Seven decades of economic methodology: a Popperian perspective

For many decades, much of the methodology writings by economists were directed at convincing others or maybe themselves that economics is a science like any other. To do so involved two key essentials, one was an understanding of what other sciences are like and the other was an understanding of what philosophers of science thought about the other sciences. Too often, economists were satisfied obtaining only the latter and thus putting too much faith in philosophers. Things have gotten worse today because we now have professional methodologists of economics. Like their predecessors, they both rely too much on philosophers of science and understand little about what other scientists do. But unlike their predecessors, they know little about what economists do.

Philosophers of science in the first third of the twentieth century were standing on their heads trying to deal with Albert Einstein's revolution and the apparent collapse of the foundations of Newtonian physics. During most of the nineteenth century, philosophers of science were convinced that science had a solid inductivist foundation – that is, that the true theories of science were the result of careful observations followed by an expert application of inductive logic. But, had theories been developed this way, how could there have been room for the revolutionary ideas of Einstein? Was it not true that the laws of physics are immutable?

Before Newton's immutable laws of physics were put into question, almost everyone thought that finding true theories was the goal of every science. Moreover, almost everyone thought that scientific induction was the infallible method of doing this. Science was simply the organized application of the scientific method. But when it came to explaining the simple mechanics of magnetism on the movement of a compass needle, Newton's mechanics fell short. And rather than give up the faith in an infallible scientific method, nineteenth-century philosophers of science chose to move the goal post. Rather than aim for true theories of science, scientists were characterized as aiming for the 'best' theories. At least, it was thought, this would be an achievable goal.

Much of philosophy of science even today is concerned with the logic of choosing the best theories. In the 1930s, it was believed that scientific method was directed at choosing the theories that can logically be verified, that is, proven true with 'positive' observations made after the creation of the theory – the view that became known as logical positivism. According to this view, a scientific theory was distinguished from religion or other metaphysics on this basis alone.

Karl Popper entered the scene by challenging the view that the goal of science was to create a stable scientific atmosphere of agreement over what was considered the best theory available. Popper thought he was extending Einstein's view – which was that science is never stable but always in a state of constant revolution. And Popper's reason for this was that science was an enterprise of coordinated criticism rather than coordinated agreement. Practicing what he preached, Popper pounded his fists on the doors of the logical positivists in Vienna trying to convince them that they were going down the wrong path. Their path involved a logic of probabilities where the 'best' theory is the one that can be shown to be the most probable theory given the positive evidence made available by inductivist scientists. Popper argued that this would not be very interesting science and instead scientific theories are interesting because they appear at first to be the least probable explanations of positive evidence.

While Popper was being shunned by the philosophers of the day, an economics scholar, Terence Hutchison, thought he would take up the challenge in 1938 by arguing that what made scientific economic theories interesting was not that they are verifiable but that they were 'testable'. He specifically gave credit to Popper for this view. Unfortunately, Hutchison did not completely understand what Popper was saying. Moreover, Hutchison's view was pretty much ignored in economics. Instead, anyone writing on methodology at that time continued the logical positivist line that verifiability was the true test of a scientific theory.

Despite there being much talk about testability in economics in the 1960s, none of this had to do with Hutchison's path-breaking view of methodology. Instead, the 1940s and 50s were the battle ground for the movement to make economics a mathematical science. A main criticism of mathematical economics was that mathematics could only provide tautologies – which were claimed to be statements or theorems that are true by virtue of their logical form rather than their empirical content. More correctly, a tautology is a statement which does not depend on the definition of its non-logical words but only on its logical words such as 'and', 'or', 'is' and 'not'. Thus, a tautology is a statement which is true simply because one cannot conceive of a counter-example. For example, the statement ‘I am here or I am not here” is true regardless of who ‘I’ am or where ‘here’ is.

At the time of Hutchison’s launch of testability-directed methodology, Paul Samuelson was beginning to write his Ph.D. thesis that promoted the mathematical basis for all economic theory. And Samuelson directly confronted the critics by saying that his version of mathematical economics could not be dismissed as a bunch of tautologies because he would require economic theorems to be testable and thereby conceivably false. For Samuelson, a testable theorem is ‘operationally meaningful’ by which he merely meant that it must be ‘refutable in principle’. To be refutable in principle, a theorem could not be a tautology. QED.

During the 1940s, and before Samuelson’s thesis could be finished and published, economics came under attack by philosophy-armed critics who demanded, as a matter of proper philosophy of science, that economic explanations must be based on verifiable assumptions. In response to this, Armen Alchian, followed by Milton Friedman, launched a counter-attack directed at the logical positivist philosophy of science. Their counter view was that assumptions did not have to be verifiable or even true so long as they ‘worked’. This had echoes of the old battle between Bishop Berkeley and the promoters of Newton’s science. Supposedly, Berkeley’s fear was that if people believed in the things that made up Newton’s laws of physics, they would no longer see the need for religion to explain the universe. So Berkeley said he would allow for Newton’s science so long as its laws were considered mere instruments with no empirically verifiable existence in the universe. That is, the laws of physics are mere useful figments of our imagination – useful intellectual instruments. Alchian and Friedman saw that it was easier to side with the Bishop and thereby avoid the philosophical turmoil that was beginning to rear its ugly head in the hands of the critics.

This then was the current flowing through the writings on methodology during the 1950s and early 1960s. Almost all of the debate was about Friedman’s defense of instrumentalism which to many seems dishonest or simply wrong headed. Those who wished to promote mathematical economics were dismayed by Friedman’s instrumentalism and set about criticizing it on perceived logical grounds. For the most part, Samuelson simply made fun of Friedman, trying to eliminate him with ridicule. And it seemed to work for most of us, and in particular, for those of us trained to be mathematical economists.

At about the same time as Samuelson was putting down Friedman in the annual meetings of the American Economic Association in the early 1960s, Richard Lipsey and Chris Achibald were, to use the words of Chris, ‘building bombs in the basement’ at the London School of Economics. They were under the tutelage of one of Karl Popper’s students, Joseph Agassi. At first they thought they would build a new empirically based economics using Popper’s views of the philosophy of science. Like Hutchison before them, they did not quite understand what they were being told. They thought that economics could be made empirical (as opposed to mathematically tautological) by promoting an econometric approach that stressed the need for ‘falsifiable’ research. Their bomb construction yielded only one significant work, namely, the first edition of Lipsey’s famous textbook where Popper’s view was openly promoted. Their project was soon dropped because they found that falsifying econometric propositions was not very easy and sometimes impossible. Popper’s view played no role in subsequent editions and thus was soon forgotten. And, both Dick and Chris jumped on the bandwagon of the critics of Popper by promoting what Popper called conventionalism. Conventionalism is the defeatist alternative to the Inductivism that dominated the nineteenth-century philosophy of science. In the modern version of conventionalism, falsifiability rather than verifiability was now to be the watchword of science. And when economists of the 1970s and 80s talked about the need for testability and falsifiability of their models and theorems, they were implicitly talking about Samuelson’s methodology pronouncement and not Lipsey’s weak moment at the beginning of his first edition.

In fact, during the late 1960s and all of the 1970s, hardly anything was said about methodology. And I can testify that it was very difficult to get journal editors to even consider publishing methodology and thus very little was published. The only consistent exception was the last chapter of the various editions of Mark Blaug’s history of thought textbook. As early as 1968 Blaug was promoting falsifiability in his history of thought book as a test of true science – but at that time he seemed to be unaware of Popper until the mid-1970s. Unfortunately, Blaug then made the same mistake as Lipsey and Archibald by thinking Popper was promoting falsifiability as the essence of his theory of science. So, Blaug began complaining that economists talk about falsifiability but never practice it. He seems never to have recognized that economists were never trying to fulfill some sort of Popperian methodology but were instead simply invoking testability and falsifiability as a Conventionalist criterion to choose the best model or theorem in the way recommended by Samuelson – that is, in a way that insulated mathematical economics from the charge of being merely a bunch of tautologies.

Blaug and his followers were misled mostly by Imre Lakatos who, in a self-promoting way, tried to claim the mantle of Popper even before Popper died. Lakatos did not know much about science but he did know a lot about mathematics. As a result, Lakatos tried to formalize methodology with what he called 'the methodology of scientific research programs' (he was using Agassi's terminology). It is not clear that Lakatos understood Popper's reasons for talking about falsifiability – namely, as a sufficient but not necessary condition for criticism. Popper called his approach to explanation 'critical rationality'. Lakatos also misled economists by his twisting Popper's view to overemphasize its growth of knowledge implications. This was unfortunate because such emphasis encouraged historians of economics to follow Blaug's lead and start talking about methodology only in terms of 'progress' and 'progressive' research strategies that Lakatos promoted. In all of this, Popper was maligned and Lakatos praised.

Blaug chose to spin off his final chapter to make a freestanding methodology book in 1980. The obvious success of this book challenged the reluctance of other publishers. There soon was a mad scramble to find authors to write books on economic methodology. The editor for one publisher, George Allen and Unwin, took the first step by commissioning me to write my 1982 book and simultaneously by agreeing to publish Bruce Caldwell's Ph.D. thesis. The following two decades have witnessed a very active development of a methodology subdiscipline in economic methodology with now two well-established journals backed by two major publishers. Unfortunately, until quite recently, almost all of the publications in these two decades have tried to turn the clock back to the 1930s problems and questions that continue to interest philosophers rather than address the methodological issues that are of interest to mainstream economists.

Methodology as a separate subdiscipline of mainstream economics has shown the developmental signs of youth and adolescence. It would still be floundering in the basement had it not been for the efforts of two leaders of the History of Economics Society, Warren Samuels and Mark Perlman. Together, they encouraged historians of economic thought to make room in their annual meetings for sessions explicitly on methodology. Critics might easily say that this was a big mistake to tie one's dingy to a sinking ship. While when I was a Ph.D. student in the 1960s, history of thought was a required course but over the last two decades, it has been difficult to find a history of thought course – let alone a required course – in any major economics program. Nevertheless, methodology has found a viable place at least in the published literature if not the curricula.

Over the last two decades there have developed at least four camps. The biggest is made up of those methodologists who approach the subject with the interests of the historian of science. This camp spent most of the 1980s exploring how they might apply their understanding of Lakatos to the history of economic thought. As a consequence, there are many articles about 'appraisal' of economic theories and methods. And thus there is much discussion of negative or positive 'heuristics', 'hard cores', 'protective belts' and 'novel facts'. For the most part, this kind of discussion, particularly that concerning the 'hard cores' of research programs, was nothing more than a replacement for the 1970s fascination with Thomas Kuhn's 'paradigms'. All of this Lakatos-inspired methodology literature has at best been a waste of time. At worse, it became of stalking horse for critics of Karl Popper view of science. Unfortunately, Lakatos did not understand Popper but, nevertheless, these critics were thrilled to have the Lakatos-created cartoon-character of Popper to bash away at. Of particular concern was the identification of Popper's view with so-called 'falsificationism'. Lakatos was responsible for this characterization of Popper and it is a false characterization that continues to be promoted in history of economic thought circles by Blaug and his followers.

The fastest growing camp is the least serious. It began with a group who became bored with the grinding that goes on in the Lakatos-inspired methodology literature. To overcome the boredom there is now an eagerness to create and pursue buzz-words and fads. In the mid-1980s, the fads were concerned with finding an alternative philosopher of science one could quote to create and demonstrate an independence from the 'old' views. In the late 1980s, the new fad was so-called 'recovering practice' which supposedly was directed at understanding how economists practice their trade rather than how they should practice it. But this too became boring. Another group subsequently tried to get everyone interesting in deciding between whether the practice of economics is concerned with 'realism' or just a 'social construction' and thus relativist. More recently the fad has been about examining whether or not models are 'mediators', whatever that means. This too is beginning to get boring. It is difficult to take serious the frequent gathering around the latest fads in order to hold conferences about them. It may make all the eager conference participants feel like they are doing something – something 'new' – but it is hard to take seriously any study of methodology that takes a back seat to the immediate social needs of conference participants.

The next camp is driven by the interest of analytical philosophers who still worry about the problems and questions raised in the 1930s. And they are still licking the wounds inflicted by Popper. Their main hope is to eliminate Popper from the scene. But the main problem with this camp is that none of them have anything more than an elementary understanding of mainstream economics. While other philosophers are thrilled with each publication from this camp, mainstream economists ignore them completely. After all, it is the concerns of this philosophy camp that Friedman’s methodology intentionally addressed and provided economists with a reason to ignore the philosophers of the 1930s. Today, it is McCloskey’s emphasis on rhetoric that has replaced Friedman, but the message and purpose is the same, namely, to give reason to ignore this philosophical camp. McCloskey’s main argument is that the philosophical camp is concerned only with big-M methodology whereas ordinary economists are concerned with small-m methodology.

The fourth camp is very small, namely me and a couple of my students – although there are signs that it may be growing. This third camp is concerned mostly, maybe exclusively, with small-m methodology from a real Popperian perspective. Popper enters the scene by our viewing every social event, including scientific decisions, as problem solving ploys. The activity of this fourth group is sometimes criticized for being ‘always the same’ but such criticism may merely reflect a concern for big-M methodology by methodologists who do not understand the ever-changing practice of economics and economic model builders that is the primary domain of the small-m methodologist.

My interest in methodology from the beginning was in examining the reasons why economic model builders assume what they assume. I wrote my thesis to critically examine the testability allowed by certain common assumptions of the day. If anyone is interested in what I found, they can check Chapters 2 and 3 of my 1989 book. What I showed was that almost all but the simplest Keynesian models are untestable as they would require more data than is practical or possible. A simple Keynesian model with three endogenous variables, one exogenous variable and six exogenous coefficients would take almost 500 observations to construct a logically sufficient refutation. And, with one of my senior advisor’s simple macro models with just six endogenous variables, one exogenous variable and seven exogenous coefficients it would take over 24 thousand observations. And worse, any model with a Cobb-Douglas production function might take over 475 thousand observations. For those model builders who really think they are saying something significant by claiming their models are testable, I think it really shows what I said before. Testability is sought only to avoid tautologies and has nothing to do with whatever Lakatos thought Popper said about falsifiability.

What Popper did say was that if you think observations matter, as a matter of logic, only observations that might be used to falsify a theory can be decisive. Confirming observations can never be decisive except in trivial situations. Testing by attempting to falsify someone’s theory or explanation is just one of many types of criticism. And it is criticism or more specifically, a critical attitude that is the hallmark of science. It is not empirical falsifiability as both friends and foes of Popper seem to think he was saying.

The small-m approach to methodology that I have been practicing for thirty-five years does not interest philosophers and that’s ok, of course. But it does interest economists. Moreover, I was pleasantly surprised at the HES meetings in Vancouver a couple years ago when even the methodologists started talking about the methodology of economic model building and stopped talking about topics such as ‘realism’, ‘progress’, ‘falsificationism’ and similar things that philosophers like to talk about. Today it is becoming clear that methodologists can make a contribution to mainstream economics by helping to sort out and criticize the usual assumptions concerning an economic agent’s knowledge and learning. To do this, methodologists will have to give up the creation and pursuit of methodological fads and learn more about modern economic theory so that they can address the needs of practicing economists. For example, methodologists should surely be able to help the mainstream economist to realize that the time has come for him or her to stop assuming that induction is a reliable process of learning. To assume that it is reliable is, after all, to assume a theory of learning that is more than 360 years old and one that was refuted over 200 years ago.

It might seem strange, but as a practicing Popperian, I think it is time for methodologists to stop talking about Popper. It is all right for them to criticize Popper, but this is something for philosophers to worry about. Today, there surely are more important things for economic methodologists to do – particularly if they are willing to address the needs of the practicing economist.