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Karl Popper and the Methodologists of Economics

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Abstract

This article chronicles the difficulties that methodologists of economics have had in introducing Karl Popper's philosophy to their fellow economists. It presents some general reasons for the problem before specifically examining the proposition that a sound appreciation of Popper's doctrines cannot be attained from simply studying the doctrines themselves. What it also requires is an understanding of the problem situation that the doctrines sought to address. This is illustrated through an examination of the way methodologists have grasped, or failed to grasp, the development of Popper's own thought about the problem of demarcating empirical science from non-science, and the related problem of whether the limits of empirical science coincide with the limits of arguability. The article demonstrates that a neglect of these considerations has produced confusion in the literature—both in the way that Popper's philosophy has been presented and in the way in which its contemporary relevance has been assessed.

Keywords: Popper, Critical Rationalism, Falsificationism.

JEL Classification: B41

1. Introduction

What problem do Karl Popper's writings present to methodologists of economics? That there is a problem of one sort or another is signified by the great accumulation of literature that addresses the issue; a corpus that now includes its own intellectual chronicle-cum-obituary (Backhouse, 2012). Indeed, it is perhaps fair to say that something went awry in the introduction of Popper's philosophy to economics. In considering the cause of such difficulties, the usual suspect is the very nature of philosophy; it has, after all, long had the reputation of being an abstruse subject—even amongst philosophers. But Popper was a philosopher who often argued that clarity of expression was an intellectual's first duty and he castigated and ridiculed those who played '...the dreadful game of making the simple appear complex and the trivial seem difficult' (1994A, p. 94). So if we dismiss the possibility that those who wrestle with the methodology of economics are themselves secondary players of 'the dreadful game', then their difficulties in understanding Popper's philosophy must have arisen for reasons other than obscurity by expression. That is to say, the problem must be attributable to either his style of philosophising and general approach to philosophical matters, or it must be due to the content of the philosophy itself. The objective of this paper is to examine the first of these possibilities.

This choice of emphasis is not to assert that the content of Popper's philosophy is unimportant to understanding the difficulties that it has presented to methodologists of economics. In particular, Popper's (1957) doctrine of the 'unity of method' (UoM)—that all of the theoretical or generalizing sciences make use of the same method and that the historical sciences, in a fashion, use it too—undeniably plays a central role in many of the Popper-related debates that have arisen in the methodology of economics; for example, between Hands (1985) and Blaug (1985) and between Neves (2004) and Kerstenetzky (2009). The UoM doctrine is also pertinent to some of the other significant papers that have assessed Popper's philosophical and methodological contribution to the social and economic explanatory endeavour such as that of Lawson (2008) and Hudik (2011). But I

hope to consider the UoM doctrine in a separate paper that is designed to complement this one; here I wish firstly to establish that our understanding of the content of Popper's philosophy can benefit from being built upon a firmer footing. In particular, I want to argue that in order to appreciate properly the significance and relevance of a philosopher's works, one must firstly appreciate both his general mode of philosophising and the particular questions and problems toward which he directed his thought. This is to say that matters of context ought to be considered before matters of more detailed doctrinal content. This approach to studying the history of ideas has a precedent: it is associated with the philosopher and historian R.G. Collingwood who called it the method of 'question and answer' (1939, p. 29). As we shall see, it is an approach to understanding the theoretical products of the human mind to which Karl Popper also subscribed. In and of itself, it will go some way, but not the whole way, toward understanding the difficulties that Popper's philosophy has created for methodologists in economics.

My attempt to consider these issues will proceed in the following manner. In section (§) 2, I supply a selective chronicle of the discussions of Popper's philosophy by methodologists in economics—one that is designed to highlight the problem that they would seem to have had in introducing his ideas to their fellow economists. In § 3, I offer some superficial reasons as to why Popper's philosophy presents a problem to a would-be student of his works. In § 4, I explain how the nature of the problem may be more thoroughly understood. In §§ 5-6, I examine how it has created difficulties for those methodologists of economics who have attempted to interpret Popper's philosophy and relate it to their own problem interests. The §§ 5-6 will go some way toward resolving the controversies that characterise the discussion of Popper's ideas in the methodological literature of economics. In § 7, I ground what would otherwise be a somewhat abstract and airy discussion of the history of ideas by giving an illustration of why Popper's philosophy continues to be important to contemporary economic theorisation and debate. In contrast to the impression that may be given by the title of Backhouse (2012), the illustration is designed to demonstrate why it might not yet be the 'calling time' on Karl Popper's philosophy. In § 8, I conclude the paper, mostly by emphasising that it

is only a job half-done because the difficulties presented by Popper's UoM doctrine require a further discussion.

2. The Chronicles of Popper

Economic methodologists had a problem in introducing Karl Popper's philosophy to their fellow economists. That this is so is signified by the titles of the many papers that have addressed the issue. One of the first economists to reference Popper's (1934) *Logik der Forschung*¹ was the late Terence Hutchison (1938); but the message that Hutchison took from Popper's analysis of the logic of scientific discovery came to be criticised by Klappholz and Agassi (1959). That criticism carried weight because Joseph Agassi was at that time one of Popper's closest collaborators. Yet Hutchison responded to the criticism, so '*Methodological Prescriptions in Economics*' (Klappholz and Agassi, 1959) begat '*Methodological Prescriptions in Economics: A Reply*' (Hutchison, 1960). That exchange coincided with the translation of *Logik der Forschung (L.d.F)* into English as *The Logic of Scientific Discovery (L.Sc.D; Popper, 2002A [1959/1934])*. Popper's analysis thereby became available to a much wider readership and *L.Sc.D* soon established itself as a stalwart reference in studies of the methodology of economics. It became, for instance, a crucial component to Mark Blaug's widely-read text on *The Methodology of Economics* (1980). But Blaug's reading of Popper was not accepted by all and by 1985 the Popper-inspired literature on the methodology of economics was deemed to merit a wholesale re-appraisal. Thus, '*Karl Popper and Economic Methodology - A New Look*' (Hands, 1985). This was followed by a thirty-two page panoramic with the objective of '*Clarifying Popper*' (Caldwell, 1991); but the clarity revealed that there was a problem in '*Dealing with Popper in Economic Methodology*' (Boland, 2003). Yet one way to deal with a problem is to deny that it exists. So, in a revisionist style, an about turn to the 1930s has recently been made. Hart (2011, p. 414) argues that Klappholz and Agassi's (1959) criticisms of Hutchison '... are misdirected because they assume that Hutchison is operating in some kind of Popperian world'; an assumption that Hart

disputes. An intellectual chronicle-cum-obituary also came to pass: *'The Rise and Fall of Popper and Lakatos in Economics'* (Backhouse, 2012).

Crudely summarised, the chronicles of Popper in the literature on the methodology of economics are a chronicle of disagreement. Why is this?

3. The Enigma of Popper

Sir Karl Raimund Popper [1902-1994] has become something of an enigma. He described himself as '... a disciple of Socrates' and in his writings he often emphasised the value and scarcity of a Socratic intellectual modesty (Popper, 1974, p. 962, 1994A). Indeed one of his leading students wrote that he '... made a fetish of modesty' (Munz, 2004, p. 22). In contrast, Steve Fuller (2003, p. 2) claims that '... over his lifetime Popper rarely received the recognition that he thought he deserved—and never tired of reminding everyone of it'. However these divergent personal impressions have arisen, intellectual modesty did not conceal Socrates under history's bushel; Popper likewise. Thus whilst Popper might, or might not, cringe at being described as one of the greatest thinkers of the twentieth century, the case is easily made. Born in *fin-de-siècle* Vienna, the span of his intellectual life, which he often dated as beginning in 1919, embraced three-quarters of that century and the intellectually more vibrant part at that. During that time, the scope of his interests and achievements were unusually wide, and included revolutionary contributions to epistemology, the philosophy of science, probability theory, the philosophy of history, and political philosophy.² And finally, if an immodest claim is made to supplement these observations, one might also say that Popper has an attribute that is shared by many great thinkers: he somehow influences the thought of the many who have never read him.

But ideas that become a part of a society's general back-ground knowledge, without actually being generally studied or taught, are ideas prone to distortion. Consider the way in which some of Popper's central motifs circulate in popular discourse. Take his idea and doctrine of 'the Open

Society' (Popper, 1966A [1945], 1966B [1945]). It presents an intellectual defence of freedom and a diagnosis of the intellectual origins and errors of utopian social planning and totalitarianism, but today it is often equated with the idea of a liberal democracy with a market economy in which 'anything goes': a catch-all absolver for all manner of intolerant opinion and behaviour. Yet a key part of Popper's argument involved the realisation that the Open Society, if it is to protect the freedoms of its people, must establish an interventionist state that is prepared to defend the tolerant against the onslaught of the intolerant.³ Moreover, Popper acknowledged that state interventionism must extend to matters economic—a position that is easily overlooked given his reputation for being a cold-war liberal, member of the *Mont Pèlerin* society, and close associate of Friedrich von Hayek.⁴ Elsewhere, it is not unusual to find Popper's critique of the '*conspiracy theory of society*' (2002B [1963], p. 165 [emphasis in original]), or the thesis that social outcomes are the result of the design of some powerful individual or group, conveniently transmogrified by some, usually a powerful individual or group, into the claim that a conspiracy theory is incapable of explaining any event at all.

For those who seek to develop a better understanding of Popper's philosophy, any prior misconceptions about the meaning of those few Popper motifs that have entered popular discourse are not too difficult to dispel. A more serious problem for the would-be student is that there are several episodes in Popper's intellectual history that have entered intellectual folklore. They present a problem in so far as they are essentially spectacular side-shows; yet the watching of a side-show can create the misimpression of having witnessed the main event. Indeed, Popper sought to dispel what he described as the 'Popper legend' (Popper, 1974, p. 963). That legend surrounded his setting the Vienna Circle's positivists aright on how to demarcate the theoretical systems of the empirical sciences from those of metaphysics. Contrary to the legend, Popper argued that this could be achieved without rendering metaphysics into meaningless nonsense or foundering on the problem of induction.⁵ Another instance of contested lore centres on Popper's confrontation with Ludwig Wittgenstein—who may or may not have been gesticulating with a fireplace poker—at the meeting

of the University of Cambridge's Moral Sciences Club in 1946 (Popper, 2002C [1974]).⁶ Yet another is his 1961 debate—or non-debate as the case might be—with Theodore Adorno on the logic of the social sciences (Adorno, *et al* 1976).⁷ Similarly, his 1965 debate—or non-debate as the case might be—with Thomas Kuhn over their differing views of science (Lakatos and Musgrave, 1970).⁸

Although the aforementioned reports or misreports of these famous intellectual confrontations are want to both distract and breed misunderstandings as to what Popper's philosophy was all about, they do supply a clue as to why that philosophy presents a problem to a would-be student of his work. The clue is that they are public illustrations of Popper's every-day mode and attitude toward philosophising—a mode and attitude that was not always shared by his interlocutors. It is something that Popper himself called attention toward in the introduction to one of his final publications. Popper (1994B) emphasised a single sentence that he had first written some fifty years earlier in *The Open Society and Its Enemies* (Popper, 1966B [1945]). He wrote that the sentence had been little noticed by others, yet, to him, summed up the approach to philosophy that he called 'critical rationalism' (Popper, 1966B [1945], p. 229). The sentence was: '*I may be wrong and you may be right, and by an effort, we may get nearer to the truth*' (Popper, 1994B, p. xii, 1966B [1945], p. 225 [emphasis in originals]).

This motto, which at first sight may seem rather trivial, actually summarises a great many significant ideas. It encapsulates Popper's subscription to the principle of fallibilism—the recognition that the best of our knowledge may be mistaken. It encapsulates Popper's commitment to a principle of tolerance and charity—that since we are all fallible we ought mutually to pardon each other's stupidities and respect and genuinely entertain one another's ideas. It encapsulates Popper's commitment to the use of critical reason—the attitude that it is by arguing rationally over our practical and theoretical proposals that we may hope to discover their potency in addressing our practical and intellectual problems. It encapsulates Popper's commitment to truth as a standard for our critical reason—that we may hope to improve our theoretical knowledge, not by searching for a

final positive and definitive justification of it, but by moving it nearer to the truth through the detection and elimination of error. And finally, it encapsulates the idea that there is an objective reality for critical reason to explore; for knowledge can only be fallible and mistaken if there is something external to the mind to be mistaken about. Taken together, these are the principal ideas of Popper's philosophy of critical rationalism.

But crucially, this mode of philosophising entails that a proper appreciation of many of Popper's arguments and doctrines cannot be attained from simply studying the arguments and doctrines themselves. What it also requires is an appreciation of his interlocutors' arguments and an appreciation of the problem that he and they were addressing. A recognition of this very difficulty features in Popper's *own* discussion of the problem of understanding a scientific theory. In 1963, during a plenary address to a conference on experimental biology, he said:

What is meant by saying that we '*understand*' a scientific theory?... Understanding a theory, I suggest, means understanding it *as an attempt to solve a certain problem*. This is an important proposition, and one which too few people understand. What is the point of, say, Newton's theory? It is an attempt to solve the problem of explaining Kepler's and Galileo's laws. Without understanding the problem situation that gave rise to the theory, the theory is pointless—that is, it cannot be understood. (Popper, 1994B, pp. 101-102 [emphasis in original])

Indeed, the problem is even here present: Popper's thesis that science begins with problems is *itself* more readily understandable if one appreciates that it has an antithesis: that science begins with observation. It was this thesis that *L.Sc.D*, in part, opposed.⁹

4. Popper and the 'Logic of Question and Answer'

So the problem that is presented to a would-be student of Popper's works now comes into clearer view. If Popper's general approach to philosophising was to develop his ideas in relation to various problems, or problem situations that had other discussants, then any attempt to understand what he had to say on any matter, at any moment in time, is a historical conjecture as to the make-up of

his problem situation at that moment in time. Moreover, the message of the Popper canon of say, 1959, might not be quite the same as that of, say, 1934—even when it ostensibly addresses the same problem. This for the simple reason that Popper’s own learning about the problem, and the state of the general critical discussion addressing it, might well have altered in the intervening time.

Accordingly, as Popper (1979, pp. 177-178) himself proposed, the development of a historical understanding of a theory is ‘... free to use anything that may be helpful’ and may ‘... be elaborated or even radically changed whenever the need arises’; for instance, whenever criticism renders a particular construction unsatisfactory. Consequently, the exploration of a thinker’s historical problem situation need not entail the mode of historical explanation that Malachi Hacoheh (2002 [2000]) adopted in his impressive intellectual biography of Karl Popper’s formative years. That book placed Popper’s early intellectual development in the social, political and cultural context of interwar Vienna; but as its author acknowledged, directly placing a theory in the more immediate context of its problem situation is ‘... a crucial intellectual history method’ (2002 [2000], p. 19).¹⁰ Indeed, as the reception of Hacoheh’s own study perhaps illustrates, opening the problem of historical understanding to the influence of ever more nebulous factors may produce far more controversial results.¹¹

Of course, all this is not to say that Popper may not bear some responsibility for the difficulties that might arise in interpreting his work. Although he was all too aware of the intellectual problem of understanding another thinker’s intellectual problems, the effort that he himself made, in his writing, to document his own intellectual problems, and who he shared them with, was variable. Generally, a reader must pay close attention to the copious footnotes to Popper’s works in order to find out who a particular argument might be directed towards. And as § 6 of this paper will make clear, it is also a challenge to detect when Popper’s own thinking about a particular problem might have changed course over time.

But a scholarly attention to detail may on occasion still prove to be an inadequate response to the problem of understanding another thinker's problem situation. This is perhaps illustrated by Popper's *own* discussion of the issue in the aforementioned conference address reproduced in Popper (1994B)—it so closely resembles the thoughts of his fellow philosopher, R.G. Collingwood [1889-1943], that it is hard to accept that Popper was not attuned to Collingwood's works when he discussed the matter. For instance, Collingwood (1939, p. 55), writes:

... according to my own 'logic of question and answer', a philosopher's doctrines are his answers to certain questions he has asked himself, and no one who does not understand what the questions are can hope to understand the doctrines.

Yet Popper did not elect to cite Collingwood in this address. Nonetheless, an important influence may be hypothesised, for elsewhere, in a paper dating from 1968, Popper compares extensively his own mode of historical explanation to that of Collingwood, whom he describes as 'one of the great students of this problem' (Popper, 1979, p. 183). Hence a reader of Popper (1994B) would have had to have either come to Popper via Collingwood, or would have had to have read this other part of the Popper canon, in order to recognise the possible intellectual influence of Collingwood's 'logic of question and answer' on Popper's own position.

Generally, the importance of the 'logic of question and answer' to understanding the theoretical products of the human mind ought to be within the grasp of economists. Let us consider some examples from within the subject's own intellectual history. Consider, for instance, Popper's own major work on the philosophy of history and the methodology of the social sciences: *The Poverty of Historicism* (1957). The book, which was first published as a series of papers in *Economica* (Popper, 1944A, 1944B, 1945), constructs a formidable doctrine on the methods of the social sciences: the doctrine that it labelled as 'historicism'. It reveals why that doctrine has a pernicious influence upon a society and its politics, proceeds to criticise it, and then presents an alternative account of what the character and methods of the social sciences ought to be. Several decades removed from the historicist horrors of Nazism, Stalinism, and the Central European Tragedy, a historically blinkered

reader of to-day might well wonder why its author went to such a trouble. In a similar fashion, consider what a student of economics, living in Great Britain during the post-war years, would have made of J.A. Schumpeter's *Capitalism, Socialism and Democracy* (1943). Given the impact of socialism on the politics of the early twentieth century, such a reader would have understood Schumpeter's *problem situation*. Contrast that reader with a counterpart in the second decade of the twenty-first century. Such a latter-day reader might well find the book's concerns to be all rather quaint and peculiar. Or consider how *The General Theory of Employment Interest and Money* (Keynes, 1936) might come to be understood by two groups of readers of which only one has a grasp of the author's problem: whether the postulates of classical economics are generally applicable to the money-using society and uncertain world in which he lived. Cue a discussion that contrasts Keynesian economics with the economics of Keynes.¹²

But rather than consider these or any further examples, let us instead consider how these kinds of factors, and a failure to adopt Collingwood's so-called 'logic of question and answer', have created difficulties for those methodologists of economics who have sought to interpret Popper's philosophy.

5. Popper and the Methodologists of Economics

To begin, anyone expecting to find in Popper's writings some advice on how to resolve methodological problems in economic research is liable to map a solution to one problem as if it were an attempt to solve another. And unsurprisingly, the fidelity of their map is liable to be disputed by those more familiar with the original terrain.

For instance, Popper is clear that the opening chapters of *L.Sc.D*—the 1959 English language translation of *L.d.F.* (1934)—address two 'fundamental problems', namely, '... the logical analysis of... the method of the empirical sciences' and '... what do we call 'empirical science'?' (2002A [1959/1934], p. 3). These are the so-called problems of induction and demarcation respectively. The

problem of demarcation, which Popper attributed to Immanuel Kant, is how ‘... to distinguish the empirical sciences on the one hand, and mathematics and logic as well as ‘metaphysical’ systems on the other’ (Popper, 2002A [1959/1934], p. 11). The problem of induction, which Popper attributed to David Hume, is that the invalidity of inductive inference presents a problem to the principle of empiricism—the principle that only experience can decide upon the truth and falsity of a factual statement; for how can the strictly universal statements that characterise generalised theories be validly inferred from particular experiences?¹³ But as Popper’s discussion makes clear, Kant and Hume are stalking-horses for his real target and problem situation: the logical positivists of the Vienna Circle.¹⁴

Famously, in his solution to the problem of demarcation, Popper proposed the criterion of ‘falsifiability’. This presented the generalising empirical sciences as being comprised of systems of statements with a logical form that is amenable to being tested by experience, or rather by the statements that report experience. Why did he offer the criterion of ‘falsifiability’? One reason is that the logical form of strictly universal statements is that they ‘... are never derivable from singular statements, but can be contradicted by singular statements’ (Popper, 2002A [1959/1934], p. 19); that is, they are ‘falsifiable’. The proposed criterion of falsifiability therefore recasts the principle of empiricism: in Popper’s epistemology empirical knowledge is neither induced from, nor verified by, experience; but the reports of experience can test the logical implications of a system of empirical theories: ‘It might be described as the theory of *the deductive method of testing*, or as the view that a hypothesis can only be empirically tested—and only after it has been advanced’ (Popper 2002A [1959/1934], p. 7 [emphasis in original]). To Popper, this solved (or perhaps dissolved) the problem of induction.

Enter an economic methodologist with a different problem. With the tradition of *a priori* economic theorisation in his intellectual gun sight, the late Terence Hutchison (1938) invoked Popper’s falsifiability criterion to classify economic theory into either the empirically falsifiable or the

analytic/tautologous. But that is not to use the criterion as Popper intended—to demarcate the systems of statements that comprise the empirical sciences from those statements that are either analytic/tautologous or metaphysical. Two of Popper's colleagues at the London School of Economics (LSE) eventually came to consider the discrepancy between the intended and the new application (Klappholz and Agassi, 1959). By way of illustration, they used the following two statements: '*ceteris paribus*, the imposition of a tax on cigarettes will raise their price' and '*ceteris paribus*, the imposition of a tax on cigarettes will not raise their price'. Each statement refers to the seemingly empirically familiar, but neither is falsifiable so long as the specific reference of the *ceteris paribus* clause is undisclosed; yet clearly neither statement is necessarily true or tautologous—each statement is incompatible with the other. In short, *pace* Hutchison, metaphysical statements do not reduce or equate to analytical statements or tautologies.

To say all of this slightly differently: Popper's problem was to formulate a criterion as to what ought to qualify as an empirical statement so far as an empirical *science* is concerned. And this is an important problem for an empirical science *even* if there are no prospects of its practitioners ever uttering a tautology. For there are seemingly empirical statements, even seemingly empirically confirmable statements, that are not empirically falsifiable statements. Consider a statement like: 'there exists a fertilizer that will increase the volume of a tobacco crop ten-fold'. How can the truth or falsity of this statement be investigated empirically when all empirical observation and investigation, precisely because it is *empirical* observation and investigation, is space and time bound? Scientific investigations require hypotheses that make a difference, either directly or indirectly, to what is observable; if they do not then they should not be admitted to empirical science.¹⁵

Enter another economic methodologist with a different set of problems. The late Mark Blaug (1980) posed two questions: where do economic theories fall when classified against the falsifiability criterion and how widely do economists practise the deductive method of testing their theories? But

an exploration of those problems may produce disagreement if Imre Lakatos's (1970) thought on the thought of Karl Popper is presented as if it were Karl Popper's thought on the thought of Karl Popper. And this was the charge that Lawrence Boland (1994, 2003, 2006) levelled against Blaug's project. Boland (1994, p. 154) proposed that 'two views of Popper' circulate amongst economic methodologists. The 'popular view', which Boland attributed to Blaug, is labelled 'falsificationism'.¹⁶ 'Falsificationism' emphasises the problem of demarcation—albeit as formulated by Imre Lakatos (1970). The other view, which is Boland's own, is that of 'the Socratic Popper' (Boland, 1994, p. 157). The 'Socratic Popper' places the criterion of falsifiability into the broader context of the fallibilist philosophy of critical rationalism. Boland (1994) argued that the two differing views of Popper mirrored the intellectual differences that arose between Imre Lakatos and the other members of Popper's seminar group at the LSE in the late 1950s and early 1960s; namely, Lakatos's differences with Joseph Agassi and William Warren Bartley III. Caldwell (1991, p. 25) drew a similar distinction between 'Popper the falsificationist' and 'Popper the critical rationalist'. More recently, Hart (2011) offers not one, but two caveats as to which Karl Popper he is writing about by entitling his own rendition of Popper's philosophy of science—replete with references to Popper's *L.Sc.D* (1959)—as '(the Lakatosian) falsificationist interpretation of Popper's philosophy of science' (Hart 2011, p. 411).

But these various Karl Popper avatars are the creation of the methodologists of economics and they surely do two things: firstly, they further the image of Karl Popper as enigma; secondly, they make the discussion of Popper's philosophy in economics highly problematical and perhaps even faintly ridiculous.

Can the problem of 'the two Karl Poppers' be more thoroughly resolved by 'the logic of question and answer'? In particular, can it be resolved by a conjecture that even a great thinker like Popper found reason to change the emphasis of his thought over time? And the reason was that his problem situation, and his reaction to it, developed over time. Unfortunately, *Clarifying Popper*, Bruce Caldwell's (1991) otherwise panoramic review of Popper's philosophy and a key source for

subsequent discussions of Popper's philosophy by economic methodologists, shunned this crucial issue:

Sorting out "what Popper thought when" is a daunting job... There will be no attempt to provide a chronological depiction of the development of Popper's thought... this paper focuses rather narrowly on Popper's methodology of science (Caldwell, 2001, p. 2).

But the job of sorting out 'what Popper thought when' is not quite as daunting as Caldwell supposed. More importantly, according to the 'logic of question and answer', it is the obvious means by which the 'two Karl Poppers' can be unified.

6. Unifying the 'Two Karl Poppers'

Earlier, in § 5, it was noted that the real problem situation of *L.d.F.* (1934) was the Vienna Circle and its doctrines concerning the theory of knowledge and meaning.¹⁷ The doctrines proposed that only two forms of proposition were meaningful or cognitively significant: firstly, analytic propositions whose necessary truth can be demonstrated by formal proof; secondly, empirical propositions whose truth or falsity can be decided by experience. For the Circle, meaningful science was thereby demarcated from meaningless metaphysics by a criterion of verification: a scientific proposition was one that could be verified by an actual or possible experience.

Popper was unconcerned with the Circle's treatment of analytical statements, but, as noted in § 5, he had a different solution to the problem of how to demarcate science from metaphysics. However, he also differed from the Circle in his conceptualisation of the underlying problem. In particular, he was not at all concerned with the problem of what is meaningful—to him metaphysical theories were not meaningless, they were just not scientific when assessed against the logical criterion of falsifiability. On the other hand, Popper clearly admired the Circle's bold attempt to wrestle with major philosophical problems—problems that were central to the interests of great philosophers like Immanuel Kant. For instance, Popper (2002A [1959/1934], p. 11) wrote that the problem of

demarcation became, with Kant, ‘... the central problem of the theory of knowledge’ and that it was ‘... the source of nearly all the other problems of the theory of knowledge’. He wrote that ‘... it is the task of the logic of scientific discovery, or the logic of knowledge... to analyse the method of the empirical sciences’ (Popper, 2002A [1959/1934], p. 3).

These passages in *L.d.F./L.Sc.D.* seemingly suggest that Popper was, at least in part, following the agenda of the Circle: all serious argument and inquiry, aside from that which fell within the curtilage of logic and mathematics, must deploy the logic of scientific discovery. For instance, in the citation that concludes the paragraph above, ‘the logic of knowledge’ is equated with ‘the logic of scientific discovery’. Elsewhere, Popper wrote ‘... epistemology, or the logic of scientific discovery, should be identified with the theory of scientific method’ (Popper, 2002A [1959/1934], p. 27). And he wrote that: ‘As to the task of the logic of knowledge... I shall proceed on the assumption that it consists solely in investigating the methods employed in those systematic tests to which every new idea must be subjected if it is to be seriously entertained’ (Popper, 2002A [1959/1934], p. 8). Hence, these passages do seem to suggest that outside of the formal disciplines, the limits of what is arguable coincide with the limits of empirical science. On *this* reading, Popper’s position *might* be summarised as something like: ‘arguability *as science as testing*’.

But on the other hand, there are also passages in *L.d.F./L.Sc.D.* that suggest that this reading is misleading. In particular, Popper’s thinking about the problem of demarcation unearthed a new problem. It first surfaces when Popper (2002A [1959/1934] § 4) considers the epistemological status of the demarcation criteria *itself*. In his discussion of this issue, Popper rejects a naturalistic theory of methodology, or the view that methodology is itself an empirical science that must study actual scientific practice. On the contrary, Popper defended the autonomy of both methodology and philosophy. He wrote that he regarded his demarcation criteria as:

... a proposal for an agreement or convention. As to the suitability of any such convention opinions may differ; and a reasonable discussion of these questions is only possible between parties having some purpose in common... There is only *one* way, as far as I can see, of arguing rationally in support of my proposals. This is to

analyse their logical consequences: to point out their fertility—their power to elucidate the problems of the theory of knowledge (Popper, 2002A [1959/1934], p. 15 [emphasis in original])

Thus, even in 1934, the problem of demarcation would seem to have produced a new subterranean problem: a *problem of arguability*. That is, whether the limits of what is rationally arguable coincide with the limits of empirical science.¹⁸ Furthermore, the passage would seem to suggest that Popper thought that they did not. As it noted, it is possible, if parties have a common purpose, to argue over the fruitfulness and usefulness of proposals.¹⁹ In this case, over the proposal to characterise empirical science by its method—principally, by the deductive method of testing.²⁰ Moreover, another passage suggests the autonomy of philosophical and methodological argument because Popper writes: ‘... the main problem of philosophy is the *critical analysis* of the appeal to the authority of ‘experience’’ (2002A, [1959/1934], p. 30 [emphasis added]).

Between 1934 and 1959, Popper further developed his thinking about this problem. One exhibit that is relevant to this conjecture is an amendment that was made, via a new footnote, to the 1959 English language translation of *L.d.F.* In the opening chapters of the book, Popper’s main theses concerning the problems of demarcation and induction are presented and he summarised their implication for epistemology thus:

I readily admit that only observation can give us ‘knowledge concerning facts’. But... this knowledge of ours does not justify or establish the truth of any statement. I do not believe... that the question which epistemology must ask is, ‘on what does our knowledge rest... or more exactly, how can I, having had the experience S. justify my description of it, and defend it against doubt?’... In my view, what epistemology has to ask is, rather: how do we test scientific statements by their deductive consequences. (Popper, 2002A [1959/1934], pp. 79-80)

This passage is from the main text of *L.Sc.D.*, the 1959 English language translation of *L.d.F.* (1934). However, in the 1959 translation, Popper added a new footnote to the passage above, rephrasing it as follows:

At present, I should formulate this question thus. How can we best criticize our theories... rather than defend them against doubt? Of course *testing* was always, in my view, part of *criticizing*. (Popper, 2002A [1959], p. 80 fn.*1 [emphasis in original]).

Now the significance of this amendment would seem to depend upon what problem it is related to. If a reader relates it to the problem of demarcation, which is what Popper is ostensibly addressing, then admittedly the reformulation seems insignificant: in deducing some consequences from a theory so that they may be compared against the reports of experience one is in search of a reason to criticise the theory. This is how the deductive method can probe (but not justify) theories and 'knowledge concerning facts'. Hence criticism, in this case the discovery of a logical contradiction, may open our minds to the inadequacy of our empirical theories, possibly thereby stimulating new conjectures. So in 1934, Popper's position *may* be read as something like: "arguability *as science as testing*"; and in 1959, given the aforementioned new footnote, it *may* be read as something like "arguability *as science as criticism as testing*". This hardly seems a significant alteration. Indeed, if the deductive method is to have any logical force, critically appraising whether the implications of an empirical theory have been validly inferred from it being conjoined to other auxiliary premises is a necessary preliminary to empirical testing.

But in subsequent publications, Popper signalled that the amendment held a greater significance. Importantly, in the first volume to his *Postscript to the Logic of Scientific Discovery* (1983, § 17)²¹ Popper explicitly acknowledged that the problem of demarcation had other problems closely linked to it; namely, what he there expressly called 'the problem of arguability' (Popper, 1983, p. 161). In response to this latter problem, Popper eliminates any ambiguity as to how the aforementioned passages of *L.Sc.D* ought to be interpreted. For instance, in the preface to the book, Popper declares that what matters to science is '... a man who wishes to understand the world and to learn by arguing with others'; and that '... *the so-called method of science consists in this kind of criticism*' (Popper, 1983, pp. 6-7 [emphasis in original]). This shift *might* be summarised as a move from something like "arguability *as science*", in which the limits of what is rationally arguable coincide

with the limits of science, to something like “science as arguability”, in which they do not. So the aforementioned formulation becomes something like: “science as arguability as criticism as testing”; where criticism is now generally regarded as the logical means to probe and test the implications of a theory. This shift in emphasis is evident in many of Popper’s post-*L.Sc.D* publications. For instance, in his 1961 exchange with Theodore Adorno, Popper laid out his views on the *Logic of the Social Sciences* in a series of crisply-formulated theses; a kind of *Maginot Line* that he wanted Adorno to attack (Adorno, *et al* 1976).²² In the twelfth thesis he wrote: ‘What may be described as scientific objectivity is based solely upon that critical tradition which... makes it possible to criticize a dominant dogma’. And in the fourteenth thesis: ‘I consider it important to identify scientific method, at least in the first approximation, with the critical method’. Still later, in a paper entitled *Science and Criticism* and dating from 1974, Popper wrote:

The criterion of scientific status which I have proposed for theories... is criticizability, rational criticism. In the natural sciences this boils down to criticizability by means of empirical tests or empirical refutations (Popper, 1994A, p. 54).

Finally, in a paper dating from 1970, Popper seems to explicitly acknowledge that he had further developed his thinking since the time of *L.d.F.*:

In those days I wrongly identified the limits of science with those of arguability. I later changed my mind and argued that non-testable (i.e. irrefutable) metaphysical theories may be rationally arguable. (Popper, 1979, p. 40 fn. 9)

Why does this represent a development in Popper’s thinking? It is because the latter position offers a generalised epistemology: a generalisation of *L.d.F./L.Sc.D*’s ‘theory of the deductive method of testing’ into the wholesale philosophy that Popper subsequently called ‘critical rationalism’ (1966B [1945], p. 232). How do the positions differ? Critical rationalism may equate (rather than demarcate) different cognitive activities so long as they each embody a ‘critical method’—at least in ‘the first approximation’. Yet it also allows for differences as to what those activities might ‘boil down to’. Thus said, critical rationalism does not make the demarcation criterion of falsifiability

redundant, for pointing out that a theory, as presently formulated, is incapable of being logically contradicted by the reports of experience may itself be a valuable form of criticism. It might, for instance, prompt the reformulation of the theory so that it does become more criticisable by becoming amenable to empirical testing. And, as Popper says, in the empirical sciences such empirical testability is what criticism 'boils down to'. Hence the criterion of falsifiability can be retained as a solution to the problem of demarcating the empirical sciences from metaphysics, but crucially it no longer ought to be read as demarcating what is arguable *per se*. If a metaphysical theory can be criticised then it is also arguable. Indeed, one should note in considering these issues, that in 1958, just one year prior to the new footnote being inserted into the English translation of *L.d.F.*, Popper wrote a detailed paper that discussed how metaphysics is criticisable.²³

Thus, the origin of the various Popper avatars in the literature on the methodology of economics is revealed: Popper's thought changed tack between 1934 and 1959. Indeed, his later position was pretty much summed up by Klappholz and Agassi all those years ago: 'Our view... is that there is only one *generally applicable* methodological rule, and that is the exhortation to be critical' (1959, p. 60 [emphasis added]). So, two cheers for Lawrence Boland, Karl Popper is indeed best understood as a disciple of Socrates and falsifiability is not the be-all and end-all of Popper's philosophy.

7. The Legacy of Popper

The fallibilist philosophy of critical rationalism rejects the idea that truth is manifest and that there is an infallible method for discovering it. In the aftermath of a great economic catastrophe, the continued relevance of this idea should hardly require much emphasis. I am referring, of course, to the British banking crisis of the first decade of the twentieth-first century. Nonetheless, given that the title of Backhouse (2012) may lead some to surmise otherwise, that event might usefully further illustrate why the philosophy of critical rationalism continues to be important to contemporary economic theorisation and debate.

Consider the theory that a panel of experts from the British Academy offered to Her Britannic Majesty Queen Elizabeth II, in response to her question, posed during a visit to the LSE, as to why no one had foreseen the so-called ‘credit crunch’. In a letter to Her Majesty, two Fellows of the Academy summarised the factors that had been identified by the panel as contributing to the failure of banks, regulators, commentators and politicians to foresee the crunch: ‘wishful thinking’, ‘hubris’, ‘delusion’, ‘the psychology of herding’, ‘a psychology of denial’ and an overall ‘failure in the collective imagination of many bright people’ (Besley and Hennessy, 2009). Let us call this sketched theory *T1*. *T1* is psychologistic: it reduces the explanation of an historical event to the individual psychologies of some of the agents directly involved in it. *T1* does not explicitly contain a strictly universal statement and those that it may implicitly contain seem rather trivial or infertile. As Popper (1957, § 32) noted, it is hard to conceive of any historical event which could not be plausibly explained by an appeal to certain propensities of ‘human nature’. Thus, aside from the agents involved testifying that they think the account offered by *T1* is true or false of them—a testimony that they might be rather unwilling to supply—it is hard to see how the deductive method of empirical testing is relevant to the assessment of *T1*.

But is *T1* within or beyond the reach of critical rationalism? How might its claims be criticised and rendered rationally arguable? A critic of *T1* might say that the pre-crash business strategies of some of the banks were inconsistent with the background knowledge represented by the basic principles of prudent banking. Further, that the persons involved in banking, be they intellectually bright or otherwise, did not need to ‘imagine’ those principles—they only needed to study them. But then the leaders of some of the crashed British banks held no qualifications in banking. Consequently, the critic might question the adequacy of the regulatory framework for appointing such persons. The critic’s remarks therefore examine *institutional conditions* that are ignored by *T1*. Our critic might also cite J.M. Keynes’s (1936) remark that money is, above all else, a device for linking the present to the future; so money in a monetary society must endure if that society is to also endure. Thus systemically inter-connected banks may enjoy an implicit societal guarantee that their business will

endure. What if the critic sketches an alternative theory, *T2*, that clashes in important respects with *T1*? What if *T2* is less charitable in its assessment of the aims and motivations of some bankers and emphasises the conflicts of interest that can exist within a bank? What if *T2* argues that, given the institutional conditions which are ignored by *T1*, it is all too rational to pursue power and personal wealth in the banking industry without having to worry too much about the costs of institutional failure? What if *T2* presents these factors as having played an important role in bringing about the insolvency of some banks?²⁴

Is it possible to critically explore the strengths and weaknesses of *T1* and *T2*? Would it assist if, under the weight of the account supplied by *T2*, the political institutions of an open society were to establish a *Parliamentary Commission on Banking Standards* with terms of reference to investigate ‘...the professional standards and culture of the UK banking sector’ and ‘... the lessons to be learned about corporate governance, transparency and conflicts of interest, and their implications for regulation and for Government policy’?²⁵

What I am suggesting is that to call ‘time’ on the philosophy of critical rationalism is really to call ‘time’ on the idea of the Open Society itself. And what I am also suggesting is that the corollaries of critical rationalism must be properly understood if they are not to be lost through neglect or complacency. This is important because the stakes are higher than some methodologists of economics seem able to appreciate. This is perhaps understandable given their neglect of the logic of ‘question and answer’, but it was with a surprising degree of complacency that Mark Blaug (1994) asserted that if criticism was all that his preferred methodology of ‘falsificationism’ amounted to then that was not very much. In fact he sarcastically declared that ‘...in the end we can say with perfect confidence that ‘we are all falsificationists now’ (Blaug, 1994, p. 114). But critical rationalists do not place their unqualified confidence in anything, let alone in the tolerance by others of the critical attitude itself. One does not need to be a philosopher to appreciate that the struggle to escape from the tutelage of authority and prejudice is an almost perpetual theme of human history.

It was, after all, the struggle that led to the trial of Socrates. Indeed, even in philosophy itself, I suspect that the real situation is better reflected by Leonard Nelson's (1949) remark:

A stepchild of philosophy, slighted and rejected, the Socratic method has survived only in name beside its more popular older sister, the more insinuating and more easily manipulated dogmatic method.

8. Conclusion

The pertinence of Karl Popper's philosophy to the methodology of economics has been much discussed and debated within the literature on the subject. In part those debates give testament to the methodological significance of the philosophical problems that Popper elected to address; but they are also a testament to the defective way in which methodologists have attended to the historical element in Popper's *oeuvre*. This historical element is especially significant to understanding a philosopher like Popper because his general approach to the problems that were of interest to him was to view them as fairly 'open': old problems could be revisited; old solutions could be improved; new solutions begat new problems. Overall, Popper's philosophy was imbued with the view that a real philosopher has philosophical problems, so to understand Popper's philosophy try and first understand the problem situation that his philosophy is addressing. Next, try and appreciate how the problem situation and his thinking about it might develop over time. This mode of philosophising has created difficulties for those economic methodologists who have attempted to interpret Popper's philosophy whilst paying insufficient regard to these considerations. This paper has attempted to address this deficit and thereby resolve some of the aforementioned debates.

Yet in many respects this paper is a job that is only half-done: it merely clears a path toward achieving a better understanding of the problems that *do* arise for the methodology of economics when it is viewed by the lights of Karl Popper's philosophy. Basically, it has arrived at the point at which others have began; namely '... with what Popper... has actually written about economics and other social sciences' (Hands, 1985, p. 84). Except that we now know that it pays to be attentive to

the problem situation that Popper was addressing at any moment in time, that his thoughts about a problem might develop over time, and that both of these factors may have a bearing on how the content of his philosophy is to be understood at any moment in time. *Praemonitus praemunitus*.²⁶

Notes

¹ Translated into English as Popper 1959 *The Logic of Scientific Discovery*.

² For a review, see chapter 1 of Miller (2006).

³ See chapter 7 fn.4 of Popper (1966A [1945]).

⁴ See chapter 17 fn. 22 of Popper (1966B [1945]). For a discussion of Popper's defence of state interventionism see Notturmo (2006) and Kerstenetzky (2007). For details of Popper's membership and involvement with the *Mont Pèlerin* society see Cockett (1994).

⁵ See *The Logic of Scientific Discovery* (2002A [1959/1934], §§ 1-6). In contrast to the Vienna Circle, and contrary to the so-called 'Popper legend', Popper did not assume that the problem of demarcating science from metaphysics was to be solved by the formulation of a so-called 'criterion of meaning' that demarcated sense from nonsense. For a discussion, see Popper (1974, p. 964). See also § 6 below.

⁶ Popper delivered a paper entitled '*Are there philosophical problems?*', but those present have differing recollections as to what was said, by whom, and whether or not a poker played an important role in the proceedings. See, for instance, Edmonds and Eidinow (2001), Munz (2004), Smiley (2004).

⁷ There was little disagreement between Popper and Adorno during their actual exchange of ideas at the 1961 congress of the German Sociological Association; but the tone changed when Adorno later augmented his contribution with a supplementary paper. Popper and Adorno's respective German disciples, Hans Albert and Jurgen Habermas, then exchanged commentaries thereby creating the so-

called '*Positivist Dispute in German Sociology*'. See chapter 6 of Popper (1994A) for his scathing subsequent commentary on the whole affair. See, also, chapters 13 and 14 of Fuller (2003) for a discussion of the background to the exchange.

⁸ Popper did not directly debate Thomas Kuhn. He occupied the Chair to an exchange involving Kuhn and several others. See Fuller (2003) for an account.

⁹ See, for example, §25 and appendix *x of Popper (2002A [1959/1934]).

¹⁰ Hacoen considers his own method as 'extending' what he calls the 'internal' theoretical problem situation so that it also considers 'external' political, social and cultural factors (2002 [2000], p. 20). He proposes (2002 [2000], p. 21) that this provides '... a fuller and more accurate account', but that he does not regard the two methods as 'challenging' one another (2002 [2000], p. 20)

¹¹ Hacoen's reconstruction of Popper's early intellectual history, for all its fascinating background detail on the milieu of Interwar Vienna, is controversial. See, for instance, the criticisms offered by Popper's some-time editors Troels Eggers Hansen (Hacoen, 2002 [2000], p. 198 fn. 82) and Mark A. Notturmo (2002).

¹² An allusion to the title of Leijonhufvud (1968). Cp. Paul Davidson's (2009, p. 38) observation that '... "Keynesian" economics... has no connection with Keynes's revolutionary analysis'.

¹³ For instance, in empirical research, how can statements that report individual situations be used to make valid inferences about situations yet to be observed, or to all the possible situations that might be observed?

¹⁴ See, for example, appendix *1 of Popper (2002A [1959/1934]). This reproduces Popper's 1933 letter to the editor of *Erkenntnis*—a journal associated with the members of the Vienna Circle. The letter critically contrasts Popper's proposed solutions to the problems of induction and demarcation

with the views of the leading members of the Circle. For an account of Popper's relationship to the Vienna Circle that is supplied by one of the Circle's members, see Kraft (1974). See also § 6 below.

¹⁵ For a detailed discussion see J.W.N Watkins (1957). I thank David Miller for drawing my attention to this article. For a highly accessible illustration see chapter 21 of Nicholas Fearn (2001); for applications to economic theorisation see J. Agassi (1971).

¹⁶ Popper (1983, p. xxxi) claimed that he never used the label 'falsificationism' to describe his position. This is because of its tendency to confuse the *falsifiability* of a statement (which is a logical property) with its empirical *falsification* (which is an empirical matter requiring a decision to accept that a statement is false). The conflation of *falsifiability* with *falsification* eases the argument that Popper overlooked the problem that a statement can never be falsified conclusively. That is, the argument that he was a 'naïve falsificationist'. Popper (1983, Introduction) attributes the argument that he was a 'naïve falsificationist' to Thomas Kuhn and Imre Lakatos.

¹⁷ See also fn. 5 and fn. 14 above.

¹⁸ Cf. the title of the seminar paper that caused Popper's legendary exchange with Wittgenstein: '*Are there philosophical problems?*' in fn.6 above.

¹⁹ See also Popper (1966A [1945A]) chapter 5 fn.5. I thank David Miller for drawing my attention to this footnote.

²⁰ Popper supplemented this principal proposal with many other proposed methodological rules. See, for example, Popper (2002A [1959/1934]) §11, §12, §20, §30. See Miller (2007 [1998]) for a discussion of *Logik der Forschung* as a treatise of methodological proposals whose application requires active decision making.

²¹ Although written in the period 1951-56, at the time that *Logik der Forschung* was being translated into English, the publication of *The Postscript* was much delayed. See the editor's foreword to Popper (1983). The book's preface dates from 1956.

²² Reprinted as chapter 5 of Popper (1994A).

²³ Reprinted as chapter 8 of Popper (2002B [1963]).

²⁴ For further development of this criticism see Thomas (2012) and also the subsequent dialogue between Notturmo and Thomas (2013).

²⁵ In July 2012, the three main British political parties cooperated to establish a Parliamentary Commission on Banking Standards. See:

<http://www.parliament.uk/business/committees/committees-a-z/joint-select/professional-standards-in-the-banking-industry/news/call-for-evidence/> (accessed: 12 September 2012)

²⁶ Forewarned is forearmed.

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