

Review Essay

Dealing with Popper in Economic Methodology

LAWRENCE A. BOLAND
Simon Fraser University

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In the late 1930s there began a full-court press to convince economists that they needed to do their economic analysis using mathematics. But the proponents were facing a problem: many reluctant converts were countering with the methodological claim that mathematics was nothing but tautologies. Tautologies and ‘analytical truths’, they said, were a typical fascination of economic cranks and quacks. At about the same time Paul Samuelson and Terence Hutchison advocated a methodological rule to avoid tautologies that would solve their problem. The rule was that every economic theorem or hypothesis must be empirically falsifiable. Samuelson’s 1941 Ph.D. thesis actually began with the methodological rule that economists should seek ‘operationally meaningful theorems’ but subsequently defines this type of theorem as ‘simply a hypothesis about empirical data which could conceivably be refuted’. He went on to say that a ‘meaningful theorem may be false’. And above all, his Ph.D. thesis [Samuelson 1965] clearly demonstrated that ‘meaningful theorems’ could be derived from his mathematical analysis of standard economic theory. For Hutchison [1938], the issue was whether economic statements are intendedly scientific and thus he advocated the methodological rule that they must be capable of being submitted to ‘intersubjective test’ or in short, they must be ‘testable’. Both authors seemed to be convinced that such a rule would assure that economic theorems and hypotheses would not be tautologies. Hutchison’s purpose was explicitly to embargo both the alleged ‘a priori’ economics of economists such as Lionel Robbins or Ludwig von Mises and the dangerous ‘scientific’ ideologies that were rampant. Samuelson’s purpose was apparently limited to defending mathematical economics from the usual criticism.

Given that methodological rules are the domain of philosophers, you would think that both Samuelson and Hutchison would have bolstered their advocated rule with the pronouncements of some famous philosopher. Hutchison explicitly invoked what he understood to be Karl Popper’s view of testability and falsifiability as expressed in the original 1934 German edition of the *Logic of Scientific Discovery* [*LScD*]. Interestingly, Samuelson made no mention of a philosopher.

Since Hutchison made an explicit reference to Popper’s work in 1938, most economic methodologists today credit Hutchison with the introduction of Popper to economists [RWR, pp. 49ff.]. But they are wrong. Hutchison does not present Popper’s critical rationalism but instead his own out-of-the-context methodological rule of falsification. Despite the opinion of some methodologists, Hutchison’s references to Popper were hardly noticed in economics [RWR, p. 88]. Instead,

throughout the 1940s and 50s, the discussion of falsifiability and testability was limited to only the methodological concerns exclusively of mathematical model builders who were quite satisfied with Samuelson's rule since it allowed them to avoid the criticism that mathematics provides only tautologies. Specifically, Samuelson recognized that as a matter of logic, while one cannot conceive how a tautology can be false, his 'meaningful' theorems can be false – hence they are not tautologies. The rule of testability solved his methodological problem.

In the 1950s, almost all of the research energies of the leading economics departments were devoted to promoting mathematical economics. Samuelson was the master builder of mathematical economic models and so his methodological pronouncements and rules carried the day. With the exception of Hutchison's 1938 work and some underground activity at the London School of Economics, Popper's view of science was rarely if ever invoked to support the methodological rule that economic theorems must be falsifiable.

If methodologists want to recognize who introduced Popper's view of science to economics readers, they should point to the 1959 *Economica* article co-authored by the economist Kurt Klappholz and the philosopher Joseph Agassi. Unfortunately, critical rationalism² – the main message of the Klappholz and Agassi article – seems to be missed by almost all economic methodologists, even those who see themselves as friends of Popper's view of the philosophy of science. Specifically, commenting on then common views of economic methodology and the alleged 'slow progress in economics', Klappholz and Agassi observe that many defenders of economics think 'that, if only economists adopted this or that methodological rule, the road ahead would at least be cleared (and possibly the traffic would move briskly along it)'. To which they added,

Our view, on the contrary, is that there is only one generally applicable methodological rule, and that is the exhortation to be critical and always ready to subject one's hypotheses to critical scrutiny. Any attempt to reinforce this general maxim by a set of additional rules is likely to be futile and possibly harmful. [1959, p. 60]

On this basis they proceeded to criticize various authors who dared to promote methodological rules – including Hutchison. In doing so, they say 'Our criticism will be based on our methodological view, which is outlined in K. R. Popper's *LScD*'. Let me stress their point again: *Popper promoted no rules except to be critical*.³

Certainly, they never promoted the so-called '*Popperian* falsificationism', which so many economic methodologists today are so fond of promoting or bashing [see RWR, pp. 276–86]. Even the economist Mark Blaug, today's leading promoter of so-called 'falsificationism', did not mention Popper in the 1968 edition of his famous history of thought textbook – even in its final chapter where he dared to openly discuss economic methodology. He did, however, list the Klappholz and Agassi article as 'further reading' on the explicit grounds they 'attack *the* methodological fallacy, namely, the notion that the progress of economics can be accelerated by laying down methodological rules for the pursuit of economic research' [1968, p. 683, emphasis in original].

Parenthetically it should be noted that '*Popperian* falsificationism' has rarely been explicitly defined in economic methodology literature. One exception is Hands [1993, p. 62], which defines it as being 'composed of two separate theses: one on demarcation (demarcating science

from non-science) and one on methodology (how science should be practised)'. He then goes on to say it proceeds in a rule-based, step-by-step fashion, namely, the 'scientist starts with a scientific problem situation (something requiring a scientific explanation) and proposes a bold conjecture which might offer a solution to the problem. Next the conjecture is severely tested by comparing its least likely consequences with the relevant empirical data. ... The final step in the falsificationist game depends on how the theory has performed during the testing stage...'. In RWR he simply says, 'Ask almost any philosopher about Karl Popper's work and one will inevitably receive a recitation about bold conjectures and severe tests; and, I would add, one would get essentially the same lecture from any economist who recognized the name. Falsificationism is what most philosophers mean by "Popperian" philosophy of science, and falsificationism is what most economists mean by "Popperian" economic methodology' [RWR, pp. 276–7].⁴

Today, falsificationism is the methodological rule that Blaug demands to be followed [1980, 1992, 1994; see RWR, p. 278]. Blaug's failure to mention Popper in his 1968 textbook's promotion of 'falsificationism' can be explained. He says that he learned falsificationism indirectly by way of Milton Friedman's 1953 methodology essay where there was no mention of Popper but there was a sort of 'vulgar, Mickey Mouse Popperism' [1994, p. 22]. By 1974, Blaug had become a born-again Popper promoter – albeit in the distorted rule-based form that was created by Imre Lakatos [1970]. Blaug and his many disciples now see so-called 'Popperian falsificationism' to be the methodological rule that all economists should now follow. Clearly, the Popper outlined in the *LScD*, according to Klappholz and Agassi, is neither the Popper promoted by Blaug (following Lakatos) nor the one suggested by Friedman.

While it is all too easy to blame Lakatos for the distortion of Popper's view of methodology (and with good reason [see Agassi 2002]), many mainstream philosophers of science have also contributed to the distortion. Almost without exception today, if you dare mention Popper's name in a philosophy seminar, the reaction will almost always be 'oh, you mean Karl Popper the falsificationist' – they will rarely recognize 'Popper the critical rationalist'. This is particularly evident when trained philosophers of economics such as Daniel Hausman [1988] discuss Popper [RWR, p. 279–80]. And Hausman is not unusual among philosophers. So, why is it that trained philosophers cannot understand Popper? One reason might be that they all take for granted the methodological presupposition which Popper called 'justificationism' [1965]. That is, they all, without question, presume that all 'beliefs' or claims to knowledge must be 'justified' – i.e., proven either analytically or empirically. Since rejecting justificationism is central to Popper's view of methodology, it is no wonder that analytical philosophers fail to understand Popper – nevertheless, their failure is at least curious since it is easy to see that Popper's view of methodology is almost the same as that of the Socrates of the early Plato dialogues. But, of course, it is not certain that all analytical philosophers actually understand that Socrates. But I digress.

For those of us who are not trained in analytical philosophy, Popper's antijustification is easy to understand.⁵ After all, claims to knowledge such as the claim that 'all men are mortal' may legitimately defeat any criticism, especially if they happen to be true. So, why is it that trained philosophers cannot see that this is enough?

THREE FALSE (BUT EFFECTIVE) APPLICATIONS OF 'FALSIFICATIONISM'

It may be easy to dismiss Hutchison's quick endorsement of what he was to call the 'falsifiability principle' since, according to Klappholz and Agassi, he misunderstood Popper. Nevertheless, the falsifiability principle was an effective solution for a political-ideological problem that Hutchison thought needed to be solved. According to Hutchison, in the 1930s, the 'barbarians really were at the gate...' [1988, p. 25]. Specifically, he said the 'barbarians ... that I was concerned about were the Nazis. They had a pseudo race science which I saw taught in German universities' [1988, p. 36]. His falsifiability principle also directly confronted Robbins [RWR, pp. 34–7] whose methodological pronouncements and 'self-evident' economics dominated much of economics teaching until the early 1960s and was, in particular, center stage at LSE where Popper independently held forth in philosophy [de Marchi, 1988a].

From a different perspective, the LSE opponents of Robbins' methodology, including the economists Richard Lipsey and Chris Archibald, considered his teaching to be a long-standing pedagogical problem. In the late 1950s, they set about 'making bombs in the basement' to try to overthrow Robbins.⁶ They learned a little bit about Popper's view of methodology and apparently decided it would make a good explosive agent for their 'bomb'. Their bomb making took place at their weekly seminar and led to many attempts to apply their understanding of Popper [see de Marchi 1988a, p. 163–6]. But their understanding of Popper was much like Hutchison's. Only Archibald seemed to understand the critical rationalism that Klappholz and Agassi promoted although he seemed to think that the only tool to use was what he called 'Popper's Demarcation Rule' [1966, p. 279]. Lipsey was intent on promoting the quantification of economics (because that is what Robbins opposed) and so the prescription that economics should be falsifiable and empirically testable would seem to solve the perceived pedagogical problem. Thus he set out in 1961 to write an economics textbook that would be based on the methodological rule he attributed to Popper – namely, a rule requiring testability and moreover requiring testing that should be directed toward refutation.

Lipsey's first edition was published in 1963 but by the time the second edition was published in 1966, his desire to follow what he took to be Popper's rule was abandoned in favour of just promoting econometrics-based testing which is more palatable than any testing that might be directed toward refutation. Archibald similarly found it difficult to practice his understanding of critical rationalism. He first tried to engage in critical rationalism by publishing a critique of the Chicago School's view of monopolistic competition [Archibald 1961]. But by 1964 he abandoned critical rationalism based on falsification in favour of critiques based on critical comparisons [1966].

The third domain of application of falsificationism is to be found in the field of history of economic thought. While Hutchison made references to examples of falsifiability in the history of the standard maximization hypothesis – namely, that over time the maximization assumption 'became less and less falsifiable' [1938, p. 115] – his main concern was the problem of 'unscientific' ideologies. Other historians of economic thought can see a slightly different problem that falsifica-

tionism can solve. For example, Blaug asks, ‘How can we study the history of science without some prior notion of what is a science and indeed what is good and bad science?’ [1994, p. 110]. In the preface to the second edition of his famous methodology book [1992, p. xv], he gives what he claims to be examples from the history of economics of ‘the use of falsificationism’ [1994, p. 111]. He also claims this is what Popper was doing, too, but seems to confuse examples of ‘the use of falsificationism’ with what Popper called examples of ‘refutations which led to revolutionary theoretical reconstructions’ [see Popper 1983, p. xxv].⁷ But despite the examples Blaug lists, he still complains that ‘modern economists do in fact subscribe to the methodology of falsificationism ... [but] fail consistently to practice what they preach...’ [1992, p. xiii].⁸

So, economists and historians of thought may find so-called ‘falsificationism’ to be a useful tool to combat all sorts of perceived evils even though in practice it may leave much to be desired in economics. But it should be pointed out that ‘falsificationism’ might also solve some interesting philosophical problems. For example, some natural scientists saw Popper as emphasizing falsifiability (not necessarily falsification) and this freed them ‘from a felt obligation to speak only of Truth’ [see de Marchi 1988b, p. 28]. Presumably then, one could justify one’s use of particular theorems on the basis that they were falsifiable and not necessarily on the basis that they were known to be true. Of course, this use of falsifiability runs counter to Popper’s opposition to ‘conventionalism’ – the methodological strategy that, rather than being concerned with whether or not a theory is true, scientists seek the ‘best’ theory where the criterion of ‘best’ is a matter of scientific convention [see Boland 1971, 1989 (chs. 4 and 5) and 1998]. In this sense, falsifiability is just one of many criteria (e.g., simplicity, generality, etc.) that might be used to determine the ‘best’ theory. And in all cases, such conventions are never claimed to be the means of establishing the truth of a theory. But conventionalism aside, Popper explicitly asks: ‘Am I really the man who had naïve falsificationism as the linchpin of his thought? ... It so happens that the real linchpin of my thought about human knowledge is fallibilism and the critical approach...’ [1983, pp. xxxiv–xxxv].⁹

TRUTH STATUS MATTERS BUT JUSTIFICATION DOES NOT

The original German edition of the *LScD* appeared 25 years before the English edition. When the former was published in 1934, some philosophical circles were absolutely convinced that science could be distinguished from non-science solely on the basis that truly scientific theories were empirically verifiable.¹⁰ But by 1959, when the English edition appeared, few philosophers continued to be so convinced as were many in the 1930s. And, when Popper (in Chapter 1 of the English edition) argued that Hume’s Problem of Induction is not solvable, by then most probably agreed [RWR, p. 85]. But, mainstream philosophers may not see this as a Problem of Induction but a problem *with* induction.¹¹ According to Popper, this is because induction was the hope of pre-Humean philosophers who thought induction was the means to justify their beliefs in science and scientific method. But, despite what Popper might have wished, dropping induction does not necessitate dropping the presumed need to justify.

As I have already noted, well-trained philosophers take for granted the need to justify one's claim to knowledge, so it is not surprising that those who recognize Popper at all think his discussions of falsifiability and refutation were somehow directed at the question of how one would justify (or by what criterion one would chose) a theory. That is, it is often presumed that he must be saying we should choose the most falsifiable. Unfortunately, justificationism blinds them from understanding Popper's point. Perhaps they do not understand Popper because they miss his polemics.¹² His challenging argument was simply that *if* 'you guys' (namely, the Vienna Circle philosophers he was trying to debate) think positive evidence matters in science,¹³ then you must recognize that as a matter of logic such evidence can be decisive only as refutations. Popper's reason was simple: every explanatory theory must include at least one strictly universal statement (e.g., 'all swans are white') and as such it could never be verified even if it were true – yet it could be easily refuted with just one piece of positive evidence (i.e., the observation of a non-white swan). With this argument Popper was not arguing for 'falsificationism' but for what he later called critical rationalism and this was clearly evident on the second page of the preface to the English edition of *LScD*.

SO, WHY THE EMBRACE OF FALSIFICATIONISM IN ECONOMICS?

During the 1960s and early 1970s Popper published several of his lectures which clearly stressed the priority of critical rationalism [e.g., 1965, p. 26] and his explicit denial of a scientific method that provides rules that would guarantee progress or justification [e.g., 1972, p. 265]. Agassi is quite direct: 'Popper almost alone, and alone in our century, has claimed that criticism belongs not to the *hors d'oeuvre*, but to the main dish' [1968, p. 317]. And critical rationalism also plays a central role in William Bartley's works of that period [1964a, 1964b and 1968].¹⁴ The role of falsification is clearly understood in the methodology of science that Popper outlined in the *LScD* only after understanding his critical rationalism. This was abundantly clear to all but the philosophers and economic methodologists who began proclaiming that 'falsificationism' was the primary contribution [RWR, p. 88] of Popper to methodology – but as I have said, this is easy to understand since the proponents of 'falsificationism' see it as a solution to their various methodological problems regardless of Popper's disavowal of such a contribution to methodology [e.g. 1983, p. xxxv]. The source of their distortion of Popper's views, and in particular their subjugation of critical rationalism, is clear. Specifically, it was the self-serving pronouncements of Lakatos and his attempt to hijack Popper's role in the philosophy of science. And it is Lakatos [1970] who ties the 'falsificationism' canister to Popper's tail much to the dissatisfaction of Agassi and Bartley.¹⁵

The first territorial claim for Lakatos in economics was made by Spiro Latsis [1972] at the end of his paper about the neoclassical theory of the firm. There Latsis added a gratuitous appendix on falsificationist methodology that had as its main purpose to distort the message of the Klappholz and Agassi article. We were directed henceforth to see Popper as a proponent of a failed methodology of falsificationism rather than a Socratic Popper who would make critical rationalism the basis to understand science. Of course, Latsis was a student of Lakatos [RWR,

p. 303, note 23] and thus was just taking Lakatos' side against the arch-enemies, Agassi and Bartley.

In 1975 Blaug published his first foray down the road of falsificationism mapped by Lakatos. Blaug's article was subsequently included in a conference volume of similar papers edited by Latsis [RWR, p. 287]. But Blaug does not seem to swallow the whole anti-Popper flavour of Lakatos and Latsis. Instead, he promotes falsificationism in much the same way that Hutchison does. And this falsificationist view of Popper is the one embraced in Blaug's very successful 1980 methodology book.

At about the same time as Latsis was promoting Lakatos, my student, Stanley Wong, published a methodology article in the *American Economic Review* [1973] that was intent on practicing Popper's critical rationalism. It was also incorporated in his Ph.D. thesis that was published subsequently in 1978. Both Wong's paper and his book embraced the Klappholz and Agassi article as the starting point for understanding Popper. However, it is easy for someone to make the mistake of thinking that it was a book going down the Lakatos-Latsis road – given its subtitle of 'A study in the method of rational reconstruction'. But, he makes clear in the preface that, on my suggestion, his book is an application of Popper's notion of rational reconstruction [1962, ch. 14] and thus throughout it is nothing less than an explicit application of Popper's critical rationalism.

The late 1970s then presented a fork in the road to anyone who wished to study economic methodology. One could have followed Wong down the Klappholz and Agassi road or instead traveled down the Latsis-promoted Lakatos road. At this time there were some young beginning economic methodologists who faced this choice including Wade Hands [1979] and Bruce Caldwell [1982]. There were also a couple of young mainstream philosophers (including Hausman) who decided that the philosophy of economics might be a fruitful place to ply their trade. They all chose to begin their travel on the Lakatos road where Popper is relegated to a mere 'falsificationist'. In so doing, there is no mention of what Popper called critical rationalism [e.g., Popper 1965, p. 26].¹⁶ Almost all of these beginning methodologists and philosophers of economics seemed unaware that there was another road to travel even though some of the early books they published began as their Ph.D. theses and thus had well researched bibliographies that included the Klappholz and Agassi article.¹⁷ Nevertheless, all began with the typical philosopher's notion¹⁸ that economic methodology must involve positions regarding the proper rule of procedure when developing economic theories or models.¹⁹ Obviously, the message of the Klappholz and Agassi article did not immediately register with all methodologists and philosophers of economics.

SEEING CRITICAL RATIONALISM AFTER SEEING FALSIFICATIONISM

In the early works of Hands and Caldwell, not only do they fail to discuss what Popper calls critical rationalism, it is clear they acquired their understanding of Popper straight from Lakatos [1970]. The evidence is, for example, that both refer to 'Popper's methodological falsificationism' [Hands 1979, p. 298; Caldwell 1980, p. 64, note 15 – see Lakatos 1970, p. 180]. While Caldwell seems to stop there, Hands goes on to talk about Popper's supposedly proposing a 'revolutionary convention-

alism' [Hands 1979, p. 296] but, again, this is just Lakatos [1970, p. 105]. And should we think in the case of Hands that this was just a youthful misunderstanding, in the 2001 book under consideration [i.e., RWR], he still talks about 'Popper's conventionalism' [RWR p. 92] even while recognizing that Popper opposes conventionalism.²⁰ Apparently, referring to the *LScD*, Hands explains this by saying that Popper 'admits to being a "conventionalist about the empirical basis" of science' [RWR p. 92, note 26]. This is a typical misreading of the *LScD*. In that book Popper is trying to debate the Vienna Circle on their terms and thus concedes what he thinks to be unimportant ground for the purposes of the debate. Reading more into this is unfortunate, particularly when Hands knows Popper explicitly rejects conventionalism.²¹

If one instead begins first by understanding critical rationalism and only then turns to understand falsification, falsification will not be seen to be such a big deal. Falsification is merely one of the means to an end. After all, one criticism of a proffered theory might be that it is unfalsifiable. So, the ability to criticize a theory does not require its fulfillment by rule of a prior condition of falsifiability. Instead, as Popper explained in 1963, 'The proper answer to my question "How can we hope to detect and eliminate error?" is ... "By *criticizing* the theories or guesses of others and – if we can train ourselves to do so – by *criticizing* our own theories or guesses." ... This answer sums up a position which I propose to call 'critical rationalism' [1994, p. 26, emphasis in original]. There are no rules for criticism other than, perhaps, one should be fair – and in a fair criticism one should also begin with a demonstration that one understands the ideas being criticized.

Alternatively, if one begins first with an understanding of the importance of falsifiability for the purposes of empirical criticism – but makes the mistake of assuming that this has something to do with a rule for proper conduct of science – then it is all too easy to say that given the Duhem-Quine thesis, Popper must be resorting to some sort of conventionalism to justify any claimed refutation [RWR, p. 98; see also Dow 2002, pp. 86–7]. That is, it is presumed that any claimed refutation of a theory amounts to a proof that the refuted theory is false – that is, supposedly that the theory has failed a test.²² But the Duhem-Quine thesis merely says that whenever a refutation is the result of a failed test, we must recognize that, since the performance of every test involves many auxiliary assumptions, one cannot be sure whether the fault is the added assumptions or the original theory in question. It is thus claimed that for Popper to recognize a refutation of any theory in question he must be accepting its auxiliary assumptions as true beyond question.²³ Of course, this is not a fair criticism of Popper since he says everything is open to question – even those assumptions that are put beyond question for the purposes of one's testing or research programme, that is, the programme's metaphysics are tentative and thus open to question whenever it is interesting to do so. To see this as a problem *for* Popper, one would have to take justificationism for granted.

Looking for ways to refute Popper's theory of science by first presuming justificationism will always be unfair. Unfortunately, this is how philosopher-critics deal with Popper. Economic methodologists have a different way. They focus on Popper's view of how social science should proceed – specifically, on his so-called Situational Analysis [see RWR, pp. 283ff.]. Specifically, Popper adopts a generalized version of neoclassical explanation as his theory of social science ex-

planation. In neoclassical economics there is only one behavioral assumption – namely, the assumption of constrained maximization or minimization – which economists often equate with being rational. For Popper the general behavioural assumption is called the Rationality Principle²⁴ but in his case it only means that a decision maker in question acts consistent with his or her aim. The theorist who explains the event that results from decisions made need only posit what the aim was and what the situation was that faced the decision maker. The combination of the posited aim, situation and the specified Rationality Principle constitutes the theorist's Situational Analysis. The most general case for this analysis is to explain a decision already made by seeing the decision as a solution to a problem. In this case, the theorist conjectures the problem and the conjectured Situational Analysis provides the logic for how the decision is thought to deliberately solve the problem.

Some economic methodologists claim that there is a fundamental problem with Situational Analysis [see Hands 1985]. The main claim is that it violates the rules of falsificationism – because, it is claimed, the use of the Rationality Principle is unfalsifiable [RWR, p. 284]. Of course, for this to be such a problem means first that one takes the rules of falsificationism as requirements. When one rejects falsificationism in favour of critical rationalism, the problem disappears. But, even so, the claim is that the only recourse is to assert that the Rationality Principle (or even Situational Analysis) is a matter of metaphysics. The typical implication drawn by such a claim is that the Rationality Principle must be tautological – but this is drawn on the grounds that mistakenly confuse tautologies with all unfalsifiable statements [see Agassi 1971]. But more important, the notion that this is a telling criticism presumes both that metaphysics is to be avoided and that when using Situational Analysis we can do this. This is a mistake, too – as was explained by both Agassi [1964] and Popper [1983]. The major point about metaphysics is that one's setting aside any statement or assumption in one's metaphysics is a matter of choice²⁵ – that is, a deliberate choice to put such an assumption beyond question and thus that choice is always open to criticism. In other words, metaphysical statements are not *inherently* unfalsifiable.

DEALING WITH POPPER IN THE 20th CENTURY

We see then that how Popper is dealt with in economics depends on which view of Popper is being considered – that is, is it the critical rationalist of the Klappholz and Agassi or is it the 'Popperian falsificationist' of Lakatos? Most of the criticism of Popper is actually directed at the 'falsificationism' that Blaug continues to promote and the one that philosophers such as Hausman like to complain about. Both are mistaken views of Popper. Blaug's view follows simply from his mistake of taking Lakatos as a reliable source for understanding Popper.²⁶ Hausman's is due to his mistake of taking justificationism for granted when considering Popper. Neither sees critical rationalism as the centerpiece of Popper's view of science.

Caldwell and Hands both claim to not make the mistake of ignoring critical rationalism. Hands is fond of repeatedly claiming that the 'falsificationist Popper' is 'the best known' or 'the standard' view of Popper's philosophy of science²⁷. Sometimes Caldwell seems to concur.

Both seem to be offering criticisms of the falsificationist Popper²⁸ but too often it comes off as if they are criticizing Popper's view of science. One could presume that this is a strategic maneuver so that if they are successful in their critiques of falsificationism, then perhaps economists will take critical rationalism more seriously. If so, there is little evidence that this has been a successful strategy.²⁹

POPPER AS 21st CENTURY SCIENCE THEORIST

This brings us to the latest approach to dealing with Popper. For some time I have thought that the proper way to deal with Popper is to make it clear that the 'falsificationist Popper' has nothing to do with the Popper promoted in the Klappholz and Agassi article, that is, with what I have identified as the Socratic Popper [Boland 1994]. Moreover, the 'falsificationist Popper' should be clearly identified as the Lakatos-Popper. But the latest attempt to deal with Popper in economics [RWR] takes a very different tack.

Hands' 2001 book [RWR] focuses on two ideas: first that the history of economic methodology was 'the movement from characterizing the method of economics as it contrasts with the different methods of other sciences in [John Stuart] Mill, to specifying rules for the proper conduct of any science, and thus economics, in [Lionel] Robbins' [RWR, p. 37, emphasis removed]. The second idea is that of 'contemporary science theory' [RWR, pp. 4–6]. It is actually difficult for the reader to focus on 'contemporary science theory' since its agenda or 'problem situation' [RWR, p. 5] sounds too much like ordinary philosophy of science without rules.³⁰ Hands spends the first two-thirds of his book showing how both the old idea of economic methodology as applied philosophy of science and the new idea of contemporary science theory are in disarray. His solution is

to change the subject, to reformat the debate, in a more viable and interesting direction. Although the narrow borrowed-rule-giving economic methodology is effectively dead, I believe this is a very fertile and productive period for work in a new more broadly defined field of economic methodology. [RWR, pp. 6–7]

In other words, his argument is that 'we simply abandon the narrow rules-based definition of economic methodology and redefine the field to be any literature that *substantively involves both economics and science theory*; on this definition economic methodology is not only alive, but alive and well' [RWR, p. 394, emphasis in original]. He would like us to call his newly expanded field 'the new economic methodology'.

The foundation of Hands' new economic methodology appears to be the same view of methodology that Klappholz and Agassi were promoting over forty years ago – specifically, the view that rules-based methodology is to be rejected [RWR, p. 396] and thus there is to be no mention of progress in the history of economic thought. In addition to recognizing that rules-based methodology is dead, the other lessons that Hands says we will learn from his new economic methodology include the realization that the shelf of philosophy of science is empty.³¹ In place of the empty shelf we are to realize that 'metaphysics matters'³² and 'pragmatism is back'³³ [RWR, p. 399].

An explicit mention of critical rationalism is avoided³⁴ but the book stands as a critique of the problem situation of traditional approaches to economic methodology. In other words, the new way to do methodology is to turn back the clock to the Popper available before the Lakatos

hijacking. The idea is to pick up where Popper [1963] left off. Specifically, Popper advocated a theory of social science that directly applied neoclassical economics in the form of his Situational Analysis. Hands would have us extend this with some new approaches to neoclassical economics by drawing on the techniques of evolutionary economics [RWR, p. 384], on old institutional economics combined with ‘neo-pragmatism’ [RWR, p. 385], on Hayek’s view of individual rationality in the context of social rule-following [RWR, p. 386] and on economic sociology [RWR, p. 387]. Window dressing aside, the bottom line seems to be that the new way to deal with Popper in economics is to simply incorporate his critical rationalism (i.e., his no-rules view of methodology) into Hands’ new economic methodology. But, apparently we are to avoid mentioning Popper’s name as it might put his critics and the neo-Popperians in a bad mood.

If I am correct to conjecture that Hands’ book is an attempt to apply critical rationalism to the philosophy of rules-based economic methodology, the main question is, I think, whether it will be an effective critique. This economic methodology book seems to be an attempt to do an internal critique along the lines that Popper traveled with his critique of 1930s logical positivism. To be effective, an internal critique must accomplish two main things. First, it must demonstrate to the proponents of the doctrine being criticized that the critic understands completely all aspects of the doctrine so that the proponents have no easy escape route. And then it must deliver a fatal blow without compromise. Hands does a masterful job of trying to demonstrate that he is fully aware of all the various avenues traveled by the Lakatos-Popperians as well as the old ‘Received View’ and its critics [chapter 3], ‘naturalized’ and ‘evolutionary’ epistemology [chapter 4], the ‘sociology of scientific knowledge’ [chapter 5] and the methodology of ‘pragmatism’ and ‘feminist epistemology’ [chapter 6]. This demonstration walks softly down Popper’s road by offering a tasty carrot but, unfortunately, his book lacks a ‘big stick’ and thus fails to strike a fatal blow.

NOTES

¹ Henceforth, I will refer to this book as RWR.

² Critical rationalism is merely the application of the ‘critical attitude’ that they promote throughout their article.

³ Does the title of this book [RWR] suggest that Hands is promoting Popper, the critical rationalist, of the Klappholz and Agassi article? It is not clear – particularly, since their article is mentioned only in the context of a discussion of my rejection of Popper’s alleged ‘falsificationism’ in favour of Popper’s critical rationalism [RWR, pp. 301–4].

⁴ It is probably significant that in this context there is never a mention of the critical rationalism that Hands says Klappholz and Agassi introduced with their 1959 article [RWR, p. 301].

⁵ Unfortunately, Hands wishes to confuse antijustificationism with antifoundationalism [e.g., RWR, p. 297, note 20]. With antijustificationism, everything is open to criticism and thus tentative but antifoundationalism opposes any tentative acceptance of assumptions as is commonly done in axiomatic analysis. The reason why some might object to foundationalism is that it is usually what Popper and Bartley call ‘fideism’ where the Church fathers simply decreed one and only one set of acceptable axioms. But for an antijustificationalist, so long as such fideistic axioms are still open to question, positing foundational assumptions for the purposes of discussion or debate is acceptable.

After all, metaphysics are foundational axioms but they are beyond question *only* by choice.

⁶ This is how Archibald put it in a letter to me about my review of de Marchi [1988b].

⁷ This is a common problem with references to Popper. The problem can be rather subtle, such as when Roger Backhouse quotes Popper talking about the ‘advance’ of science but then says Popper is talking about the ‘progress’ of science [1997, p. 87]. Surely it is not true that every advancing step is a progressive step. That is, Popper’s use of ‘advance’ is more general as it is merely the recognition of movement forward through time.

⁸ But, any requirement of falsifiability surely does not also require evidence of actual falsifications. After all, some will say, a true universal statement may be falsifiable but being true it will never be refuted with true counter evidence.

⁹ By stressing ‘fallibilism’ here he is not denying that he may have always been a ‘falsificationist’ but only that he is not a naïve one since empirical criticism still requires falsifiable theories. And, of course, as I suggested elsewhere [Boland 1997, pp. 67–8], one must always be careful to recognize that authors often try to repair their history when being interviewed or when writing their retrospective view of their own work. This may be particularly so with Popper who, it can be argued, was eager to answer his critics. Not only is this evident in his *Open Society* [1962] and his *Conjectures and Refutations* [1965], but obviously so in the preface of the *Postscript* [Popper 1983]. Nevertheless, this is no excuse to simply ignore what he says there.

¹⁰ Even as late as 1955, Fritz Machlup was talking about the ‘problem of verification’ but interestingly without any mention of Popper even though German was Machlup’s native language.

¹¹ This can be seen as the principle position embodied in conventionalism [see Boland 2003, chapter 1].

¹² It could be argued that since there is no Popper ‘cannon’, any interpretation is fair game. Moreover, I think Popper’s polemics obscures his own view of the matters at hand because he goes too far out of his way to demonstrate he truly understands his opponents’ view. While such a demonstration is essential for a fair and effective criticism, it is not always clear that he is not attempting to disarm his opponents either by tricking them or by ‘playing his cards too close to his vest’. For example, talking in Vienna Circle terms – in the case of the *LScD*, talking in terms of ‘basic statements’ – too often leads the readers to think he agrees with their terms when he does not. Given the difficulty he saw with trying to engage the Vienna Circle, perhaps his reluctance to put all his cards on the table is understandable. Nevertheless, the resulting misunderstandings that he complained about are ones for which he cannot deny at least some responsibility.

¹³ That is, if you think that the positive evidence of observing white swans somehow can be used to verify the truth status of the statement ‘all swans are white’.

¹⁴ Moreover, as Bartley put it, ‘The importance lent to the falsifiability criterion and the demarcation problem by Popper and others distorts his thought’ [1968, p. 43]. But this is not to say that falsifiability was not important to Popper but that philosophers and economic methodologists who discuss his work by first discussing the importance of falsifiability – and only then mentioning critical rationalism – are ‘putting the cart before the horse’.

¹⁵ Both of whom, of course, were Popper’s students from the mid-1950s.

¹⁶ In this regard it should be noted that while Caldwell [1982] lists ‘critical rationalism’ as one of ‘four crucial aspects of Popper’s thought’, the definition given is that of ‘falsificationism’ [p. 38]! It may be that Caldwell was misled by his later Feyerabend quotation which also distorts the meaning of critical rationalism [p. 85]. Interestingly, in an earlier paper, Caldwell recognizes the ‘critical attitude’ aspect of Popper’s view of science [1980, p. 73, note 13] but this appeared before the choice was made to travel down the Lakatos road.

¹⁷ Hands [1979] has the Klappholz and Agassi article in the references but it is not cited in the text – and in the references it is attributed to ‘Agassi, J. and Klappholz, K.’ [p. 301].

¹⁸ See for example Daniel Hausman's discussion quoted by Neil de Marchi [1988b, p. 20].

¹⁹ A notable exception is Raphael Sassower's published Ph.D. thesis [1985] which is entirely an exercise in Popperian critical rationalism.

²⁰ There is one strategic way this may not be true; I will explain this later.

²¹ Another of Popper's students, Ian Jarvie, has recently put together a selection of quotations from Popper's LSD which, I think, demonstrates how easy it is to interpret some of what Popper says as being a set of rules for belonging to the community of scientists – Jarvie [2001] calls it the 'Republic of Science'. But one must not be misled by this into thinking such rules or conventions are rules of scientific method. They are not; they play a role no different than Robert's Rules of Order, which are always open to revision in any organization by invoking 'standing rules' which are specific deviations from Robert's Rules. The main point is that conventions carry no guarantees leading to true theories. I think Popper includes them only to demonstrate to the Vienna Circle that one does not need verifiability to play the social game of science.

²² Actually, from the critical rationalist point of view, a refutation is the result of a successful test. To say a refutation is a failed test presumes that the purpose of testing is to verify the theory.

²³ Or acceptable by convention [RWR, p. 92].

²⁴ Rationality, even in neoclassical economics, need not refer to any psychological process but only to the notion that the theorist or the decision maker can provide a rational (i.e. logically valid) argument in favour of the decision made [see further, Boland 2003, chapter 3].

²⁵ A typical example in economics is the neoclassical maximization hypothesis [see Boland 1981]. This is also an example of a rule that might be found in Jarvie's Republic of Science since without using the maximization hypothesis as the primary behavioural assumption, one's explanations would not be seen to be part of neoclassical economics.

²⁶ And this is despite Popper's warning that Lakatos is not a reliable source [1983, p. xxiii, note 1].

²⁷ Apparently this justifies his spending so much effort talking about it.

²⁸ In fact, Hands reveals that he has 'spent many years trying to knock down individual rules-based approaches one shot at a time' [RWR, p. 395].

²⁹ Perhaps they are merely repeating the same strategic mistake I have claimed (footnote 12) Popper made in his LSD.

³⁰ He provides a list of issues that need to be addressed in 'contemporary science theory', specifically, 'underdetermination, theory-ladenness, the social nature of science, relativism, antifoundationalism and naturalism' [RWR, p. 5]. But, this is somewhat confusing, as they seem to be the same old issues addressed by his so-called 'shelf of scientific philosophy' [RWR, p. 2].

³¹ He fails to note that if Popper's books were ever on the shelf, the rules-based methodologists never read them.

³² A point already made by Popper's students in the 1960s [e.g., Agassi 1964].

³³ It might be interesting to note that Chapter 12 of my 1982 book specifically advocated a 'problem-dependent methodology' that sounds a lot like the pragmatism Hands is promoting here. He also tries to anticipate potential critical responses from those among 'the critical rules-seeking methodologists' to his new economic methodology. Specifically, he would have them recognize that the old methodology made no impact on the economics profession. Unfortunately, there is much to look forward to since the critics never got this message when I delivered it in the Epilogue of my 1989 book.

³⁴ I guess we would not want to let the intended audience of rules-based economic methodology know that by going down the road of 'new economic methodology' they would be returning to Popper's philosophy of science.

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