Towards a useful methodology discipline

by Lawrence A. Boland

Much has happened in the field of economic methodology since my first methodology book was published in 1982. And much has not happened. When my first book was published there were only four economic methodology books in the bookstores. There was Stanley Wong's Cambridge Ph.D. thesis [Wong 1978], Ian Stewart's textbook that comments on econometric methodology [Stewart 1979], Homa Katouzian's book that tried to link methodology to ideological disputes [Katouzian 1980], and Mark Blaug's 1980 book that evolved from being the closing chapter of the earlier editions of his famous history of thought textbook. While some theorists found Wong's book interesting, Katouzian's and Stewart's books were mostly ignored in North America. Only Blaug's book had any impact on mainstream economists, and then only on those economists who wished to be able to say something about methodology. Blaug's book was nevertheless a major turning point for those of us interested in the study of economic methodology. Not only did a well-respected publisher publish his book but also the author was well known as the result of the success of his history of economic thought textbook. And the success of his methodology book prompted another leading British publisher to take the plunge into economic methodology. As a consequence, in 1981, Geo. Allen and Unwin's economics editor, Nicholas Brealey, chose to publish my methodology book and Bruce Caldwell's book on the history of economic methodology. With the publishing of our two books in 1982, the explicit academic discipline of economic methodology was born.

Today methodology sessions are a staple of every conference of the History of Economics Society in North America. In the last twenty years there have been many books and articles published that are exclusively devoted to economic methodology. But during the 1980s the promotion of methodology was limited to being a sub-section of the history of economics discipline. It is interesting to note that when the American Economic Association introduced an itemized list of books and articles in 1963 with its *The Journal of Economic Abstracts*, methodology was grouped with 'General Economics' and history of thought was a separate category. But when this journal was

renamed the *Journal of Economic Literature* in 1969, methodology and history of thought were grouped together by an ad-hoc committee of the AEA as a sub-category of '00 General economics; Theory; History; Systems'.

The combining of methodology with history of economic thought has been a major problem for me since my interest in methodology has always been the here-and-now practical methodology questions that face every theorist and model builder. The keystone question is: When building a model, what should be assumed and why? My interest has thus been the so-called small-m methodology rather than the big-M methodology that interests historians of economic thought and philosophers of science and economics.

During the 1980s and 1990s the issues addressed in the methodology conference sessions were mostly about big-M questions. Is there growth in economic knowledge? Is economics falsifiable? Do falsificationist economists practice what they preach? Is economics a form of rhetoric? Does economics have enough realism? Is Friedman an instrumentalist or a follower of John Dewey? Fortunately, the old warhorse big-M questions – such as 'Is economics a science?', 'Is economic value-free?', 'Should economics be so mathematical?' – had long ago been abandoned. Good thing, too, because such questions would never get a hearing in a mainstream economics department seminar. Unfortunately, the past tendency for methodologists to discuss such questions might be one reason why there are no methodologists teaching in the leading mainstream departments.

Parenthetically, it is interesting to note (as I did in 1987) there is virtually no overlap between the list of attendees at meetings of the History of Economic Society (HES) and the tenured teaching staff at any leading economics department in the US. Examining the list of candidates for executive offices of the American Economic Association for the year 2000 shows that only economists at six universities: California-Berkeley, Chicago, Columbia, MIT, UCLA and Yale are included. Comparing the list of schools represented on the AEA slate with the list of schools represented by participants at the Vancouver HES meetings shows less overlap than was evident at the New York meetings in 1986 where there was one from both lists, namely, from Columbia – the location of the meetings that year. Surely, if there is to be any progress in the development of the methodology discipline, there should be more overlap. No wonder the Vancouver session

that discussed the progress of methodology over the last twenty years was so disappointing to some participants.

In the 1980s and 1990s, seldom, if ever, were there methodology discussions of the decisions that model builders have to face. Rarely did anyone consider the small-m methodology questions such as: If one wanted to be sure that one's theory or model is falsifiable, what should be done? If one can offer an explanation for why prices are what they are, must one also explain why they are not what they are not? Are the presumptions of ordinary calculus consistent with the behavioural assumptions of the models we build? Are the assumptions concerning how individuals make decisions consistent with the methodology applied by the theorists explaining those decisions? Are microfoundations of macroeconomics necessary? Is there an inconsistency between the methodological individualism at the foundation of neoclassical economics and the view that market prices constitute needed social knowledge? Is there some fundamental reason why neoclassical economics seems unable to handle dynamic questions concerning the process of reaching an equilibrium or the growth of technology?

While my first methodology book sold well under the circumstances and continues to be cited, those new methodologists interested in the big-M methodology questions apparently find little of interest in that and my other books. But, judging by the two recent Vancouver conferences, one of the HES and the other of the International Network for Economic Methodology, the infatuation with big-M methodology seems to have come to an end. In Vancouver, there were even multiple sessions about model building. Whether this amounts to a belated endorsement of my advocacy of small-m methodology remains an open question. It is to urge such a movement that motivates my discussion here.

The futility of big-M methodology

It is understandable that historians of economic thought would be interested in big-M methodological issues. In the 1980s the big question among history-of-thought oriented methodologists was about whether there has been 'progress' in economics. How would one answer such a question? The obvious strategy to deal with this question was to consult philosophers of science and determine what criteria they would use. Is economics today more rigorous than past economics? More falsifiable? More testable? More

general? More useful? More realistic? And so on. That is, to answer the question, one needs some sort of quantitative measure to first evaluate or appraise the work of past economists. Then, one would use this measure to appraise subsequent work to determine if it represents a higher level of achievement.

Unfortunately, the big-M question of progress always seems to lead to futile debates. Not only will people dispute anyone's choice of a criterion of progress (usually one advocated by one or more philosophers), but there will also be endless debates over the superiority of one's favourite philosopher over another's chosen favourite. Should one rely on the views of Kuhn or Popper? Maybe, Lakatos is better. Maybe Hacking and Bhaskar are worth considering instead.

There are other big questions that involve methodological choices but they are less obvious. Every history of economic thought textbook writer must face the question, which economists should be discussed? And most important, how should they be discussed? Should one focus on the personal lives of the economists in question? Or should one focus on broad schools of thought and then only note the contributions of those economists towards the advancement of the schools' research programmes?

If one were to follow the traditions of the history of natural science, one might opt for the 'personal lives' approach if one believed that scientific method is inductive as it is supposedly based on Bacon's seventeenth-century philosophy of science. Such a history of thought would extol the personal virtues of the scientists in question, demonstrating how pure-in-heart, unbiased, clear-headed, careful, etc. they are. The implicit reason for examining personal lives is that science provides true theories that have been arrived at by first collecting data that can be considered facts. Without reliable facts, inductive scientific method would be useless. In inductive science, reliability is a personal matter. I think, today, few historians of economic thought would choose to follow this tradition.

Instead, a typical historian of economic thought might opt for the more modern view of science which followers of Karl Popper call conventionalism. According to this view, science is a continuing enterprise run by a community of likeminded researchers who produce and develop ideas. Thus the historian of thought talks about specific subjects and identifies the contributors to the development of these subjects. The development would be seen to be along a historical continuum. Whether or not one has made a contribution is not dependent on the personal lives of the researchers but on whether their research meets

the conventional standards of the day. Thomas Kuhn's history of science is the paradigm of this approach to the history of thought, although Imre Lakatos offered a useful competitor as is evident in many discussions of 'scientific research programmes', 'negative and positive heuristics', 'hard cores', 'novel facts', 'degenerating and progressive programmes', etc.

Again, my point is that historians of economic thought, whether explicitly or implicitly, must consider big-M questions. The only big-M methodology question ever considered by practicing economic theorists and model builders is the one about testability. However, these economists rarely consider testability to be a measure of anything but merely a litmus test. I doubt many mainstream theorists have any idea of why they should be concerned with testability. The big-M question about which philosopher of science practicing economists ought to listen to will not be raised in mainstream economics departments. The *only* methodology questions of interest in mainstream departments will be about modeling techniques. These are small-m methodology questions.

Is there a future for small-m methodology?

There would seem to be some promising developments in small-m methodology. At the Vancouver meetings there was much discussion of a recent book, *Models and Mediators*, edited by Mary Morgan and Margaret Morrison [1999]. Having examined this book, I am not sure it represents much progress. The book includes essays about modeling in natural science and in economics and econometrics. Of the ten contributors there are six who do research in the history and philosophy of science and four in the history of economic thought. Only one of the ten papers comes close to providing small-m methodology – but it is a start.

Recently, two conferences were held that tried to build bridges between methodologists and practicing economists. The first in 1996 brought philosophers and economists together to discuss some case studies [Backhouse and Salanti, 2001a]. According to one of the conveners, there was little useful common ground. Mostly this was because the case studies were responding to questions coming from philosophy, not economics – which I would describe as methodology for methodology's sake. The second conference was about macroeconomics and took place in 1998 and made a more

concerted effort to engage practicing economists with methodologists [Backhouse and Salanti, 2001b]. I have not seen the volume that was produced by this conference, but according to one of its editors, the conference focused more on small-m technical methods rather than big-M questions that might interest philosophers of economics. By all indications, this sounds like a much better start.

Suggestions on where to go from here

Most economic model builders today routinely – but unknowingly – deal with small-m methodological questions. Let me identify a few that I see as important fields for methodologists to plow. Most are ones that I have addressed over the last thirty years but that still need much more work. All of these suggested fields provide small-m questions that methodologists can ask their non-methodologist colleagues, especially those colleagues who are busy building economic models.

Knowledge and Learning: The one decision concerning economic models where methodologists should be able to assist their model-builder colleagues is the question of what one assumes about the knowledge and learning of the agents being modeled. How do the agents learn from errors? How do the agents know they are not maximizing? For that matter, how do they know they are maximizing? Typically, model builders either assume perfect knowledge (hence no learning) or they model learning by assuming the agent has employed some sort of inductive logic to acquire the needed knowledge. Either way more methodology questions are begged than answered. These model builders need help.

Disequilibrium awareness: Almost all mainstream models have at their core the identification of a state of equilibrium. The nature of that equilibrium is the consequence of specific assumptions made in the model. Since Alfred Marshall's Book 5, much of economics is about the neighbourhood properties of the equilibrium state. For example, if the price goes up causing a disequilibrium, what then must happen for the next state of equilibrium to be reached? Without an explanation for why the price went up, the analysis is limited to the mechanical properties of the model. If the model is to be more than an exercise in mechanics, the model builder must specify how the agents become aware of the disequilibrium and then how they deal with it. In the 1960s and 1970s many theorists tried to deal with this modeling problem, but little progress was made. A small-

m methodologist should be capable of offering constructive criticism to those model builders who continue to focus all of their attention on the state of equilibrium without ever coming to grips with the question of how the equilibrium is reached.

Does the existence of multiple equilibria pose a problem for economic explanations? Some model builders do address the need to explain the process of reaching an equilibrium but they still run into difficulties. Today, the most common modeling technique is based on game theory. To a certain extent, game theory allows an explanation of the process of reaching an equilibrium. But, as most proponents of game theory recognize, too often the constructed models allow for multiple equilibria and game theory seems incapable of explaining why one equilibrium state is obtained rather than one of the others. Again, a small-m methodologist should be capable of offering constructive criticism of those model builders who continue to focus all of their attention on the state of equilibrium without ever addressing the explanatory problem of multiple equilibria.

What are the limitations of game-theory modeling and how can they be overcome? In addition to the problem of dealing with multiple equilibria, there is the overall question of where the rules of the game come from. There is very little discussion of this question in the game-theory literature. It remains the dirty little secret problem that model builders seem reluctant to address. Small-m methodologists could provide some helpful prodding. Without addressing this question, it is not at all clear what is accomplished by devoting so many resources to game theory.

What are 'stylized' facts and what is their role in the model-building process? Another limitation of game theory models is that they seldom deal with empirical data directly. Those models that do deal directly with empirical data are usually based on econometric theory. But, econometric models make use of more fundamental theoretical models. All too often, models are designed to 'explain' so-called stylized facts rather than any readily observable data. What do builders of theoretical models mean by the term 'stylized facts'? Kaldor introduced the term as a realistic common ground to compare competing explanations, specifically, for a competition between theorists of MIT vs. those of Kings College, Cambridge. But today's model builders seem to mean something different, namely, something that everyone must explain but nobody thinks represents the real world. So, if the role is not to be the formation of a realistic common ground for debate,

what is the role for 'stylized facts'? The small-m methodologist should be capable of helping the model builders to answer this question and thereby help them understand what they are doing.

I have provided just a few of the small-m questions that have interested me. Surely, these and other similar questions are ones that small-m methodologists can pursue in an effort to help their model-building colleagues.

Concluding remarks

One important characteristic of small-m methodology is the absence of any need to appeal to the authority of one's favourite philosopher of science. While big-M methodologists strive to be seen as the high priests of economics, small-m methodologists strive to be seen as useful as plumbers. One implication of this humble approach to methodology is that the practice of small-m methodology is usually on a case-by-case basis. As such, there is little reason for historians of economic thought to show much interest. So, if there is to be an award for pursuing small-m methodology, it will not be one involving numerous invitations to international conferences. Instead, it might just be the everlasting gratitude from one's non-methodologist colleagues.

Bibliography

Backhouse, R. and Salanti, A. (eds) [2001a] *Macroeconomics and the Real World, Volume 1: Econometric Techniques and Macroeconomics*, Oxford: Oxford University Press.

Backhouse, R. and Salanti, A. (eds) [2001b] *Macroeconomics and the Real World, Volume 2: Keynesian Economics, Unemployment and Policy*, Oxford: Oxford University Press.

Bhaskar, R. [1978] *A Realist Theory of Science***,** 2nd edn, Brighton: Harvester.

Blaug, M. [1980] *The Methodology of Economics: Or How Economists Explain*, Cambridge University Press.

Boland, L. [1982] *The Foundations of Economic Method*, London: Allen & Unwin.

Boland, L. [1986] *Methodology for a New Microeconomics: The Critical Foundations*,

London: Allen & Unwin.

Boland, L. [1989] *The Methodology of Economic Model Building: Methodology after Samuelson*, London: Routledge.

Boland, L. [1992] *The Principles of Economics: Some Lies My Teachers Told Me*, London: Routledge.

Boland, L. [1997] *Critical Economic Methodology: A Personal Odyssey*, London: Routledge.

Caldwell, B. [1982] *Beyond Positivism*, London: Allen & Unwin.

Hacking, I. [1983] *Representing and Intervening*, Cambridge: Cambridge University Press.

Katouzian, H. [1980] *Ideology and Method in Economics*, New York: New York University Press.

Kreps, D. M. [1990] *Game Theory and Economic Modelling*, Oxford: Clarendon Press.

Kuhn, T. S. [1962/1970] *The Structure of Scientific Revolutions*, 2nd edn 1970, Chicago: University of Chicago Press.

Lakatos, I. [1970] 'The methodology of scientific research programmes', in I. Lakatos and R. Musgrave (eds), *Criticism and the Growth of Knowledge*, Cambridge: Cambridge University Press.

Morgan, M. and Morrison, M. [1999] *Models as Mediators*, Cambridge: Cambridge University Press.

Stewart. I. [1979] *Reasoning and Method in Economics: An Introduction to Economic Methodology*, London: McGraw Hill.

Popper, K. R. [1934/1959] *The Logic of Scientific Discovery*, London: Hutchinson & Co.

Wong, S. [1978] *The Foundations of Paul Samuelson's Revealed Preference Theory: A Study by the Method of Rational Reconstruction*, London: Routledge & Kegan Paul.