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Keynes, Kuhn and the Sociology of Knowledge: a comment on Pernecky and Wojick

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Abstract

Mark Pernecky and Paul Wojick use T.S. Kuhn's philosophy of science to diagnose '*The problematic nature and consequences of the effort to force Keynes into the conceptual cul-de-sac of Walrasian economics*'. But their diagnosis is itself problematical in nature and consequence. It assumes the virtues of a pre-Kuhnian philosophy of knowledge that the Kuhnian meta-framework overtly discards. One way to eliminate the inconsistency is to recognise that Kuhn's philosophy of science and sociology of knowledge function to immunise theories from criticism. Anyone who wishes to embrace a sociologically more critical philosophy ought to consider instead the philosophical attitude of critical rationalism.

Key words: Keynes, Kuhn, Popper, Paradigm, Sociology of Knowledge

JEL Classification: B41

The prisons are the frameworks. And those who do not like prisons will be opposed to the myth of the framework.

Karl Popper (1994, p. 52)

1. Introduction

This article comments upon ‘The problematic nature and consequences of the effort to force Keynes into the conceptual cul-de-sac of Walrasian economics’ (Pernecky and Wojick, 2019). It aims to explain why Pernecky and Wojick’s use of T.S Kuhn’s (1996 [1962]) philosophy of science is itself problematical in both nature and consequence. [An alternative approach that is based upon K.R. Popper’s philosophy of critical rationalism is proposed.](#)

2. Pernecky and Wojick’s diagnosis: Keynes as a casualty of paradigm incommensurability

The thrust of Pernecky and Wojick’s (*P&W*; 2019) diagnosis is that the original and revolutionary insights of Keynes’s (1936) *General Theory* have been lost on incorporation into mainstream macroeconomics. *P&W* (2019, p. 769) attribute this to ‘Kuhnian incommensurability’. The variants of Walrasian General Equilibrium (WGE) modelling represent the orthodox paradigm of economics; the theories and concepts of Keynes’s *General Theory* belong to a different paradigm. In accordance with Kuhn’s (1996 [1962]) philosophy of science, *P&W* declare different paradigms to be ‘*largely if not wholly incommensurate*’ (Pernecky and Wojick, 2019, p. 771, emphasis added). What this qualification entails precisely, *P&W* leave a little moot, much like Kuhn’s own discussion of his usage of the term ‘incommensurate’ as not quite meaning ‘incomparable’, but being more suggestive of the ‘painful’ and ‘grave difficulties’ presented by foreign language translation (Kuhn 1974A [1970], p. 267).¹ Nevertheless, to demonstrate their diagnosis, *P&W* (2019, p. 770) identify and discuss several instances of Keynes’s ideas being ‘ignored’, or ‘misrepresented’, or ‘mistranslated’—for these are ways to accommodate their incommensurability with WGE.² Keynes is thereby forced into the ‘Walrasian cul-de-sac’—a system of thought that explores not ‘the economic society in which we actually live’ (Keynes, 1936, p. 3), but ‘the economic society that we might prefer to imagine’ (Pernecky and Wojick, 2019, p. 771). This has problematical consequences because it prevents much

of the economics profession ‘developing a better understanding of the functioning, and malfunctioning, of market systems’; they fail thereby to identify crucial policy prescriptions (Pernecky and Wojick, 2019, p. 769).

3. The problematical nature of the diagnosis

Now there is something of an unacknowledged problem in the nature of this diagnosis. Let me explain why.

In Kuhn’s philosophy of science, ‘a paradigm is what members of a scientific community share, *and*, conversely, a scientific community consists of men [sic] who share a paradigm’ (Kuhn, 1996, p. 176, emphasis in original). In this sense Kuhn (1996, pp. 175-176) acknowledged that his usage of the term was both ‘intrinsically circular’ and ‘sociological’. In addition, he illustrated this by defining what a given circle encloses—‘it stands for the entire constellation of beliefs, values, techniques, and so on shared by members of a given community’ (Kuhn, 1996, p. 175). And of course, by necessity what a circle does not enclose it must exclude—Kuhn wrote that ‘the proponents of competing paradigms must fail to make complete contact with each other’s viewpoints’ (Kuhn, 1996, p. 148), and even that ‘the choice between... competing paradigms proves to be a choice between incompatible modes of community life’ (Kuhn, 1996, p. 94). Kuhn (1996, p. 148) gave several reasons for such paradigm ‘incommensurability’. He said that ‘the proponents of different paradigms will often disagree about the list of problems that any candidate for paradigm must resolve’, or that their ‘standards or definitions of science are not the same’ (Kuhn, 1996, p. 148), or that the textbook ‘exemplars’ of scientific achievement differ between the communities (Kuhn, 1996, p. 187). Indeed, just as different language communities may use the same words to denote different things, Kuhn (1996, p. 149) proposed that different scientific communities may use ostensibly equivalent theoretical terms in ways that possess different conceptual meanings. In figurative terms, Kuhnian incommensurability supposedly arises because ‘the proponents of competing paradigms practice

their trades in different worlds' (Kuhn, 1996, p. 150). Thus Kuhn wrote that 'the proponents of competing paradigms are always at least slightly at cross-purposes', and that 'the competition between paradigms is not the sort of battle that can be resolved by proofs' (1996, p. 148); '... debate is about premises, and its recourse is to persuasion as a prelude to the possibility of proof' (Kuhn, 1996, p. 199). In short '... it is precisely the abandonment of critical discourse that marks the transition to a science' (Kuhn, 1974B [1970], p. 6).

Boldly stated, Kuhn's philosophy of science is an exemplar of the *sociology of knowledge*, or the idea that the content of knowledge is somehow a function of social experience, or individual or group interests, or a background framework that somehow structures how a person thinks. Stronger still, it is a *sociologising philosophy* that largely presents knowledge as something that is structured by a relationship between the knower and their community. The strongest term, which has often been used to classify Kuhn's philosophy, is that it is implicitly a species of *epistemological relativism* (Popper, 1974 [1970], 1994; Wisdom, 1975, 1987; Notturmo, 1984; Munz, 1985; Gellner, 1974, 1985; Phillips, 1987; Bartley, 1990; Gregory, 1994).³

But if this is the basis to Kuhn's notion of paradigm incommensurability, then how are *P&W* not only able to compare the merits of the supposedly 'incommensurable' paradigms of Keynes and WGE, but also able to declare Keynes's paradigm as superior? According to Kuhn, the proponents of competing paradigms are always at least slightly at *cross-purposes*, facing *grave difficulties* of inter-paradigmatic translation, even practising their trades in *different worlds*. In contrast, *P&W* are able to move effortlessly between paradigms, making a complete contact with the WGE viewpoint and revealing it to be replete with ignorance, misrepresentation and mistranslation. But those kinds of judgement require the *comparison* of supposedly incommensurate paradigms. *More than that even, if P&W are to conduct an inter-paradigmatic comparison within a strictly Kuhnian framework, they require a meta-paradigm within which both WGE and Keynes may be evaluated methodologically (Gellner, 1985); yet ex-hypothesi they do not possess one.*

In his own attempt to reshape the philosophy of science, Kuhn himself could not avoid these difficulties. Thus, in his *'The Structure of Scientific Revolutions'*, it is not without reason that the opening chapter is entitled *'A Role for History'*. For according to Kuhn, only *the history of a science* and its so-called 'revolutionary' episodes signify its development (Kuhn, 1996, §1, §IV, §IX, Postscript). Outside of such extraordinary periods, the normal scientific problem is to reconcile the results of all and any research with the prevalent paradigm—what Kuhn called the 'puzzle-solving' of 'Normal Science' (Kuhn, 1996, §IV). Moreover, the extraordinary event of the paradigm shift supposedly only happens to a scientist 'not by deliberation and interpretation, but by a relatively sudden and unstructured event like the gestalt switch' (Kuhn, 1996, p. 122). For this reason, Kuhn's notion of the paradigm shift is considered by some to be a-rational or even implicitly a species of anti-rationalism or irrationalism (Wisdom, 1975; Munz, 1985; Phillips, 1987; Bartley, 1990; Popper, 1994).⁴

Nevertheless, it was the historical identification of such episodes that enabled Kuhn, rather conveniently, to break out from the otherwise closed circle of the Kuhnian sociology of knowledge and attempt to reshape the philosophy of science itself. The facts of scientific history, he found, are able to speak for themselves, uninfluenced by a theory or paradigm shaping their selection, whereas all other facts are theory-laden and housed in paradigms!⁵ As Munz (1985, pp. 116-119) writes:

... when it comes to history, Kuhn is not a Kuhnian... He allows historical examples in the history of science the absolute bite which he denies to all other facts of nature so called. History, so it seems, is for Kuhn the one exception. Here, for once, in the entire realm of human knowledge, we have a set of hard and brute facts that invite, indeed compel, us rationally to give up old paradigms for the philosophy of science advocated by Kuhn.

My point in detailing this controversial feature of Kuhn's philosophy is to highlight that P&W cannot use a similar ploy; they cannot follow in the footsteps of Kuhn and treat the history of economic ideas as a series of ready-made facts that are available for paradigm-free inspection. For not only do they explicitly write that '*all* facts are theory laden' (Pernecky and Wojick, 2019, p. 771, emphasis added), they also write that '*...the immense appeal of Walrasian General Equilibrium (WGE)...*

prevented the revolution that Keynes envisioned from becoming a revolution in the classic Kuhnian sense' (Pernecky and Wojick, 2019, p. 770).

Thus, there is a contradiction in *P&W's* Kuhnian inspired diagnosis: if *P&W* are right in declaring Keynes's ideas to be superior, then they must be wrong that Keynes and WGE represent incommensurable paradigms. Alternatively, do *P&W* blithely accept Kuhnian notions whilst failing to recognise that to do so is to contradict their own diagnosis?

The answer to this question resides in *P&W's* (2019, §3) analysis of the competing paradigm's conceptualisations of 'rigor'. There, we find a judgement in Keynes's favour that the Kuhnian meta-framework ought philosophically to rule out of court. In particular, *P&W* argue that the concept of 'rigor' has a fundamentally different meaning in the two paradigms. They argue that for the affiliates of the WGE paradigm, 'rigor' is exemplified by the deduction of theorems from the axiomatic postulates of WGE, in the style of the 'logical-mathematical sciences' and with the highest degrees of 'mathematical sophistication' befitting that community of practice (Pernecky and Wojick, 2019, p. 773). But in their comparison of this conceptualisation to that of Keynes, *P&W* (2019, pp. 772-773) write:

While Keynes engages in a significant degree of deduction, the postulates he uses correspond much more closely with reality, and thus contain more 'rigor' as scientists have historically used the term... In Keynes's methodology, the postulates which form the basis for deduction follow from inductive inferences made from that which is empirical. As Keynes writes '[O]ur knowledge of propositions tends to be obtained in two ways: directly, as the result of contemplating the objects of acquaintance; and indirectly *by argument...*' (Keynes, 1921, p. 12). 'That part of our knowledge which we obtain directly, supplies the premises of that part which we obtain by argument. From these premises we seek to justify some degree of rational belief about all sorts of conclusions' (Keynes, 1921, p. 111).

It is this that enables *P&W's* judgment that '...the methodological approach of Keynes *more closely resembles that of the sciences...* than the approach associated with WGE' (Pernecky and Wojick, 2019, p. 773, emphasis added). But clearly, this judgment is not anchored to Kuhnian philosophical moorings. It is inconsistent with Kuhnianism and made possible only by adopting a *pre-Kuhnian*

philosophy of science; namely, the inductive-empiricism inaugurated by Francis Bacon and developed by the British empiricists (Priest, 1990). By the first decade of the twentieth century this philosophy had come to be formulated by Keynes's Cambridge colleague, Bertrand Russell, into a whole series of epistemological doctrines as to how the generalised knowledge of science was both psychologically derived and logically inferred from the supposedly 'primitive certainty' of acquaintance with sense data (Russell, 1964 [1912], p. 19). After Russell, the Vienna Circle's logical positivists developed the empiricist project into various doctrines, most famously the so-called 'verification principle': that an empirical statement's truth or falsity is contingent upon experience. In turn, the Vienna Circle's doctrines were famously criticised by Karl Popper.⁶ Kuhn's (1974b [1970]) paper in *Criticism and the Growth of Knowledge* ought to be read by these lights: it is a comparison and juxtaposition of his views with those of Popper and the earlier doctrines of the logical positivists. This is not the place to delve into all of that, but suffice to say that for Popper and Kuhn empirical statements are theory laden and decidedly not founded upon a primitive certainty given by sensation.⁷ In any event, *P&W* have no qualms as to whether a logically invalid inductive inference can supply *any* degree of rational justification to an argumentative conclusion of *any* sort whatsoever. This also demonstrates that their judgement relies upon a pre-Kuhnian philosophy. Kuhn, in contrast, when comparing his philosophy of science to that of Sir Karl Popper, wrote that:

...neither Sir Karl nor I is an inductivist. We do not believe that there are rules for inducing correct theories from facts, or even that theories, correct or incorrect, are induced at all. Instead we view them as imaginative posits, invented in one piece for application to nature' (Kuhn (1974B [1970], p. 12).

To sum up: *P&W* introduce Kuhn's paradigm concept as if it came from nowhere, but it was actually a response to explicit problems in the philosophy of science. This omission enables *P&W*'s (2019, p. 773) judgement that '...the methodological approach of Keynes more closely resembles that of the sciences'. That judgement, however, relies upon the inductive empiricist philosophy of science that Kuhn overtly discarded. What is more, it is rendered without recognising the sociology

of knowledge and attendant epistemological relativism which Kuhn put in its place (albeit with ‘the facts’ of history being conveniently somehow excepted).

4. The problematical consequences of Kuhn’s Sociology of Knowledge

A judicious weeding and pruning of *P&W*’s framework, however, can eliminate the problematical nature of their diagnosis. Central to such an endeavour would be a new question for the sociology of knowledge: what are the implications of the Kuhnian philosophy of science for a community that is genuinely interested in the critical discussion of its own values, standards, assumptions or beliefs?

I venture to suggest that the embracement of the Kuhnian philosophy of science has its own problematical consequence: it functions to immunise traditional values, standards, assumptions or beliefs from criticism. For Kuhn (1996 [1962]) claimed that the foundational assumptions of a physical science are never directly amenable to critical assessment without something crucial being ‘lost in translation’. And he claimed that the foundational assumptions change only when the brute facts of history declare that they have altered—something which happens only when scientists undergo peculiar gestalt experiences. But if all this is claimed of the physical sciences, then what prospect is there for criticising *any* community that structurally justifies knowledge in the same way?

5. Critical Rationalism and ‘the myth of the framework’

Thus, if *this* is what Kuhn’s philosophy of science and doctrine of paradigm incommensurability maintains, then why should a man with the critical ambitions of J.M. Keynes embrace it? The rigors of deductivism and empiricism may be reconciled if we propose, following Popper, that the business of empirical science is not to justify or prove the theories and assumptions that we entertain, but to probe their logical implications so that they may be *critically* compared for consistency with the reports of our experience (Popper, 2002A [1959], §27).

Here, it is important to understand that both Kuhn and Popper were opponents of logical positivism's verificationist epistemology and inductivist logic.⁸ But Popper was also an opponent of Kuhn's highly sociologised philosophy of science and its attempt to justify positively scientific knowledge by reference to the supposed consensus paradigm of the scientific community. For Popper, *all* variants of justificationist epistemology share a structural problem: to avoid an infinite regress of positive rational justifications they have to place an arbitrary limit on rational discussion by installing, beyond the reach of criticism, some foundational authority that can judge the veracity of a statement or theory. The logical positivists' verification principle coupled to inductive logic was one variant of *so-called justificationism*—and as noted in §3, both Popper and Kuhn rejected inductivism and the thesis that knowledge has a firm and indubitable 'bedrock' foundation in primitive experience. But the Kuhnian notion of being committed to a community of practice and its paradigm is another variant of justificationism: a *dogmatic* variant in which the 'foundations' to knowledge are set by the authority of the established community *and are thereby relative to it*. Recall: '*... it is precisely the abandonment of critical discourse that marks the transition to a science*' (Kuhn 1974B [1970], p. 6). Mark Notturmo (2003, p. 11) memorably dubs this the thesis of 'floating foundationalism... since it retains the foundationalist theory of rationality and its demand for justification by logical argument, but leaves the foundations themselves floating in midair'.⁹

Popper (1974B [1970], 1994) also considered Kuhn's philosophy of science, when stripped of its sociological and psychological trappings, to be a mistaken logical thesis: a thesis that either leads to dogmatism or relativism. He dubbed the mistake 'the myth of the framework':

The myth of the framework is... the doctrine that one cannot rationally discuss anything that is fundamental, or that a rational discussion of principles is impossible. This doctrine is, logically, an outcome of the mistaken view that all rational discussion must start from some principles or, as they are often called axioms, which must be accepted dogmatically if we wish to avoid an infinite regress—a regress due to the alleged fact that when rationally discussing the validity of our principles or axioms we must again appeal to principles or axioms. Usually those who have seen this situation either insist dogmatically upon the truth of a framework of principles or axioms, or they become relativists: they say that there are different frameworks and that there is no discussion between them and thus no rational choice. But all this is mistaken. For behind it there is the tacit

assumption that a rational discussion must have the character of a justification, or of a proof... or of a logical derivation from admitted premises. But... there is also another kind of rational discussion: a critical discussion... it consists in comparing the consequences of different theories... or frameworks and tries to find out which of the competing theories or frameworks have consequences that seem preferable to us (Popper, 1994, pp. 59-60).

By this means, the attitude of *critical rationalism* becomes the prelude to the possibility of persuasion, but only for those persons whose minds are open to criticism and the possibility of personal fallibility in their choice of initial assumptions. Indeed, one might be so bold as to entertain the idea that J.M. Keynes was such a person. Not least, because *in some places* he more or less said so. Admittedly, Keynes (1936, p. viii) wrote of his 'long struggle... of escape from habitual modes of thought and expression' and he recognised the difficulties faced by those brought up on old ideas which 'ramify... into every corner of our minds'. *All of that is reminiscent of Kuhnianism*, but he wrote also that escape was made possible by 'a highly abstract argument and also by much controversy', by 'new ideas', and 'by persuading economists to re-examine critically certain of their basic assumptions' (1936, pp. v-viii). Most notably, he understood that probing the valid implications of basic assumptions enables their critical examination. Thus he did not directly attack *or denounce* the premises of his opponents' arguments, nor did he wait for policy makers to experience a *gestalt* shift in their perception of them in relation to his. His assault, by *critical* argument, was upon the acceptability of the logical consequences that follow from his opponents' premises. That has the hallmarks of critical rationalism not Kuhnianism. *For in the former, premises are not really stipulated, Kuhnian style, by some mysteriously persuasive community, or accepted by some extra-rational event that resembles a gestalt-like conversion. In critical rationalism, premises are simply assumed or entertained so that their logical implications may be probed critically using whatever means we are clever enough to apply* (Klappholz and Agassi, 1959; Notturmo, 2000, 2014; Miller, 2006; Thomas, 2017). Although it would be absurd to claim that Keynes adopted Popper's philosophy of knowledge, important passages in Keynes's writings have the same character—they anticipate and pre-empt the critical rationalist philosophy:

Now I range myself with the heretics... But I was brought up in the citadel and I recognise its power and might. A large part of the established body of economic doctrine I cannot but accept as broadly correct. I do not doubt it. For me therefore it is impossible to rest satisfied until I can put my finger on the flaw in that part of the orthodox reasoning *which leads to the conclusions which for various reasons seem to me to be unacceptable*. I believe that I am on my way to do so (Keynes, 1973 [1934], p. 489, emphasis added).

The classical theorists resemble Euclidean geometers in a non-Euclidean world who, discovering that in experience straight lines apparently parallel often meet, rebuke the lines for not keeping straight—as the only remedy for the unfortunate collisions that are occurring. Yet, in truth, there is no remedy except to throw over the axiom of parallels and to work out a non-Euclidean geometry. Something similar is required to-day in economics (Keynes, 1936, p. 16).

The Popper scholar, David Miller, compares critical rationalism to [justificationist epistemologies](#) in these terms:

It is better to be unjustifiably right, Popper tells us, than to be unjustifiably wrong. This is just as well, since we can never be justified in what we think or do, but if we are lucky we can sometimes be right (though we never know when)... the elimination of false hypotheses cannot be justified, but that does not imply that it is not actual... There is no justified or secure basis to which we refer in this critical activity, but that does not mean we never succeed in eliminating errors (Miller, 2016, p. 76).

Someone may object that the comparison of the critical rationalist and Kuhnian philosophies is *itself* a matter of inter-paradigmatic debate in the philosophy of science. They may say that there is no escape from the Kuhnian paradigmatic approach at any level. In response, however, the critical rationalist might well *criticise* this thesis as mistaken, not least because it seems to be self-refuting: it concedes that inter-paradigmatic *debate* occurs, even at the most abstract philosophical level. How is such a debate possible if the positions are incommensurable? Indeed, it was Kuhn (1974B [1970], p. 1) himself who wrote, at the head of a twenty-three page paper comparing his views on scientific development to those of Popper, that ‘a disciplined comparison of the two may produce a peculiar enlightenment’.

And someone may say that to claim, following Miller (2016, p. 76), that ‘it is better to be unjustifiably right, than to be unjustifiably wrong’ looks suspiciously like the critical rationalist [philosopher](#) attempting to positively justify their philosophy after all. But as Miller (2006, p. 80)

notes, are critical rationalists wrong not to have abandoned truth; only the pretensions of **proof or** justified truth? What are the criticisms of this decision? Or even more philosophically, the critical rationalist may reply: 'I am open-minded about my attitude of open-mindedness, and that extends to being open-minded enough to consider seriously any criticisms you may have of **its implications**. So what are they?'¹⁰

On the other hand, what critical rationalism must concede is that the truly committed dogmatist need not adopt the attitude of critical rationalism and that no critical argument can force them to do so. Yet that is just another way of stating that logical argument does not have the power to compel assent and that the most a valid deductive argument can offer us is not a *proof*, but a *choice* between the truth of its conclusion and the falsity of one or more of its premises (Notturmo, 2000, 2014). One would hope, however, that WGE-inspired economists are generally in favour of choice.

6. Conclusion

P&W want to criticise the WGE framework and declare Keynes's as superior. Their adoption of the Kuhnian philosophy of science and sociology of knowledge, however, contradicts this venture: if *P&W* are right in declaring Keynes's ideas to be superior, then they must be wrong in thinking that Keynes and WGE present incommensurate paradigms. To by-pass this contradiction, *P&W* assume the virtues of a pre-Kuhnian philosophy of science and use it to contrast Keynes and WGE. But this resorts to a philosophy that their Kuhnian meta-framework overtly discards. This creates an inconsistency.

These problems, however, are easy to avoid if one recognises, *ab initio*, that the adoption of Kuhn's philosophy of science and sociology of knowledge has a problematic consequence: they function to immunise theories from criticism. Anyone who wishes to embrace a sociologically more critical philosophy would therefore oppose Kuhn's philosophy of science and sociology of knowledge. They might instead consider the philosophical attitude of critical rationalism. There is no

contradiction in doing this. Critical rationalism is a philosophical attitude that Keynes's own writings often implicitly and pre-emptively anticipate: he sought to use reason and argument as a means to escape from the power and might of classical orthodoxy and not as a means to demonstrate its impregnability.

It follows that *P&W's* critique of the way the proponents of WGE ignore, mistranslate and misrepresent Keynes's theories and ideas is itself an insightful form of *criticism*. It fits with Popper's explicit teaching that it is important to search systems of thought for the ways within them which could possibly be used to deflect criticism; the ways in which they could become 'reinforced dogmas' (Popper, 2002B [1963], p. 440; Bartley, 1984, p. 242). And crucially, *P&W's* diagnosis of the efforts to force Keynes into the conceptual cul-de-sac of Walrasian economics depends upon the Kuhnian notion of paradigm incommensurability *only* because *they say it does*. Simply put, they might more fruitfully have stated their argument and criticism of WGE without recourse to Kuhnian nomenclature and without issuing an open invitation to the proponents of WGE to respond: 'We do not understand you because you do not speak our language'.

To conclude, *Kuhn's incommensurability thesis is unhelpful to P&W's case*. Rival paradigms cannot be incompatible if they are *wholly* incommensurate because '... there would be no bar to accepting both of the "rival" paradigms for they are not really rivals!' (Phillips, 1987 p. 23). But on the other hand, if paradigms are commensurable, then the possibility for inter-paradigmatic discussion exists as does the prospect for a cross-paradigmatic understanding being developed. Critical arguments may then be deployed in the knowledge that they offer not proof, but rational choice. My own sense is that *P&W's* insightful analysis for the most part actually demonstrates this, as does some of their remarks about 'the mindset' of particular economists and their '*...willingness to set aside strongly held preconceptions*' (Pernecky and Wojick, 2019, p.774). In short, there are no reasons why those with a critical attitude need commit themselves to the Kuhnian prison and embrace the admittedly fashionable *myth of the framework*.

Notes

¹ 'Incommensurable' in my Oxford English Reference Dictionary (1996) is defined as 'having no common standard of measurement; not comparable in respect of magnitude or value'.

² Pernecky and Wojick (2019) discuss, to varying degrees, the reception and depiction of several of Keynes's key concepts and theories by those associated with the WGE orthodoxy; namely, uncertainty, involuntary unemployment, the role of nominal wages in price determination, equilibrium and the propensities toward self-adjustment in markets.

³ Ernest Gellner (1974, p. 178), for instance, writes: 'Kuhn's philosophy would seem inevitably to lead to a relativism and repudiation of empiricism. His own emphatic rejection of these inescapable implications of his views would seem to be a sincere enough expression of his personal temperament and position, but to lack much logical relationship and coherence with his central argument'. Cp. Kuhn (1974A [1970], §5).

⁴ For a detailed discussion of the issue see Munz (1985). Bartley (1990) offers a diagnosis as to why Kuhn's sociologised philosophy has been influential in the Western academy. Wisdom (1987, Ch.3) offers some conjectures as to why Kuhn's a-rational aspects hold special appeal to social scientists.

⁵ Cp. E.H. Carr's *What is History* (1962, p. 5): 'The facts speak only when the historian calls upon them: it is he who decides which facts to give to the floor and in what order or context'.

⁶ See, Ayer (1959) for classic statements of the Vienna Circle's philosophy and Popper 2002A [1959] for a critique of it.

⁷ Perhaps more accurately, Kuhn (1996 [1962], p. 126) hedged his position as partly depending upon the impact of his own philosophy: 'But is sensory experience fixed and neutral?... In the absence of developed alternatives, I find it impossible to relinquish entirely that viewpoint. Yet it no longer functions effectively, and the attempts to make it do so through the introduction of a neutral language of observation now seem to me to be hopeless'.

⁸ See §3 and fn. 6.

⁹ Notturmo (1984) is a carefully constructed examination of the similarities and differences between Popper and Kuhn's philosophies. See also Notturmo (2000, ch. 4).

¹⁰ For further discussions of critical rationalism, see, for example, Popper (2002B [1963] Introduction, ch.1); Miller (2006, chs. 2 and 3).

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