meetings – and surely no methodology would ever have been accepted that might have been seen to defend Friedman in any way. So, I submitted my paper to the *Journal of Political*

Economy in March of 1978. Simultaneously, I sent a copy to Milton. By the end of the following month, I had received a long letter from him giving explicit support for my criticism of the critics of his essay. Two days later I received a letter from George Stigler who, as editor of the *JPE*, rejected my submission presumably on the grounds of a referee's report that said I had misread Friedman. I wrote back to Stigler enclosing a copy of the letter from Friedman and suggesting that my paper could be worthy of reconsideration. Apparently, Milton's opinion carried no weight and George simply said that the *JPE* was 'not interested in this paper or any reasonable revision of it '. Fortunately, Mark Perlman – with the subsequent advice of Mark Blaug – chose to publish my paper. Thus, I went from a journal with 15 thousand subscribers at that time to one with 26 thousand. A definite Pareto improvement, I would say.

Friedman's essay vs. Friedman's methodology

Years later I was told that a saga had ensued because the publication of my article violated some sort of detente between Chicago and the Ivy League among the members of the editorial boards of the AEA – presumably it was an implicit understanding which assured that no article would be published that one could think anyone might use to defend Friedman's view of methodology. Subsequently, two (more public) questions flowed from the publication of my 1979 article. First, as historians of economic thought are wont to do, some of them began worrying over who was the first to identify Friedman as an instrumentalist. Second, there was the misdirected worry over just what philosophy or methodology of science Friedman-*the-man* truly advocates.

Now, I have never claimed that I was the first to publicly identify Friedman's essay as an argument for instrumentalism. Moreover, since I helped Wong write his 1973 article that made this identification, I am in a position to know that I was not the first. However, it still must be recognized that until my infamous 1979 article was published, the common view was that Friedman-the-man was a logical positivist. So I think the more interesting question that historians of thought should be concerned with is *why* the abrupt change in the common view? Surely my article played a significant role. Could it simply be that I, as a mere methodologist, made a convincing argument?

To a great extent I have always found the question of what Friedman-the-man's true position is regarding methodology to be a waste of time. After all, my article was not about Friedman-the-man. It was only about the methodology promoted in his 1953 essay. Nevertheless, several writers consider it an important question. It is doubtful whether Friedman-the-man would want to be narrowly categorized as an instrumentalist – and such a characterization was never my concern; my concern was only with what Milton argued *in his essay*.

In correspondence with a couple methodologists, Milton reported that he thought my 1979 essay was 'entirely correct'. Of course, what Milton probably thought was 'entirely correct' is my argument that all the critiques were wrong. And it should be recognized, as a matter of consistency, instrumentalism would never have us worry over whether my assumptions concerning the nature of his essay are true but only whether they obtain the desired result, namely, the refutation of all the critiques of his essay. But remember, the central issue is that my 1979 article was about Milton's 1953 essay. In my article, I made no claims about Friedman-the-man. Surely, Friedman-the-man is free to say all sorts of things that are inconsistent with *my* interpretation of his essay.

Instrumentalism vs. ideology

In Part 1 of my 1997 book, I argued that much of the methodological criticism surrounding Milton's 1953 methodology essay is ideologically motivated. Moreover, I think the ideological basis of the methodological criticism too often is seen to be a sufficient justification for *unfair* criticism. And, I think it also seems to justify a certain degree of hypocrisy.

The ideology and hypocrisy was most evident at a conference I attended at Trinity College, Cambridge, in 1983. This conference – organized by the post-Keynesian *Cambridge Journal of Economics* – was held to celebrate the 100th birthday of John Maynard Keynes; it was a conference held specifically to discuss Keynes and *his* methodology. Over half of the participants were econometricians – presumably they thought that the conference must obviously have been intended to be about econometric method and their reason would be that in 1939 Keynes published a critique of econometric methodology. A group session was organized on the last day to discuss the entire conference. This, I thought, was an excellent opportunity to engage in some *empirical* methodology research – and so I conducted a survey of the attending econometricians' view of methodology. To do this, I first outlined the essential, fundamental notions of Friedman's essay concerning instrumentalist methodology but *without* ever mentioning his name or his article. Then I asked just two survey questions. First, who in the group agreed with these fundamental methodological notions? Amazingly, all of the econometricians held up their hands to show agreement. So, for my other question, I asked who among them agreed with Friedman's methodology. Since virtually every one of them attended the conference because they identified with left-of-center post-Keynesian economics, it probably was not surprising that they all denied any agreement with Friedman's instrumentalist methodology. Of course, this inconsistency could be clear evidence of hypocrisy but it may simply be evidence of their ignorance of methodology – more likely, in most cases, it is simply both.

The hypocrisy is most evident among today's mathematical model builders, including both econometricians and game theorists who might deny any espousal of Friedman's methodology. Typically, they willingly dismiss any concern for the realism of their assumptions and instead see methodological questions to be only about the 'tractability' of their models.[2] This attitude is, in the end, nothing more than straightforward instrumentalism – yet, I doubt today's model builders would ever see themselves as followers of Friedman's methodology essay.

More than thirty years ago I noted that economists could be divided into two groups: those that agreed with Friedman's essay and those that did not. Given the overwhelmingly dominate emphasis on formal mathematical model building in today's graduate schools, we are considered free now to assume whatever we want so long as we are explicit *and* the result is a mathematically tractable model. With this in mind, it should be clear to everyone that today the latter group has become very small.

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Notes

1 Much of my discussion here is based on a more extensive version that is available in Chapters 1 and 4 of my 1997 book.

2 In my recent book [Boland 2003], I explicitly discuss how today's model builders usually exhibit profound confusion whenever they dare to discuss methodology. Typically, we are told that their purpose is to produce a so-called 'scientific' theory and thus: 'In constructing such a theory, we are not trying to get at the truth, or even to approximate to it: rather, we are trying to organize our thoughts and observations in a useful manner' [Aumann 1985]. And in particular, the purpose of building models or constructing theories is seen to be analogous to creating an office filing system, and as such: 'We do not refer to such a system as being "true" or "untrue"; rather, we talk about whether it "works" or not, or, better yet, how well it works ...' [*ibid*.]. This attitude is merely instrumentalism one level removed from the practical level with which Friedman's essay was concerned. That is, it is now not a question of whether the practical policy recommendations are useful or work, but whether the assumptions are useful or work in the context of the mathematical objectives of formal model building.