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Discussion Article: Comments on João Pinheiro da Silva's paper: 'Popperian Hayek or Hayekian Popper?'

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The title of this article, 'Popperian Hayek or Hayekian Popper?' poses a question that I feel it never quite explores. Indeed, the short paper devotes less than half of its discussion to differences between Popper and Hayek. Its author, João Pinheiro da Silva, writes that 'It is obviously impossible to reduce Hayek's methodological proposal to a dozen pages' (Silva, p. 53). And he is obviously right about that. Silva tells us toward the end of his paper that we have a Hayekian Popper and not a Popperian Hayek. But I wish that he had actually explained why we have a Hayekian Popper instead of just stating that we do. Silva seems to believe that what made Popper's position Popperian is that it defended the unity of scientific method, or what he calls 'methodological monism'. But he does not adequately try to explain what Popper understood by the unity of scientific method and how it differed from what others have meant by that phrase. And this is significant. Popper told us that he agreed with John Stuart Mill, Auguste Comte and Carl Menger that the methods of the theoretical natural sciences and the theoretical social sciences are fundamentally the same. But he also told us that the methods that he had in mind might differ from the methods that they had in mind. So it is not at all obvious that Popper meant the same thing that most others who have talked about the unity of scientific method have meant by that phrase – or that he defended it for the same reasons. It would have been good if Silva had devoted more time to exploring such issues. And in what follows I will try to do so myself.

Let me begin by saying that Comte, Mill, the logical positivists, and most others who have argued for the unity of scientific method, understood scientific knowledge to be certain knowledge and scientific method to be a process of inductively inferring universal generalisations from observations of particular events. The Scientific Method, for them, meant the inductive method. The unity of method meant that the natural sciences and the social sciences both use, or should use, the inductive method. And it went hand in glove with the unity of science thesis – the idea that all of science can be based upon and reduced to the same epistemological foundations (usually understood as physics) – with the idea that scientific theories can be known with certainty, of one form or another, and with the idea that theories in the social sciences can be known with the same certainty as theories in the natural sciences. We know from Hayek's own writings that this is how he originally understood the method of natural science, and that his critique of scientism was that the social sciences were trying to use the inductive method in areas it doesn't belong. But this is not how Popper understood scientific method or the unity of scientific method. And Popper underscored the point by writing,

> 'Professor Hayek uses the term "scientism" as a name for "the slavish imitation of the method and language of science". Here it is used, rather, as a name for the imitation of *what certain people mistake* for the method and language of science' (Popper, 1957, p. 105).

Popper wrote that the methods of science:

> '… always consist in offering deductive causal explanations, and in testing them (by way of predictions). This has sometimes been called the hypothetical-deductive method, or more often the method of hypothesis, for it does not achieve absolute certainty for any of the scientific statements which it tests; rather, these statements always retain the character of tentative hypotheses, even though their character of tentativeness may cease to be obvious after they have passed a great number of severe tests.
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> Because of their tentative or provisional character, hypotheses were considered by most students of method, as provisional in the sense that they have ultimately to be replaced by proved theories (or at least by theories which can be proved to be 'highly probable', in the sense of some calculus of probabilities). I believe that this view is mistaken and that it leads to a host of entirely unnecessary difficulties' (Popper, 1957, p. 131).

Popper, in fact, taught that there is no such thing as the scientific method over and above the method of conjecture and refutation, or trial and error. He taught that all scientific knowledge is hypothesis, or conjecture, or guesswork – and that scientific knowledge is different from *mere* guesswork not because it is justified, or certain, or has been shown to be true, or probable, but because it can be *tested*, and because we actually do try to test it as best we can. And he held this to be true of both the natural theoretical sciences and the social theoretical sciences. It is in this ironic sense, *and only in this ironic sense*, that Popper upheld the unity of scientific method.

I call it ironic, because Popper, unlike proponents of induction who defended the unity of scientific method, did not defend it in order to bolster the 'scientific status' of the social sciences, or the cognitive authority of the social sciences, or the possibility of achieving scientific certainty in the social sciences. Contrary to what Silva's use of the term 'methodological monism' might suggest, Popper generally thought that scientists can use whatever methods they think will help them to solve their problems, so long as they use them critically and in an effort to discover truth. And he wrote that

> 'As a rule, I begin my lectures on Scientific Method by telling my students that scientific method does not exist. I add that I ought to know, having been, for a time at least, the one and only professor of this non-existent subject within the British Commonwealth' (Popper, 1983, p. 5).

Popper taught that science begins with problems and with hypotheses, or guesses, about how to solve them. He taught that what makes science 'science' is not that our hypotheses are certain, or true, or made in accordance with certain methods, but that we can test them to find and eliminate their errors. And he taught that our tests never verify or even confirm our hypotheses, but typically lead to new problems. He developed his so-called tetradic schema—$P_1 \rightarrow TT \rightarrow EE \rightarrow P_2$—to encapsulate the idea. Here, P_1 is a problem, *TT* is a theory tentatively offered to solve it, *EE* is our attempt to eliminate the errors in *TT* via experiment and criticism, and P_2 is a new problem that arises from *EE*. Popper thought that this is the 'method' of all science, be it natural science or social science, and indeed the method of all rational discussion. It is, once again, in this ironic sense, *and only in this ironic sense*, that he upheld the unity of method. And it might, for this reason, be more appropriate to characterise Popper as upholding methodological relativism than methodological monism.

Silva, however, seems to think that 'methodological monism' involves a lot more than this. He writes: 'I intend, in this essay, to delineate Hayek's methodological dualism through the concept of "spontaneous order", which I believe makes him immune to any pretension of scientific unity and thus immune to Popperian falsificationism' (Silva, p. 46). But while he is certainly right that Hayek's ideas about undesigned spontaneous orders are both important and key to understanding his philosophy, he never says why methodological monism is so bad, or why it is at odds with Hayek's concept of spontaneous order, or how that concept would make one immune to it – let alone immune to falsificationism. And what he does say not only seems to conflate the idea that the social sciences and the natural sciences use the same method (the unity of method thesis) with the idea that the natural sciences and the social sciences are themselves one and the same (the unity of science thesis). It also suggests that this conflation is somehow necessary for Popper's falsificationism. I wish that Silva had said more about this, since I don't see any reason to believe either contention. Indeed, I do not understand what it would mean for Hayek, or anyone else, to be *immune* to Popperian falsificationism – which in turn raises questions about how many different kinds of falsificationism there might be aside from Popper's.

Be this as it may, my own sense is that Popper's defense of the unity of scientific method is not so strict or absolute as Silva seems to think. It should be clear that Popper, in defending the unity of scientific method, never meant to deny that there are differences, and indeed very important differences, between the methods of the natural sciences and the methods of the social sciences. For Popper not only tells us this himself, he also talks about such differences in the very context of arguing for the unity of method. He thus writes:

> 'I do not intend to assert that there are no differences whatever between the methods of the theoretical sciences of nature and of society; such differences clearly exist, even between the various natural sciences themselves, as well as between the various social sciences' (Popper, 1957, p. 131).

And he later identifies the fact that the social sciences can use the so-called 'zero-method' – the method of logical or rational construction, part and parcel of situational logic with its assumption of complete rationality and complete information – as the most important difference in the methods of the natural and social sciences (Popper, 1957, p. 141).

I find other things that Silva says misleading, and sometimes simply false. He thus writes that: 'While Popper was attending Vienna's positivist circles, Hayek attended the seminars of some of the fiercest critics of positivism' (Silva, p. 54). This, I take it, is supposed to suggest that Popper himself was a proponent of positivism and that Hayek was a critic of positivism. But if I am right about this, then I find it misleading in at least two ways. For Popper, on the one hand, was never a member of the Vienna Circle – which was essentially Moritz Schlick's private seminar – because he was never invited to attend its meetings. And Popper, on the other hand, actually *was* one of positivism's fiercest critics. This, indeed, seems to be one of the reasons why Popper was never invited to attend any of the meetings of the Vienna Circle. Members such as Moritz Schlick and Otto Neurath were not only skeptical of his views, they regarded him as 'the official opposition'.

Silva also writes: 'given [Hayek's] contact with Karl Popper, several historians and philosophers have characterised his final position as Popperian, that is, a position that would have accepted the unity of the scientific method' (Silva, p. 46). Does this mean that accepting the unity of scientific method – as opposed, say, to accepting fallibilism, or our inability to prove the truth of scientific theories, or the idea that scientific theories cannot and need not be justified – is what makes a philosopher or a position Popperian? So that Mill and Comte and

Menger were all Popperians because they accepted the unity of scientific method, even though they disagreed with Popper about what scientific method actually is, or why the methods of the social sciences and the methods of the natural sciences are fundamentally the same?

Silva also writes: 'In defending monism and the need to seek causal explanations, Popper automatically includes predictions of specific events as part of the scientific work given the logical equivalence between explanation and prediction and the need to subject theories to evidence' (Silva, pp. 55-56). But science, for Popper, is not about predicting the future. It is about explaining what we do not understand. And we try to explain what we do not understand by showing that and how it follows logically from certain laws and initial conditions that we do understand. Here, the point to be made is that Popper thought that predictions of specific events are part of scientific work not because the aim of science is to predict future events but because predicting future events can provide a way of testing our explanatory theories. If we can deduce a prediction from an explanatory theory and certain initial conditions, then we can test that theory by looking to see whether or not that prediction comes true. But contrary to what Silva seems to suggest, there is no *need* to falsify theories. I do not understand why he thinks that there is. We would certainly not want, let alone need, to falsify a theory if it is *true*. But we do want a theory to be *falsifiable*. For if a theory is not falsifiable, then there is no way test whether it is true.

These are just a few of the problems that I see in Silva's paper. There are others, Silva, writes: 'The product of this discussion is clear and results, not in a more Popperian Hayek, but in a Hayekian Popper' (Silva, p. 57). I think that what he means by this is that Hayek influenced Popper, at least about the unity of scientific methods, more than Popper influenced Hayek. I do not think that the product of his discussion is clear at all, or that it clearly results in a Hayekian Popper. Silva wants to say that Popper became more Hayekian in his later years by softening his methodological monism. He writes that

> 'Popper's defense of methodological monism gradually weakens so that in his essays of the late 1960s he uses concepts such as situational analysis and objective comprehension, which outline a methodological dualism that is the result of an understanding of the singular objects in the social sciences' (Silva, p. 57).

But Popper uses the concept of situational analysis, situational logic, and the logic of the situation in many if not most of his discussions about social science. He uses it, more specifically, in *The Open Society and Its Enemies* and, indeed, in *The Poverty of Historicism* – and he uses it in the very context in which he defends the unity of scientific method. Both of these books can be traced, at least, to the 1940s. So I don't quite see how Silva's idea is supposed to work.

Nor do I see that or how Popper softened his methodological monism. And quite aside from this, my own sense is that it was Hayek who softened his opposition to the idea that the natural sciences and the social sciences use more or less the same methods, and that he did so in changing his understanding of scientism and consciously under the influence of Popper.

This is something that I think Silva knows, for he tells us that Hayek first used the term 'scientism' to refer to 'a slavish imitation of the method and language of Science' and that he later softened his critique of scientism when he recognised that the method and language that some social scientists were slavishly imitating were not actually the method and language of natural science.

And we know that Hayek consciously changed this view under the influence of Popper because Hayek himself tells us so. Thus, in the preface to *Studies in Philosophy, Politics and Economics*, Hayek wrote that:

> 'Readers of some of my earlier writing may notice a slight change in the tone of my discussion of the attitude which I then called 'scientism'. The reason for this is that Sir Karl Popper has taught me that natural scientists did not really do what most of them not only told us that they did, but also urged the representatives of other disciplines to imitate. The difference between the two groups of disciplines has thereby been greatly narrowed and I keep up the argument only because so many social scientists are still trying to imitate what they wrongly believe to be the methods of natural sciences. The intellectual debt which I owe to this old friend for having taught me this is but one of many, and it is therefore only appropriate that this volume should be in gratitude inscribed to him' (Hayek, 1967, p. 2).

Here, the point to be made is that Hayek softened his critique of scientism because he came to recognise that his ideas about the methods of the natural sciences were mistaken. More specifically, he came to recognise that the natural sciences do not adhere to the inductive method of observation and generalisation, contrary to what many positivists claimed. Hayek tells us that Karl Popper taught him this. But he also recognised that if his ideas about the methods of the natural sciences are false, then his idea that the methods of the social sciences are fundamentally different from those of the natural sciences is also likely to be false. This is what Hayek means when he says that 'The difference between the two groups of disciplines has thereby been greatly narrowed'. And this, simply put, means that the differences between the methods of the natural sciences and the methods of the social sciences are not as great as he had thought.

Silva seems to recognise this possibility. But he also immediately rejects it. He writes:

> 'In reformulating his understanding of the method of the physical sciences, [Hayek] realises that their method is actually closer to that which he proposes to the social sciences than he thought. Does this mean that Hayek comes to conceive of the unity of the method? No. Hayek will continue to reiterate that the object of study of the social sciences are spontaneous orders and that the method involved in explaining phenomena with such order of complexity can never be confused with that of the physical sciences. Hayek only acknowledges that the differences between the two disciplinary groups have narrowed, but never asserts the unity of the method' (Silva, p. 57).

But my own sense is that it does indeed mean that Hayek came to conceive of the unity of method, at least as Popper understood it, and that he did so regardless of whether he ever explicitly asserted it.

I think that this is the way Hayek saw it too. He told James Buchanan in an interview in 1978 that he and Popper 'became very close friends' in the years they were together at the LSE – 1945 to 1950 – and that 'we see completely eye-to-eye on practically all issues' (Hayek 1978). And he told other interviewers, late in life, that he and Popper were very close friends and that 'to a very large extent I have agreed with him, although not always immediately' – adding that 'on the whole I agree with him more than with anybody else on philosophical matters' (Hayek, 1994, p. 51).

My own sense is that it is this, as opposed to any misconceptions that they may have had about the importance and centrality of Hayek's concept of spontaneous order, that have led many philosophers, historians and economists to understand his 'final position' as Popperian.

Works Cited

Silva, João Pinheiro da (2021) 'Popperian Hayek or Hayekian Popper?' *Economic Thought*.

Hayek, Friedrich von (1967) *Studies in Philosophy, Politics and Economics*. University of Chicago Press, Chicago.

Hayek, Friedrich von (1978) *The Hayek Interview*s. Pacific Academy for Advanced Studies. Downloaded from http://www.hayek.ufm.edu.

Hayek, Friedrich von (1994) *Hayek on Hayek*. Edited by Stephen Kresge and Leif Wenar. University of Chicago Press, Chicago.

Popper, Karl R. (1957) *The Poverty of Historicism*. Routledge, London.

Popper, Karl R. (1983) *Realism and the Aim of Science*. Routledge, London.

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