

Reply to Arne Heise's 'The incommensurability of Keynes's and Walrasian economics and the unsuccessful escape from old ideas'

Rod Thomas, Newcastle Business School, Northumbria University,
Newcastle upon Tyne, UK
rod.thomas2@btinternet.com

I thank Arne Heise for his commentary on my debate with Mark Pernecky and Paul Wojick (Pernecky and Wojick, 2019, 2020; Thomas, 2020). Here, I offer a reply to some of its content.

Let me begin by saying where I agree with Heise and the initial contribution of Pernecky and Wojick (*P&W*).

I agree that Keynes's writings, when compared to those derived from Walrasian general equilibrium (WGE) theorising, offer an alternative explanation of the functioning and malfunctioning of economic systems. I agree also that these alternative explanations are comprised of theoretical systems. And I agree that the economic systems whose behaviour they seek to explain are important to our experience of the real world – crudely put, it matters if a person finds themselves involuntarily unemployed and it matters to all of us if millions do. That is one reason why an economist has a theoretical interest in explaining the workings of economic systems. The theoretical systems so developed, however, are products of the ever-fallible human imagination and ought not to be confused with the real world; *P&W* write, for instance, of a '...theories direct relationship to reality' (Pernecky and Wojick, 2019, p. 772). I suspect also that we agree that both Keynes's system and WGE are simplified accounts of the workings of the real world; Heise (2021, p. 17), for instance, distinguishes conjectural knowledge from objective truth and refers to the 'verisimilitude' of a theoretical system, whereas *P&W* cite approvingly János Kornai's view that an acceptable theory must describe the real world '...more or less accurately' (Pernecky and Wojick, 2019, p. 773).

And I agree that since our theories about the world may also inform our actions in the world, then such theoretical systems also indirectly affect our experiences of the real world. Keynes, for instance, wrote about his 'agenda of government' (Keynes, 2015A, p. 55) and how '...the ideas of economists and political philosophers, both when they are right and when they are wrong, are more powerful than commonly understood' (Keynes, 2015B, p. 262). Most memorably, he wrote of the analogy between a 'newspaper beauty contest', where readers are asked to guess the readership's most favoured photographic beauty, and how professional investment may involve anticipating what average opinion expects the average opinion to be (Keynes, 2015B, p. 211). *P&W* (2019) acknowledge all this by stressing how theoretical systems inform policy prescriptions. Indeed, they go much further than that. They theorise that an economist may prefer a particular theoretical system, not because they think it is the best that the human mind has so

far conjectured, but because they have personally invested human capital in its development, or been trained in the application of its nostrums, or if they think it offers, via others, access to 'highly-regarded departments', 'prestigious journals', or 'prized circles of influence' (Pernecky and Wojick, 2019, p. 778-779); a beauty contest indeed. Heise (2021, p. 16, fn.12) similarly writes that theory choice may be based upon 'career perspectives'.

Thus, I share a concern wherever the scientific pursuit of truth becomes distracted by extra-scientific interests like personal fame, prestige, or position. As W.W. Bartley (1990) noted in his prescient and compelling analysis of this issue, the objection is not to self-interested behaviour, it is to the institutional frameworks that fail to channel it properly. Admittedly, in our present age it may appear rather quaint and old-fashioned, but I prefer it when the Academy channels its efforts toward the advancement of learning. Indeed, I think there is an important distinction to be made here, one that is increasingly becoming central to an understanding of our times, but one that *P&W* were unable to state clearly because of the Kuhnian philosophical framework that supposedly informed their analysis (more on which later).

Consequently, I agree that these are all reasons why the critical comparison of alternative theoretical systems ought to be of acute interest. Indeed, my own sense is that it was this idea – that the critical appraisal and comparison of theoretical systems is important – that motivated *P&W* (2019) to write their paper, just as I think it also motivated Keynes to develop his theoretical system as an alternative to the orthodoxy of his times. I think we agree on all this. And I think that we agree that the critical comparison of these theoretical systems is not going to be assisted by any tendency to ignore, misrepresent, misinterpret, or mistranslate key components of Keynes's theoretical system – all charges that *P&W* (Pernecky and Wojick, 2019) explicitly levelled against supporters of WGE-style theorising.

Moreover, I agree that *P&W*'s (Pernecky and Wojick, 2019) account of how such neglect, misrepresentation, mistranslation, and misinterpretation is accomplished was insightful – they argued their case and supplied useful quotations and citations to illustrate it. All of this, I think, is germane to understanding what Heise (2021, p. 16) describes as the '...misconception of Keynes's ideas arising from Walrasian distortions – 'lost in translation'!. Finally, I should like to be clear that, contrary to the impression given by Heise (2021, p. 14) and possibly elsewhere, it was this that I found 'insightful' and not *P&W* supposed use of Kuhn's philosophy of science in the composition of their case. But overall, that is a long list of agreements. So where do we disagree?

A principal difference may already be apparent from what I have written above – I find no need to mention Thomas Kuhn's philosophy of science in discussing these issues. Indeed, I see no reason to think that Kuhn's philosophy of science is supportive to *P&W*'s case whatsoever. On the contrary, if one subscribes to it, then the critical comparison of theoretical systems may become rationally impossible because of the very philosophy of science one has adopted. Crudely reiterated, this is because Kuhn (1996) formulated a philosophy of science in which the theoretical systems of a science reside within a so-called 'paradigm': '...the entire constellation of beliefs, values, techniques, and so on shared by members of a given community' (Kuhn, 1996, p. 175) and he argued that alternative paradigms are 'incommensurable' (Kuhn, 1996, p.112): 'each group uses its own paradigm to argue in that paradigm's defence' (Kuhn, 1996, p.94). Thus, '...the proponents of competing paradigms practice their trades in different worlds' (Kuhn, 1996, p. 150)

with each community having its own conceptual apparatus which applies its vocabulary to nature in different ways (Kuhn, 1996, p. 149). Indeed, for Kuhn, where alternative paradigms exist contemporaneously within a field of study, then that field of study barely constitutes a science at all (Kuhn, 1996, pp. 4-5). So-called 'normal science' is characterised by a 'particular scientific community' being engaged in 'paradigm-based research' or 'puzzle solving' (Kuhn, 1996, §II-IV), for it cannot be characterised by the rational comparison of supposedly alternative paradigms because, by Kuhnian lights, such paradigms are incommensurable.

To be clear, Kuhnian incommensurability therefore means that '...the proponents of competing paradigms must fail to make complete contact with each other's viewpoints' (Kuhn, 1996, p. 148) and even that there are '...incompatible modes of community life' (Kuhn, 1996, p. 94). And it means that commensurability cannot be restored by comparing the absolute truth of each community's claims by means of a 'neutral language of observations': 'truth' like 'proof' is a term with only 'intra-theoretic applications' (Kuhn, 1996, p. 126; Kuhn, 1974A, p. 266).

Kuhn's philosophy also brings with it a particular theory of rationality; namely, it flows only from commitment to a paradigm, from being able to show how decisions and judgments are derived from, and justified by, the standards and values that it sets. It follows that a shift between paradigms, a so-called 'scientific revolution', cannot itself be rational. Kuhn speaks instead of paradigm shifts using terms like 'conversion' and compares them not to the making of a rational decision, but to a 'gestalt switch' (Kuhn, 1996, p. 150). And it is why he claims that their '...explanation must, in the final analysis, be psychological or sociological' (Kuhn, 1974B, p. 21).

These fundamental features of Kuhn's philosophy, and how they contrast with an alternative such as Karl Popper's critical rationalism in which rationality involves the critical exploration of what our theoretical systems imply, must be understood if his more startling and alarming declarations are to make any real sense, which is probably why such declarations tend to be wholly absent from *P&W's* (Pernecky and Wojick, 2019) paper or Heise's (2021) commentary. Ponder, for instance, this gem: '... it is precisely the abandonment of critical discourse that marks the transition to a science' (Kuhn, 1974B, p. 6).

Accordingly, these are the reasons why, following many others, I classified Kuhn's philosophy as a 'sociology of knowledge' or even a 'sociologising philosophy' characterised by 'epistemological relativism' (Thomas, 2020, p. 1417). As such, I argued that it is not a philosophy that one ought to adopt if one thinks that criticism is our only known method for the detection of error. Cue instead my discussion of the *fallibilist* philosophy of critical rationalism, the *non-justificational* use of deductive logic in argument and empirical scientific testing, and a vision of a community – given what I have written above let us not mistake it for 'the Academy' – that is genuinely interested in the critical discussion of its own theories, values, standards, assumptions, and beliefs (Thomas, 2020). This is all very different to the Kuhnian account of science as the making of commitments, or what Popper called 'the myth of the framework' (Popper, 1994). As Lakatos (1974, p. 93) observed: 'The clash between Popper and Kuhn is not about a mere technical point in epistemology. It concerns our central intellectual values'.

Thus, adopting Kuhn's philosophy supposedly to inform the critical comparison of two theoretical systems, when that very philosophy presents the two systems as being paradigm-bound, relativistic, and incommensurable, is hardly a viable basis for conducting such a comparison. It is at worst a contradiction: the desired critical comparison is positively disabled by the adoption of Kuhn's philosophy. Or at best, it is a bit like scoring an own goal in association football – it positively enables others to say: '*We don't understand you! You say your paradigm is incommensurable to ours only then to declare yours as superior!*'. And of course, given that *P&W* move effortlessly between the supposedly incommensurate systems of Keynes and WGE, showing where one supposed Kuhnian paradigm ignores, misinterprets, misrepresents, and mistranslates the other, then they are obviously not in actuality subscribing to Kuhn's philosophy. Under Kuhn's philosophy such an analysis would require some form of meta-paradigm that renders the two systems commensurable, but *P&W* did not introduce one. What they did instead, in the section in which they declared Keynes's system to be superior to that of WGE (Pernecky and Wojick, 2019, §3), was to resort implicitly to a pre-Kuhnian philosophy of science in which knowledge is inductively derived from a supposedly pure empirical base (i.e., the neutral language of observation that Kuhn thought non-existent). But obviously, that merely introduced a new inconsistency to the content of their argument; namely, it was no longer based on Kuhn's philosophy of science.

All of this is the subject of discussion in Thomas (2020), but Heise (2021) makes no effort to recount the details. This is unfortunate because it seems to me that he makes claims that are similarly contradictory. For instance, Heise (2021, p. 13; p.13 fn. 6) criticises two economists for failing '*...to replace their [Walrasian] lenses*' when viewing Keynes's theoretical system, while simultaneously attributing their failure to them '*...ignoring the problem of paradigm incommensurability*'. But *if* the problem of paradigm incommensurability is a genuine one, then Keynes is only understandable to those two economists in terms of the problems, exemplar achievements, language, and techniques of *their community's* so-called Kuhnian paradigm. Thus, what did Heise expect them to do? It seems to me that they acted in accordance with *his* very principle of paradigm incommensurability. I am bewildered why so many economists seem to think that paradigm incommensurability is a real problem, while simultaneously discussing it as if it were not. Let us be consistent and simply accept that it is not.

But equally, it is never wholly clear what Heise means by 'the problem of incommensurability' because he hardly attempts to discuss Kuhn's philosophy. The abstract and introduction to his paper suggest that he intends to work with the notion of '*...incommensurable paradigms in a Kuhnian understanding*' (Heise, 2021, p. 12, emphasis added), but unhelpfully that agenda is swiftly jettisoned; Heise writes: '*To be sure: I do not know what Kuhn meant by "incommensurability" and whether he took this term as synonymous with the term "incompatibility"*' (Heise, 2021, p. 14 fn. 8). Heise (2021, p. 14 fn. 8) proceeds to formulate his own peculiar locution in which paradigms are 'incommensurable' yet comparable under 'clearly defined and reasonable' standards. I do not find this to have much of a resemblance to a Kuhnian understanding of incommensurability.

Instead of examining Kuhn's philosophy, or my exchange with *P&W* concerning its merits in comparison to critical rationalism, Heise judges that the *Cambridge Journal of Economics (CJE)* is not '...the right place for a discussion of the philosophy and sociology of knowledge of Thomas Kuhn' and that we ought simply to dismiss Kuhn's philosophy as being '...vague and in need of interpretation' (Heise, 2021 p. 14). Such interpretations of Kuhn's philosophy, however, are not to be published in an economics journal which attempts to utilise it; Heise prefers that they be published in a philosophy journal. Heise supplements this judgement with another: that Kuhn's philosophy is 'moody' (Heise, 2021 p. 15). Its 'moody' nature supposedly being illustrated by Margaret Masterman's (1974) finding that Kuhn's *The Structure of Scientific Revolutions* (1996) contained 21 different definitions of what a 'paradigm' is.

However, what Masterman (1974) documented were the 21 different ways that Kuhn (1996) elucidated his concept of paradigm, noting afterwards that '...not all these senses of paradigm are inconsistent with one another: some may even be elucidations of others' (1974, p. 65). I am not myself surprised that Kuhn was able to supply 21 elucidations of his paradigm concept given the importance he attributed to it in his philosophy, so I am not inclined to dismiss him for that reason alone. Moreover, Masterman proceeded to reduce Kuhn's 21 elucidations of the paradigm concept to three main senses, one of which she labelled, much like I did, 'the sociological sort' (Masterman, 1974, p. 65).

Be that as it may, my comment upon *P&W*'s paper queried its supposed use of Kuhn's philosophy precisely because they elected to present their analysis as being based upon it and the *CJE* decided to publish that claim. This is in line with the *CJE*'s declared interest in publishing articles on methodological topics and inviting commentary. It seems to me that if philosophical doctrines are incorporated into methodological arguments in a way that is inconsistent with those very doctrines, then the advancement of learning will be poorly served if those arguments are allowed to pass without comment simply on the grounds that the doctrines are philosophical. Nor do I think such inconsistencies can be demonstrated without first explaining the doctrines. And it seems to me that it would be churlish not to detail an alternative doctrine that eliminates the inconsistency and advances the debate if one is readily to hand.

But more importantly to present purposes, I do not understand how Heise thinks his paper can possibly be 'an attempt to reconcile' (Heise, 2021, p.12; p.18) *P&W*'s claims concerning their use of Kuhn's philosophy, with my critique of those claims, given that he is not even willing to consider the content of those claims, or my critique of them.

To reiterate, I argued in my paper that *P&W* had no real need for Kuhn's philosophy in the compilation of their case against WGE and crucially that *they did not actually use it* in their purported demonstration of the superiority of Keynes's theoretical system to that of WGE; thus, there is nothing really for Heise to reconcile in the way that he claims.

What I think Heise *is* proposing is that Keynes's theoretical system and that of WGE be *compared* using Imre Lakatos's (1974) 'Methodology of Scientific Research Programmes' (MSRP). But he seems also to suggest that Lakatos's methodology of comparing research programmes be augmented with the idea that a research programme incorporates a 'world view' or 'pre-analytical vision', so that the '...Kuhnian concept of incommensurability... be brought to life: different paradigms are always (as a necessary and sufficient condition) incommensurable, as they are based upon different 'world views' or 'pre-analytical visions'' (Heise, 2021, p. 15). But

in the context of a discussion of Lakatos, I do not claim to understand what Heise means by this. My reading of Lakatos is that he rejected Kuhn's notion of incommensurability and based his MSRP on research programmes being *inconsistent* with one another, but *comparable* (see for instance, Lakatos, 1974, pp. 178-179). Such comparison to be conducted using his MSRP. Moreover, Lakatos (1974, p. 179) explicitly writes of his MSRP that its '...main aspects were developed from Popper's ideas'. Thus, I suspect Heise's discussion of incommensurability in the context of MSRP may simply be another instance of presenting the problem of incommensurability as if it were real, while proceeding to discuss it as if it were not.

Where Heise does follow Lakatos (and the strictures of Kuhn) is in drawing inspiration from the oft-repeated claim that Popper neglected the 'Duhem-Quine' problem: that the empirical testing of a theoretical system only ever relates to a system of theoretical statements and never one component part. A theory cannot therefore be falsified conclusively (the so-called 'naïve falsificationism' that both Kuhn (1974B) and Lakatos (1974) wrote about). Indeed, the report of an empirical test cannot be considered conclusive for the same reason. Consequently, according to Lakatos, we need instead a more complex and sophisticated methodology for discriminating between research programmes. That is, we need his MSRP. Cue Lakatos's complex of labels requesting the identification of a programme's 'hardcore' of stipulated assumptions, its 'negative heuristic' of loyalty to the 'hardcore', the 'protective belt' of auxiliary hypothesis that may be surrendered, modified, and remodified to deflect a purported falsification, and the 'positive heuristic' for progressing the explanatory reach of the programme etc. I am myself sceptical whether such conceptual proliferation can make the objective discrimination that Heise desires, or that it transcends the insight that at every turn what a valid deductive inference offers us is not a proof, but at best a choice between the truth of its conclusion and the falsity of one or more of its premises (Notturmo, 2000).

For instance, if the deductive logic of a theoretical system supposedly 'proves' that involuntary unemployment cannot exist, but we have empirical reports that it does, then are the assumptions of the deductive system true? We may say that the real conditions are not as the theoretical system demands, or that other special factors are exerting an influence, or that the reports of involuntary unemployment misdescribe the real situation or mismeasure it. Some may always prefer such explanations to entertaining the hypothesis that a treasured element of the theoretical system is false. As Popper joked, there is no logical formula for intellectual honesty. But this does not alter the feature of a valid deductive argument that matters; namely, it invites us to use critical reasoning in the investigation of these explanations, just as it enabled the general plan of Keynes's attack on the orthodoxy of his day (Leontief, 1949). And in investigating these conjectures, for that is what they are, we may be as critical as far as our ingenuity may carry us. Similarly, whatever we decide at each stage, our knowledge will remain conjectural even when it seems able to withstand the critical barrage – we may discover truth, but not with certainty. Crucially, however, the normative interest of the critical rationalist attitude remains the discovery of truth through the elimination of falsity and in the methodological rules that may assist in that, and not in psychological or sociological comforts.

Critical rationalist philosophers like W.W. Bartley III, David Miller and Mark Notturmo have made great efforts to restate and clarify these points; but equally, on my reading, the problem identified in the Duhem-Quine theory was considered by Popper (2002) in his *The Logic of Scientific Discovery* where he offered a methodological discussion of how an empirical science ought to respond to it. This was summarised by Klappholz and Agassi (1959, p. 60) as ‘...be critical and always ready to subject one’s hypotheses to critical scrutiny’. For Popper, there was not much more to be said than that, but that suggested attitude is very different to one that develops and dogmatically defends research programmes for a panoply of extra-scientific reasons. Sadly, it was not only Keynes who fell victim to the Academy’s tendency to ignore, misinterpret and misrepresent the best of its critical thinkers. On Lakatos and Popper, see for example Agassi (2021). But all that is another story.

Literature

Agassi, J. (2021) ‘Science sans subjectivity: the sad case of Imre Lakatos’, *Philosophy of the Social Sciences*, vol. 51 (5), 507-511.

Bartley, III, W.W. (1990) *Unfathomed Knowledge, Unmeasured Wealth: On Universities and the Wealth of Nations*, La Salle Illinois, Open Court.

Heise, Arne (2021) ‘The incommensurability of Keynes’s and Walrasian economics and the unsuccessful escape from old ideas’ *Economic Thought*, 10.2, pp. 12-19.

Keynes, J.M. (2015A) [1926] ‘The End of Laissez-faire’, pp. 39-61 in R. Skidelski (ed.) 2015. *The Essential Keynes*, United Kingdom, Penguin.

Keynes, J.M. (2015B) [1936] ‘The General Theory of Employment, Interest and Money’, pp. 176-262 in R. Skidelski (ed.) 2015. *The Essential Keynes*, United Kingdom, Penguin.

Klappholz, K. and Agassi, J. (1959) ‘Methodological prescriptions in economics’, *Economica*, vol. 26, 60-74.

Kuhn, T. S. (1974A) [1970] ‘Reflections on my critics’, 231-278 in I. Lakatos, and A. Musgrave, (eds.) 1974. *Criticism and the Growth of Knowledge*, Cambridge, Cambridge University Press.

Kuhn, T. S. (1974B) [1970] ‘Logic of Discovery or Psychology of Research?’, pp. 1-23 in I. Lakatos, and A. Musgrave (eds.) 1974. *Criticism and the Growth of Knowledge*, Cambridge, Cambridge University Press.

Kuhn, T. S. (1996) [1962] *The Structure of Scientific Revolutions*, Third edition, Chicago, University of Chicago Press.

Lakatos, I. (1974) [1970] ‘Falsification and the Methodology of Scientific Research Programmes’, pp. 91-196 in I. Lakatos, and A. Musgrave (eds.) 1974. *Criticism and the Growth of Knowledge*, Cambridge, Cambridge University Press.

Leontief, W. (1949) ‘Postulates: Keynes’ General Theory and the Classicists’, pp. 232-242 in Seymour E. Harris (ed) 1949. *The New Economics - Keynes’ Influence on Theory and Public Policy*, London, Dennis Dobson.

Masterman, M. (1974) [1970] ‘The Nature of a Paradigm’, pp. 59-89 in I. Lakatos, and A. Musgrave (eds.) 1974. *Criticism and the Growth of Knowledge*, Cambridge, Cambridge University Press.

Notturmo, M.A. (2000) *Science and the Open Society*, CEU Press, New York.

Pernecky, M. and Wojick, P. (2019) 'The problematic nature and consequences of the effort to force Keynes into the conceptual cul-de-sac of Walrasian economics', *Cambridge Journal of Economics*, vol. 43 (3), 769-783.

Pernecky, M. and Wojick, P. (2020) 'A response to "Keynes, Kuhn and the sociology of knowledge: a comment on Pernecky and Wojick"', *Cambridge Journal of Economics*, vol. 44(6), 1425-1428.

Popper, K.R. (1994) *The Myth of the Framework: In Defence of Science and Rationality*, (ed.), M.A. Notturmo, London, Routledge.

Popper, K. (2002) [1959] *The Logic of Scientific Discovery*, London, Routledge.

Thomas, R. (2020) 'Keynes, Kuhn and the sociology of knowledge: a comment on Pernecky and Wojick', *Cambridge Journal of Economics*, vol. 44(6), 1415-1424.

SUGGESTED CITATION:

Thomas, Rod (2021) 'Reply to Arne Heise's "The incommensurability of Keynes's and Walrasian economics and the unsuccessful escape from old ideas"' *Economic Thought*, 10.2, pp. 20-27.
<http://www.worldeconomicsassociation.org/files/journals/economicthought/WEA-ET-10-2-Thomas.pdf>