WORLD ECONOMICS ASSOCIATION

ECONOMIC THOUGHT

History, philosophy, and methodology

Volume 10, Issue 2, 2021

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ISSN 2049-3509

Published by the World Economics Association

Sister WEA open-access journals: *World Social and Economic Review* and *Real-World Economics Review*

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Economic Thought 10.2: 1-8, 2021

A methodological perspective on economic modelling and the global pandemic

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A question that recent research on the global pandemic raises is: how do the assumptions underlying epidemiological models and economic models differ? Epidemiological models we now know have become quite sophisticated (see Avery et al., 2020). Debate among economic methodologists regarding the nature of modeling has generated a considerable literature as well (Reiss, 2012; Hands, 2013). Yet these two literatures are largely non-communicating. Perhaps this is because economics has produced relatively little research on pandemics (though see Boianovsky and Erreygers, 2021). Yet it might still be asked, what might economic models be missing that epidemiological models capture? And might there be some sort of methodological bias in mainstream economics that plays a role in this? One way, then, one might begin to answer these questions is by identifying the main phenomenon in question, namely, in the case of the pandemic, a particular type of process, and ask what the nature of this type of process is. Then we may ask whether there is something about this type of phenomenon that places it out of bounds for current economic methodology. Thus, what sort of phenomenon is a pandemic?

The pandemic as a phenomenon: Contagion

A pandemic may be defined as "an epidemic occurring worldwide, or over a very wide area, crossing international boundaries and usually affecting a large number of people" (Last, 2001). What is thus characteristic of a pandemic is the extensive spread of a disease through its transmission across large numbers of people through their contact and interaction with one another. Epidemiological models explain this using the concept of *contagion* – a concept not widely used in mainstream economic modelling. Methodologists might accordingly ask themselves, what is it about mainstream economic models that explains this difference, and how would economic models need to be reformulated to explain a pandemic in the way epidemiologists and other disciplines do using the concept of contagion?

Contagion is a social behavioral concept in the following sense. A contagion is generally understood as a transmission of something – a disease, a taste, norms, habits, a practice, etc. – which acquires an increased social significance in virtue of how it travels across people through their contact and interaction. The dynamic it involves, I suggest, can be described as a two-direction, two-level feedback system: people's contact and interaction affect their shared circumstances, changes in which feed back upon and affect their individual circumstances, changes in which feed back upon and affect the nature of their contact and interaction, changes in which again affect their shared circumstances, and so on and so on until or unless something brings this all to a halt. The first level, the process level, involves people's contact and interaction; the second level, the shared circumstances or an aggregate outcome level involves the development of something – a pandemic in the case of a disease – over and above people's process level interaction that produces it.

The difference between a system modeled only in one-level, process-based terms and one modeled in two-direction, two-level feedback terms can be seen as the difference between a 'complicated' system and complex one (Rosser, 2021). To illustrate the latter, we can see how such a system has been modelled in connection with models of innovation adoption. Everett Rogers, trained in communications and sociology, influentially modelled innovation adoption as a process whereby for a given population a rise in the number of adopters of something (a new technology, a fashion, a norm, etc.) is followed by a declining number of remaining adopters until the innovation, which functions as the aggregate outcome, is standardized and ceases to be an 'innovation' (Rogers, 1962; 2003). Frank Bass, in the field of marketing, in one of the most highly cited management science papers, formalized and generalized Rogers' model in developing his normal distribution, Bass innovation diffusion curve (Bass, 1969; see Davis, 2019).

Applying this to the pandemic, whatever distribution pandemics exhibit, normal or otherwise, this diffusion dynamic can be used in herd immunity models to describe how a disease first becomes socially significant with a high incidence of infections when immunity in a community is low, then becomes less socially significant with a lower incidence of infections as immunity rises, and then ceases to be a pandemic and a socially significant public health problem when immunity becomes widespread. Moving, then, from the model to methodology, what can we take away from this?

Self-fulfilling prophecies

Robert Merton, the famous sociologist, described in an epistemologically interesting way how in cases such as those above a change in the status of a socially significant aggregate outcome produced by a process of interaction between agents can reverse what is believed to be true. His classic example is the bank run (Merton, 1948). A bank examiner mistakenly judges a bank to be insolvent; this causes depositors to withdraw their money from the bank; the bank then actually becomes insolvent. It was thus first false that the bank is insolvent, and then true that the bank is insolvent, on account of the interaction between the bank examiner and depositors.

The innovation diffusion and pandemic herd immunity cases are thus somewhat different. It is true there are innovations and pandemics, but that innovations are standardized and immunity may become widespread does not change this and make it false. In contrast, Merton's truth reversal case involves what is called a self-fulfilling prophecy, where a false statement becomes true by means of a social interaction process. (The opposite, a self-defeating prophecy is where a true statement becomes false through a social interaction process. A well-known case is the Y2K problem where 'computers will fail at the start of 2000' was true until people acted to prevent this and made it false.) In the language of performativity (Callon, 1998), then, the combination of the statement and social interaction 'performs' or produces its truth value.

Arguably, then, there is a similar self-fulfilling prophecy dynamic operating in some countries in the current global pandemic. Assume it is generally true that public health authorities can manage the disease in the sense of minimizing its spread (like it is true for Merton that originally the bank is actually solvent). Nonetheless, in some countries critics of these authorities (like the mistaken bank examiner) have argued, or prophesized, that those authorities will not be able to manage the disease. This has led some people in these countries to adopt risky health behaviors (like depositors withdrawing their money). As a result, in some countries public health authorities have indeed failed to manage the disease (like the bank actually becoming insolvent). What was true, that public health authorities could manage the disease, became false due to how criticism of public health authorities has affected social interaction.

Notice, then, that both Merton's financial example and this health one employ a particular kind of analysis of the social interaction process level, namely, one in which one agent (the bank examiner and public health authorities) has some special power to influence others' opinions. In effect, the social interaction process is asymmetric across agents with some agent(s) being in a position to interpret the situation at hand and thereby influence how others interpret it. (Another example might be central bank forward guidance.) Epistemically, the determination of truth resides with one agent who is in a position to 'perform' it for others.

This sort of analysis, however, is uncommon in economics. One reason is that economic agents are all assumed to have their own preferences and generally form their beliefs independently of one another. Consequently, they are not subject to others' influence, and contagion social interaction processes cannot generally occur. Contagion is thus not a widely used concept in economics. Interestingly, however, one recent economics research program, behavioral economics, has employed an analysis of this sort, and does so by departing from the assumption in standard economic modelling of agents' presumed independence.

Libertarian paternalism's self-fulfilling prophecy analysis

Behavioral economics' libertarian paternalism, then, effectively employs a self-fulfilling prophecy analysis in its explanation of the influence choice architects have on others' choices. Choice architects design and alter other agents' choice domains, and this sets off sequences of new – for example, healthy – choices and the possible emergence of 'new' tastes in a community. If what was originally true was that agents believed they preferred unhealthy choices, but their new healthy choices make them "better off, *as judged by themselves*" (Thaler and Sunstein, 2008, p. 5; their emphasis), then what was previously true becomes false due to the special influence of choice architects.

If this sort of analysis is novel for economics, it is not in business and marketing studies. Libertarian paternalism, of course, assumes choice architects are concerned with agents' well-being and act to promote beneficial social goals. However, choice architects can also act in their own interest and promote their own goals at the expense of others. The decoy effect, or asymmetric dominance effect, is where consumers have a preference between two options that a seller can alter by introducing a third option (Huber *et al*., 1982; see Angner, 2012, pp. 38ff). (The consumer prefers *A* to *B*; the seller prefers *B* to *A*; the seller introduces *C* which is less preferable for consumers to both *A* and *B*; when *C* is designed correctly, the consumer can be influenced to prefer *B* to *A*.)

Seller choice architects are only one instance of what is presumably the general case of powerful or influential agents being able to influence other agents' choices through how they act in social interaction with them – libertarian paternalism's other-regarding choice architects thus being a special case. What is it that keeps this sort of analysis from being regularly used in standard rational choice theory?

The departure from standard rational choice modelling this involves, then, lies in behavioral economics' characterization of choice as reference-dependent. Reference-dependence is inconsistent with the neoclassical rationality theory independence of irrelevant alternatives axiom (IIA) and expected utility's independence axiom (IA) that each rule out agents changing their choices when new options are introduced. In effect, though for whatever reason the context of choice is changed, people's choices will still not change. Thus, whereas on the standard view, choices is always reference-independent, or context-independent, for behavioral economics it is always reference- or context-dependent (Kahneman and Tversky, 1979). Context matters.

This tell us one reason why standard, rational choice economic modelling is not well-positioned to explain the pandemic. Two-level contagion models, such as the examples discussed above, are built around agents influencing one another. As libertarian paternalism and the decoy effect show, some agents can influence other agents if they succeed in altering the context of choice for the latter. Since this sort of influence is ruled out on the standard view of rational agents via the IIA and IA axioms, nothing can emerge in that analysis over and above agent interaction in the way of a socially significant aggregate outcome that might manifest itself in a truth or belief reversal. Context doesn't matter.

In effect, standard rationality models are 'flat' one-level models in which aggregate outcomes are essentially a benign reflection of the process level, as in microfoundations DSGE representative agent macroeconomic models and in ordinary microeconomic market analysis. What you see at the agent level is, scaled up, essentially what you get altogether, and any social level commentary or social significance interpretations fall outside the model – at least in core standard economics models. Thus, though methodologists might be interested in whether truth reversals occur in economic processes, the performativity idea, this issue is simply irrelevant to much standard thinking – or perhaps non-scientific according to its methods and modeling practices.

Some heterodox and non-standard modeling strategies

What exceptions to this thinking there exist in economics besides in behavioral economics tend to fall on economics' heterodox periphery or if explored in mainstream theory are treated as anomalous. An example of the former are post-Keynesian economics models that emphasize uncertainty, such as Minsky models that show how banks that supply credit to firms in good times can over-estimate firms' credit-worthiness, this can then influence firms to incur greater debt, and in a downturn firms then find themselves overextended. At the aggregate outcome level, the process level (asymmetric) interaction between banks (an authority like Merton's bank examiner) and firms can generate systemic financial instability – Minsky's financial instability hypothesis (Minsky, 1992).

One methodological implication of this, Dow (2021) comments, is that the mainstream commitment to mathematical modeling may itself prevent adequate explanation of complex systems because of the bias this involves toward producing deterministic representations of economic relationships. This in turn bars thinking about policies and institutional design that might address the possible effects of uncertainty in such systems. In effect, complex system feedback loops and uncertainty go hand-in-hand, but both are ruled out by the methodology of mainstream modeling. Further, since much heterodox economics is free of this bias, it contains significant opportunities for methodological advancement in economics.

Another example is how George Soros (2013) modeled boom-bust cycles in financial markets. Like J.M. Keynes's famous beauty contest (1936), traders form interdependent expectations about asset values that may drive up prices – the upswing phase of a cycle. Yet given the speculative and conventional nature of traders' bets, expectations are fragile and subject to abrupt reversals that drive down prices – the downswing of a cycle. Like Merton's analysis, what occurs at the aggregate level and at the individual interaction level reflexively act upon one another generating phase changes driven by belief reversals (Davis, 2020a).

It is interesting, then, to see how in rare cases mainstream theory approaches two-level types of analysis. One example, the information cascade literature, shows how fads and fashions result when information is limited and agents follow other agents' choices (e.g., Bikhchandani *et al*., 1992). Agents indeed interact, but since agents' preferences remain private, the extent to which there are second level aggregate outcome social effects like fads and fashions feeding back upon their interaction depends on the assumptions one makes about the nature of information. In contrast, contagion models transmit their effects at the deeper level of motivation.

Another example, from behavioral economics, investigates social preferences (e.g., Fehr and Schmidt, 1999). Agents who have other-regarding social preferences act differently from agents who do not, and this might be interpreted as a type of aggregate level social influence. But it is hard to see how this sort of analysis could ever mount to the level of a contagion dynamic because all agents are still utility maximizers ultimately independent of one another, and whether agents even have these types of social preferences is always seen to be an empirical question.

Methodologists' looking under the lamp-post problem

Let us try to take stock of all this from a methodological perspective. For many years now it has been argued that methodologists ought to concern themselves with describing and analyzing the phenomena that economics investigates rather than engage in normative theory appraisal in the spirit of Popper, Kuhn, and Lakatos (Backhouse *et al*., 1998; see Hands, 2019). A problem with this prescription is that it means methodologists need to work with phenomena largely as they are understood by economists. Thus, on the one hand, for a 'flat' one-level mathematical modelling practice in which aggregate outcomes are essentially a reflection of the process level, this not only tends to tie methodologists' analysis of the phenomena to process-level theorizations of the economy, but also to the particular understanding of process-level of interaction dominant in economics, namely, a market-centric theorizing that emphasizes arm's length, contact-less interaction between economic agents. On the other hand, phenomena that especially need to be conceptualized at the aggregate outcome level, such as characterized the 2008 financial crisis, characterize the current pandemic crisis, and confront us with climate change, are likely to get limited attention or fall beyond methodologists' scope of investigation.

These limitations do not apply to all the social sciences, epidemiologists, computational scientists, or to philosophers who study them. This arguably reflects economics' professional organization, including that it pushes heterodox and non-standard economics to its periphery. Whereas economics is organized in relatively hierarchical, core-periphery way with mainstream approaches strongly dominating research and training (see Heckman and Moktan, 2020), many other social sciences are organized in less hierarchical, more pluralistic or open way that supports greater research heterogeneity, puts weaker filters in place on recognizing new kinds of phenomena, and thus create a greater space for their philosophy of science examination. So, an important problem that methodologists face as philosophers of science is in their being primarily philosophers of economics. To the extent that economics is slow to incorporate new content, if dominant theories tend to be slow changing, methodologists are likely to be limited to explaining in a backward-looking way how economics manages old content.

The light under the lamp-post metaphor suggests that everything is dark beyond the reach of the light economics throws. But the metaphor breaks down if methodologists adopt a revised view of the phenomena. Rather than say the task they face is to describe and analyze, the task might be seen instead as prescribe and analyze. What phenomena should economics explain? Financial crisis? Inequality? Pandemics? Climate change? This begins with asking, where in economics are such things already being described and explained, where outside of economics are they being described and explained, and how might we look at the differences between them?

One of the reasons methodologists abandoned the normative theory appraisal approach is that its normativity was rejected. Perhaps one reason for this was that criticism of economics seemed to threaten a backlash with methodology being increasingly regarded as irrelevant in the economics profession. Accordingly, analyzing the phenomena as given by economics might make a contribution to economics, and this might improve methodology's standing. Yet curtailing judgment and limiting the scope of evaluation also threatens making methodology into a positivistic type of investigation, limited to offering conceptual nudges, and avoiding discussion of methodological value judgments. Is there, then, a way of maintaining a focus on the phenomena and also a critical scrutiny of economics that analysis entails?

Briefly, a way to do this is to take the phenomena as given, not solely by economics, but as phenomena that science at large takes as given. For methodologists, doing this can change the nature of the analysis it produces. Analyzing the phenomena as economics sees them puts the emphasis on the consistency of their explanation with economics' own goals. Analyzing the phenomena as science may see them emphasizes the compatibility of economics' explanation of the phenomena with how they may be explained elsewhere in science, such as in epidemiology.

This alternative view of the phenomena that methodology investigates increases its scope of investigation and holds a potential for aligning it more closely with other disciplines and indeed other methods developed elsewhere in science. The field of methodology since its postwar re-emergence in the 1980s has become increasingly self-sustaining, while also structurally more complex over time (Davis, 2020b). Surely it can now also accommodate a more interdisciplinary future path of development, and increasingly incorporate more complex forms of modeling and analysis into its research as have been developed in other disciplines.

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SUGGESTED CITATION:

Davis, John (2021) 'A methodological perspective on economic modelling and the global pandemic' *Economic Thought*, 10.2, pp. 1-8.
http://www.worldeconomicsassociation.org/files/journals/economicthought/WEA-ET-10-2-Davis.pdf

Economic Thought 10.2: 9-11, 2021

Comment on John Davis's 'A methodological perspective on economic modelling and the global pandemic'

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By exploring what economic methodology can learn from epidemiology John Davis provides a constructive foray into another discipline, accompanied by an insightful critical commentary on the current state of the field of economic methodology.

He notes that epidemiological models of contagion are more complex than most economics models by dint of starting from the nature of the subject matter. A pandemic is seen as a 'two-direction, two-level feedback system': between individual behaviour and interactions at the first level and the aggregate outcomes which set the circumstances which motivate subsequent behaviour and interactions at the second level. It is argued, using some examples, that the independent individualistic nature of the rational optimising agent in mainstream economic models precludes the possibility of a second level; motivation for choice continues to be individual optimisation rather than social, and interactions only influence the information base. There is no scope for contagion.

Davis argues further that the resulting limits on economic modelling are not addressed by the mainstream economic methodology literature. He notes the predominant (i.e. mainstream) approach to economic methodology as being the positivist study of actual practice such that judgments are not made, e.g. about the characterisation of individual behaviour. Rather economic methodologists could usefully consider a broader range of understandings of the subject matter, such as is found in other disciplines. This would provide the basis for critically examining the mainstream characterisation of individual behaviour and its motivation. Options would then open up to new methodologies, including importing new methods from other disciplines like epidemiology.

Davis explores other examples in economics which do account for feedback systems, such as Minsky's financial instability hypothesis. This account of the open-system subject matter of socio-economic systems means that knowledge in financial markets is generally uncertain. Interactions in expectations formation at a social, conventional level are fundamental and in turn influence outcomes which are the basis of further expectations formation. These outcomes depend not only on shifting conventional expectations but also on the evolution of institutions and practices, including the consequences of financial innovation. The transmission of expectations can thus be understood as a form of contagion.

So Post-Keynesian theory would seem to be a good potential comparator for epidemiological models. Davis's argument that the starting-point should be discussion of the nature of the subject matter accords well with Post-Keynesian philosophy. There would also be agreement that the independent-agent basis of mainstream models conflicts with the nature of feedback loops within real socio-economic systems (an analysis that draws on other disciplines' understanding of the subject matter). Further there is a substantial Post-Keynesian literature on economic methodology which, as Davis advocates, makes judgements about how theory can best capture the nature of socio-economic systems.

It is important for this discussion that the issue is framed by Davis in terms of modelling, leaving unanswered the question of the sufficiency of mathematical models to account for complex feedback systems. It would be interesting to have more discussion of this issue in relation to complexity economics since it is explicitly couched in terms of complex feedback systems. As far as Minsky is concerned, he quite deliberately avoided embodying his theory within a single formal model; rather he used models à la Keynes as aids to thought. This methodological position was grounded in the view that, given the nature of the subject matter, uncertainty was the norm. The processes he identified were systemic but not deterministic.

It could be argued then that epidemiological models would be too deterministic for economics. But the argument could be turned around: perhaps epidemiological models are too deterministic for pandemics. It was clear early on in the COVID pandemic that widely-cited epidemiological models were too limited. They needed to be considered alongside specialist knowledge based on behavioural research and on public health practice. These can contribute knowledge of behavioural responses to different types of policy and of institutional design and adaptation in light of public health requirements based on both experience and theory. All of these have the capacity to alter the nature of the feedback loops. Many epidemiological models now incorporate behavioural changes identified ex post through statistical analysis. But projecting behaviour forward in predictive modelling is still subject to considerable uncertainty. Key factors are the importance for outcomes of the degree of trust in the policy-making authorities, the clarity of communication and the effectiveness of policy delivery. In general there is considerable variability in the degree to which causal factors are even approximately deterministic, and therefore amenable to inductive extrapolation. Inputs from other types of expertise are still required. There is scope for non-mainstream economic methodology to inform discussion of epidemiological models.

Davis has shown that looking outside economics allows us to address methodological issues with a fresh eye. But it is not made altogether clear why economics should turn first to other disciplines as the source for new economic methodologies rather than to pre-existing bodies of economic theory and associated methodologies (like those of Minsky or of complexity theory). The noted marginalisation of non-mainstream theory within the hierarchical structure of economics could well be of rhetorical importance. When faced with an argument for ontological awareness, there could be resistance to references to particular ontologies which are uncongenial. Arguing that economics should be more like other disciplines in being open to different understandings of the subject matter and consequent methodology may well have more rhetorical force within mainstream economics than arguments in favour of non-mainstream economics. Would this be the case particularly for disciplines in the physical sciences rather than the social sciences?

This is a thought-provoking analysis from John Davis, raising questions, answering some and provoking others. I hope that he builds further on this paper in future research.

SUGGESTED CITATION:

Dow, Sheila (2021) 'Comment on John Davis's "A methodological perspective on economic modelling and the global pandemic"' *Economic Thought*, 10.2, pp. 9-11.
http://www.worldeconomicsassociation.org/files/journals/economicthought/WEA-ET-10-2-Dow.pdf

Economic Thought 10.2: 12-19, 2021

The Incommensurability of Keynes's and Walrasian Economics and the Unsuccessful Escape from Old Ideas*

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Abstract: The Cambridge Journal of Economics witnessed an important debate between Mark Pernecky and Paul Wojick on the one side and Rod Thomas on the other about the usefulness of Thomas Kuhn's sociology and philosophy of science in explaining why Keynes's revolutionary ideas exposed in the General Theory have been 'lost in translation'. This brief note is an attempt to reconcile Pernecky and Wojick's claim that Keynes's new economics of the General Theory and Walrasian General Equilibrium are incommensurable paradigms in a Kuhnian understanding and Thomas's critique that – if they were incommensurable – Pernecki and Wojick's appraisal of Keynes's paradigm as a better approximation to the 'real world' than Walsrasian General Equilibrium is inconsistent within that very Kuhnian framework.

Keywords: Keynes, Kuhn, Paradigm, Incommensurability.
JEL classification: B 2, B 40, B 5

Introduction

Pernecky and Wojick (henceforth P&W) published a very "insightful analysis" (Thomas 2020, 1423) in the Cambridge Journal of Economics on the nature of Keynesian and Walrasian economics in order to better understand "why the key theoretical constructs found in the *General Theory* […] have […] been ignored or misrepresented: or they have been mistranslated when an effort has been made to 'absorb' them […]" (Pernecky and Wojick 2019, 770). According to P&W, this is not due to a conceptional vagueness on the part of Keynes, but due to the incommensurability of Keynes's new economics and theorising on Walrasian General Equilibrium[1]. The lack of awareness of such paradigmatic incommensurability and the inability of most economists who attempted to make sense of the *General Theory* to disentangle themselves from preconceived ideas meant that they read Keynes's theoretical contributions through the lens of Walrasian General Equilibrium. As a result, "(t)his does an obvious injustice to Keynes and an even more important injustice to the goal of producing an accurate and ultimately helpful

* I would like to thank Rod Thomas and Michel S. Zouboulakis for valuable comments. As always, the usual disclaimer applies.

[1] Although P&W do not clearly define their understanding of Walrasian General Equilbrium, I take Dynamic Stochastic General Equilibrium (DSGE) modelling in all its variations as the mainstream paradigm here dubbed as Walrasian General Equilibrium.

understanding of the 'economic society in which we actually live'" (Pernecky and Wojick 2019, 770).

By using the conceptions of 'incommensurability' and 'paradigm', P&W explicitly refer to Thomas Kuhn's theory of scientific revolutions[2]. For Kuhn, scientific revolutions occur when the reigning paradigm has fallen into 'crisis' due to internal (deductive) inconsistencies or external (inductive) falsification[3] and will eventually be abandoned for a competing paradigm if (and only if) such a competing paradigm exists and is unaffected by the internal or external factors that triggered the crisis. Of course, the Great Depression of the 1930s has been seen by many as the external factor falsifying Walrasian general equilibrium economics in general or the (neo-)classical, self-regulating economics of the Marshallian and Pigouvian mould in particular (which was the main target of Keynes's attack on the 'citadel')[4]. Keynes's new economics of the *General Theory* were taken as the new paradigm, eagerly accepted mainly by the younger generation of economists in the USA (see e.g. Stanfield 1974) – the rising hegemon of academic economics after WW2. P&W's point is that such a Kuhnian revolution never occurred[5] because the necessary paradigm shift failed to materialise. And this was the case because early interpreters of the *General Theory* and, later, most other economists failed to replace their lenses, instead viewing the *General Theory* through their accustomed prism of the Walrasian paradigm, ignoring the problem of paradigm incommensurability.[6]

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[2] It is (still) disputed whether Kuhn's concept of scientific revolutions can be applied to the social sciences as he himself was in doubt about „what parts of social science have yet acquired such paradigms at all" (Kuhn 1970: 15). Although Kuhn was apparently willing to reserve the economic discipline a special status among the social sciences in this respect (see Kuhn 1970: 161), I am not concerned here with what Kuhn's final verdict would have been but rather claim that the economic discipline has already reached a paradigmatic status – otherwise the intensifying discussion on (a lack of) paradigmatic pluralism in economics would be groundless.

[3] I am eagerly conceding that 'falsification' is not the term used by Kuhn in this context and that he rejected the idea that falsification is a sufficient criterium for paradigm shifts. However, Kuhn stresses the influence of 'empirical anomalies' – which are nothing else than violations of "empirical paradigm-induced expectations that govern normal science" (Kuhn 1970: 52f.), i.e. can be taken as falsifications.

[4] Some reader may argue that Walras and Marshall belong to different paradigms. This is not my understanding of a paradigm: although Walrasian and Marshallian approaches differ in methodological perspectives, yet they share the same analytical core – which is why I would rather rate them as variants of the same paradigm.

[5] At this point, we have to distinguish between the non-occurrence of a scientific revolution because the economic discipline is still in a pre-paradigmatic state and the non-occurrence because of the resilience of the incumbent paradigm. In fact, the economic discipline at the time of the publication of the *General Theory* was most certainly still in a pre-paradigmatic state, yet Keynes's seemingly tried to shift the path economics was about to take (and, probably, was already further down the road in the UK than in the US and continental Europe) in becoming a mono-paradigmatic science after WW2. It is in this sense (only), that Keynes's new economics could be seen as revolutionary in a Kuhnian sense and that this revolution finally failed.

[6] The disequilibrium economics of Robert Clower and the 'rationing approach' of Edmond Malinvaud are probably extreme examples of Walrasian interpretations of Keynes's theoretical constructs, completely ignoring his analysis but merely inferring what Keynes must have "[…] had in the back of his mind" (Clower 1965: 290).

Although Thomas (2020) found this analysis 'insightful' (see above), he criticises P&W for running into an internal inconsistency: "[...] if P&W are right in declaring Keynes's ideas to be superior, then they must be wrong in thinking that Keynes and WGE [Walrasian General Equilibrium, A.H.] 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" (Thomas 2020, 1423). The solution he proposes is to abandon the 'Kuhnian prison' as the backdrop for a criticism of Walrasian general equilibrium economics and to adopt "the philosophical attitude of critical rationalism" (Thomas 2020, 1415).

The incommensurability, incompatibility and incomparability of paradigms

I would like to begin my brief remarks with a disclaimer: I do not believe an economics journal to be the right place for a discussion of the philosophy and sociology of knowledge of Thomas Kuhn[7]. Although it must be acknowledged that Kuhn's conceptions of the 'paradigm' and 'incommensurability' are certainly vague and in need of interpretation, I will not engage in discussing what Kuhn meant or what Kuhn really meant. Therefore, I am not discussing whether Kuhn took 'incommensurability' and 'incomparability' as synonymous or, at least, supplementary[8], or whether he saw his philosophy of science as incompatible or even incommensurate (and, therefore, incomparable?) with critical rationalism. Rather, I take those parts of Kuhn's theory

[7] To stress the point: I am not disallowing methodological discussions based on Kuhnian philosophy of science in economics journals – economics journal shall publish whatever the editors appraise as appropriate. And, as far as I am concerned, methodological discussions related to economic theorising should always be welcomed in economics journals. Having said that, I honestly belief, that a broader Popperian critique of the Kuhnian philosophy (or sociology) of science is better placed in philosophy journals – which is why I concentrate my comments on those issues related to a better understanding of Walrasian economists' failure to come to terms with Keynes's new economics.

[8] To be sure: I do not claim to know what Kuhn meant by 'incommensurability' and whether he took this term as synonymous with the term 'incompatibility'. However, the arguments to be presented are in need of a clarification of these terms and their relations: Although some sources define incommensurability as something being immeasurable/incomparable, I take paradigms to be incommensurable because they are based on alternative ontologies. Incommensurability is, therefore, a necessary characteristic of different paradigms. However, that does not rule out the possibility that these paradigms can be compared. This is even the case when comparison is supposed to lead to evaluative judgements as long as the standards for such judgements are clearly defined and reasonable. Incompatibility is yet another feature which means that different parts (theories, methods, models) cannot consistently be joint. We could add more terms or concepts such as 'inconsistency' which Lakatos uses in describing rivalling SRPs and could discuss whether he rejects the notion of incommensurability with respect to SRPs – challenging my proposition that Lakatos SRPs can be taken as similar in conceptual meaning to Kuhn's paradigms. However, this would distract from the scope of this note. Suffice to say that Lakatos does not reject the concept of incommensurability with respect to SRPs but appears to claim that incommensurability precludes the rational choice between SRPs – a conclusion readily accepted in the context of my remarks but contested in the philosophical realm (see e.g. Miner 1998).

eclectically – of course, as I understand them or as I believe them to make sense[9] – which I rate as useful in understanding the development of the economic discipline.

The moodiness of Kuhn's concept of a paradigm is legendary: it is said that his *Structure of Scientific Revolutions* (SSR) contains as many as 21 different definitions of what a paradigm is (see Masterman 1970). This is why it might be advisable to borrow more definite content from the Lakatosian concept of Scientific Research Programmes (SRP), which is less catchy but similar in conceptual meaning: a paradigm or SRP is the set of theories and models which form the backbone of scientific inquiry. What is more important than the label is the content: paradigms or SRPs comprise three dimensions:

1. The *ontological or heuristic dimension* is concerned with the essence of the object of inquiry: its basic constituents. It represents the 'world view' underlying a paradigm or, as Schumpeter termed it, its 'pre-analytic vision'.
2. The *epistemological dimension* breaks down the pre-analytical vision situated in the ontological dimension into core and auxiliary assumptions or, in Lakatosian terms, determines the 'negative heuristic' which "specifies certain claims of the research programme as not revisable" (Brahmachari 2016, p. 5) and the 'positive heuristic' forming a protective belt around the core axioms. This can be tinkered with if, for instance, empirical evidence or the pursuit of a particular perspective indicate it would be politic to do so.
3. The *methodological dimension* can be seen as 'meta-methodical', as it specifies the procedures accepted by the epistemic community to discriminate between 'truth' and 'non-truth' or 'science' and 'non-science'. It is part of the professionalisation of a scientific discipline to agree on a common methodological foundation.

Given these considerations, the Kuhnian concept of incommensurability – just as moot as the 'paradigm' – may be brought to life: different paradigms are always (as a necessary and sufficient condition) incommensurable, as they are based on different 'world views' or 'pre-analytical visions'.[10] Any set of theories which share the same ontological basis may be incompatible in their epistemological and methodological dimensions – i.e. with respect to their specific assumptions in the protective belt in Lakatosian terms (e.g. the assumption of imperfect markets is obviously incompatible with the assumption of perfect markets) or with regard to their methodical perspective (i.e. taking a static approach versus a dynamic approach) – yet they are certainly

[9] Moreover, I add, as will be seen below, Lakatosian ideas and terminology wherever I believe them to be more concise, illustrative or meaningful – this may be taken as unforgivable by purists of the philosophy of science. I apologise by pointing out that I am not interested in exegesis but merely try to make sense of such ideas and terminologies.

[10] The most eminent example of a scientific revolution and arguably the analytical foundation of Kuhn's SSR (see Kuhn 1957) – the Copernican cosmological revolution – is based on such a shift in the 'world view' or 'pre-analytic vision' which makes the 'old' geo-centric Ptolemaic paradigm incommensurable with the 'new' helio-centric Copernican paradigm: cosmology is thus either geo- or helio-centric but evidently it cannot be both.

commensurable in forming a common paradigm based on a "[…] strong network of commitments — conceptual, theoretical, instrumental, and methodological" (Kuhn, 1970: 42).[11] On the other hand, different paradigms – as incommensurable as they necessarily are – may (and actually will) share a common methodological understanding as a quality-control device and, therefore, may well be compatible in this respect. Finally, I do not see any reason why different paradigms – as incommensurable as they necessarily are – cannot be compared (with respect to their core axioms, their postulates, their policy proposals, etc.) with each other as Thomas (2020) appears to suggest (see my footnote 8). In fact, if different paradigms coexist – a situation pluralists take to be the only healthy state of the economics profession – a comparison of paradigms is needed in order to make an informed choice between the use of any paradigm in the first place (see e.g. Heise 2020a).[12] Moreover, if comparison does not translate into a simple contrasting juxtaposition, modes and objectives of comparison must be conceived. Arguably, verisimilitude (i.e. the likelihood that conjectural knowledge is objective truth) is the most obvious candidate as objective of comparison (and choice). However, if verisimilitude cannot seriously be taken as a rational criterion of comparison and choice due to the methodological restrictions known as the 'Duhem–Quine critique'[13], other objectives might be more practical: for instance, the realisticness of assumptions or the complexity of models (Ockham's razor) in relating deductive outcome to empirical reality (for a more detailed discussion, see Heise 2020a).

Kuhn's SSR, Keynes's GT and Walrasian general equilibrium theorising

With respect to the controversy between P&W and Thomas, these elaborations have the following bearing: I wholeheartedly follow P&W's argument that Keynes's *General Theory* incorporates the outlines of an alternative economic paradigm which is incommensurate to theorising on Walrasian General Equilibrium. And, therefore, I endorse the view that most of Keynesianism as depicted in textbooks and accepted by mainstream journals is a misconception of Keynes's ideas arising from Walrasian distortions – 'lost in translation'! Moreover, I would personally subscribe to P&W's view that Keynes's new paradigm provides a better and more appropriate tool for understanding 'the real world' than Walrasian general equilibrium economics – and, if this is to mean that Keynes's paradigm is superior to WGE, I would also support that conclusion.

[11] New Classical Macroeconomics and the different variants of neo- and standard-Keynesianism combine to form the Walrasian 'Dynamic Stochastic General Equilibrium' model (DSGE), yet they are incompatible with respect to (protective belt) assumptions of market structures and information availability. In terms of P&W's contribution, sharing the same paradigms means, with respect to the different Keynesianisms, that they adapt and absorb Keynes's theoretical constructs into a WGE 'world view' or 'pre-analytic vision'.

[12] Of course, the choice can also be based on forms of compulsion (e.g. career perspectives) or simply ignorance (about rival paradigms).

[13] According to the 'Duhem–Quine critique', only single theoretical statements can be objectively falsified, not entire paradigms. However, even falsifying single components of paradigms may cast light on the capabilities of paradigms and their status as 'progressive' or 'degenerating' (in Lakatosian parlance). As I have tried to show, the inability of standard neoclassical labour economics to explain the (negligible) impact of minimum wages on employment certainly casts some doubt not only on neoclassical labour market theory but also the entire paradigm (see Heise 2020b).

But this is only my personal view based on my assessment of the core assumptions of what I believe to be Keynes's paradigm as compared to the core assumptions of WGE. Yet this is where Rod Thomas's critique comes in: if there is no objective inter-paradigmatic comparison on the basis of verisimilitude, the choice of a paradigm must be based on more subjective criteria, such as an assessment of assumptions or model structures. Although this cannot be helped – certainly not by rejecting Kuhn's entire approach and replacing it by an alternative (e.g. Popperian 'critical rationalism'), which may well run into exactly the same problem of not being able to objectively discriminate between competing theories[14] – it is simply to accept the pluralistic nature of the economic discipline and to advocate inter-paradigmatic comparison and methodological rigor as quality-control devices to shield the discipline against the accusation of pure relativism.

This, of course, is a crucial point: what are the core assumptions – the world view or pre-analytic vision – of Keynes's new economics in contrast to the core assumptions of WGE? The latter can be named rather easily: the axioms of rationality, (gross) substitution, neutrality of money and ergodicity seem to be unchallenged in order to found a paradigm ontologically describing an *inter-temporal exchange economy optimally allocating scarce resources* as its world view or pre-analytic vision. However, with respect to the new paradigm exposed in the *General Theory*, such core assumptions encapsulating a different world view or pre-analytic vision are less obvious: Keynes not only failed to inform the readers of the *General Theory* about his alternative ontological base, but he also sowed some doubt about the incommensurability of his new economics with WGE (or, rather, the Marshallian version of that paradigm) when he called his magnum opus 'general' instead of 'alternative' and at various occasions declared (neo-)classical economics to be the specific (full employment, full capital utilisation) version of his more general approach[15] – does that not imply the compatibility and, indeed, commensurability of Keynes's ideas and WGE?[16] This at least appears to have been the appraisal of most fellow economists starting the chicken-and-egg discussion about which approach is the more general and which is the more specific. And P&W happen not to inform their readers about the evidence on which they built their judgement of incommensurability. Or, to put it more precisely: what is the incommensurable world view or pre-analytic vision in Keynes's *General Theory* that sets it apart from the exchange paradigm of mainstream WGE?

Earlier versions of Book I of the *General Theory*, unfortunately omitted in later revisions for the 'principle of effective demand', indicate that Keynes rejected the ontological basis of the exchange paradigm (which he labelled 'barter', 'real exchange' or 'cooperative economy') for something he called the 'monetary economy' or 'entrepreneur economy' (see Keynes 1979a; Keynes 1979b). Although Keynes remained rather silent about what exactly – in terms of its axiomatic structures – characterises this new paradigm and although he was not sufficiently

[14] This assertion is based on Paul Feyerabend's (1975) work and may well be contested – however, I leave this discussion in the hands of the philosophers.

[15] "We are thus led to a more general theory, which includes the classical theory with which we are familiar, as a special case" (Keynes 1936: XXIII).

[16] And is not Keynes's neglect of market imperfections in the *General Theory* rooted in his desire and strategy to make his paradigm as compatible – and commensurable? – with the orthodoxy?

aware of the importance of at least sketching his ontological basis,[17] this void did not go unnoticed: it has been suggested that Keynes's world view or pre-analytic vision is that of *social reproduction under uncertainty based on nominal obligations* (and private property as its underlying feature; see e.g. Heise 2019), assuming as core axioms non-substitution, monetary non-neutrality and non-ergodicity (see Davidson 1984; Davidson 2005).

Conclusion

This brief note was an attempt to reconcile P&W's claim that Keynes's new economics of the *General Theory* and WGE are incommensurable paradigms in a Kuhnian understanding and Thomas's critique that – if they were incommensurable – P&W's appraisal of Keynes's paradigm as a better approximation to the 'real world' than WGE is inconsistent within that very Kuhnian framework[18]. Accepting paradigmatic pluralism as the only adequate state of the economic discipline, comparing economic paradigms which are necessarily incommensurable must become an acknowledged branch of scientific inquiry within the field of economics in order to prepare for the informed (but not necessarily an invariably determinate) choice between competing paradigms which every scientist has to make – and which P&W obviously made in favour of Keynes's new economic paradigm, yet without sufficiently disclosing their selection procedure to convince Rod Thomas.

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[17] Which is something of a mystery, for he accused mainstream theory of precisely such "a lack in clearness and generality in the premises" (Keynes 1936: XXI).

[18] Of course, if one denies or rejects my postulate of an analytic distinction between incommensurability and incomparability of paradigms, my attempt of reconciliation must be of no avail.

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SUGGESTED CITATION:

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.
http://www.worldeconomicsassociation.org/files/journals/economicthought/WEA-ET-10-2-Heise.pdf

Economic Thought 10.2: 20-27, 2021

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

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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 themself 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 (Notturno, 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 Notturno 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.

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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

Economic Thought 10.2: 28-46, 2021

From 'What New Political Economy Is' to 'Why Is Everything New Political Economy?'

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Abstract*:* In this paper, I aim to discuss New Political Economy as a label for the economic analysis of politics, in the English language. The term 'political economy' itself, although it has ceased to be the preferred term by which economists refer to their discipline, it is still being used by a variety of scholars, especially for interdisciplinary research with political science, international relations and other social sciences. Marxist-inspired social scientists also have a claim on the term 'political economy'. The term gained relevance again with economists, in the 1950s, thanks to various critiques of orthodox economics, especially to the theory of economic policy and to economic planning. They ignored issues of political economy, such as the self-interest of politicians. The public choice movement revived these issues by applying rational choice theory to politics and preferred the label "public choice" to designate its movement. Scholars and traditions not affiliated with the public choice movement prefer the label '(new) political economy' to refer to their own economic analysis of politics. The search for a proper label is still ongoing, but they show how they can differentiate their objectives and affiliations

Key words: political economy; new political economy; public choice; political economics; theory of economic policy; economics and politics
JEL classification: B22; B25; D7

'New Political Economy' (NPE) is, in its simplest definition, the economic study of politics. It is somewhat a branch of the 'new kiosk economics of everything' (Mäki, 2012), specialized on the study the polity with the rational choice framework. The economic study of politics has had many names: 'political economy[20]', 'new political economy[21]', 'political economics[22]', '(new) political

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[19] This paper is part of my Ph.D. thesis *Dreaming of Unity: Essays on the History of New Political Economy*. It has been published as a working paper from the Center in the History of Political Economy (nº 2018-16), Duke University. Contact: rga1605@gmail.com. I thank the reviewers for comments.

[20] (Hibbs, Fassbender, 1981); (Stigler, 1988); (Drazen, 2000); (Weingast, Wittman, 2006).

[21] (Whiteley, 1980); (Gamble, 1995); (Sayer, 1999); (De Mendonça, Araújo, 2003); (Screpanti, Zamagni, 2005); (Besley, 2007).

[22] (Heilbroner, 1970); (Hirschman, 1971); (Persson, Tabellini, 2000); (Alesina, Persson, Tabellini, 2006).

macroeconomics[23]', 'positive political economy[24]', 'neoclassical political economy[25]', 'modern political economy[26]' and 'public choice[27]'. Such a variety of examples means that, just as its semi-synonymic predecessor term 'political economy', the term 'New Political Economy' can mean different things to different writers[28]. All the definitions, however, seem to include some degree of interdisciplinarity.

This article has five sections. Section 1 studies how many scholars have used the term "political economy" in recent years, including rational choice and non-rational choice based social scientists. Section 2 studies the rise and fall of the theory of economic policy and economic planning, and how it was pivotal to the emergence of the new political economy. Section 3 exposes the history of the public choice movement, the first one to use the term 'new political economy' to refer to its approach, before deciding for the term 'public choice'. Section 4 exposes the different debates especially between the political economists affiliated with the public choice movement and those who are not on the dispute for the term. Section 5 concludes, emphasizing how labels might matter.

1. The many names of political economy

Defining (new) political economy as the economic study of politics has the problem of running into a truism: 'such a vague definition may have the virtue of being all-inclusive, it gives no real sense of what is being studied' (Drazen, 2000, p. 5). It should be remembered that, in spite of many different definitions, economists once referred to their discipline as 'political economy'. The preference for the term 'political economy' lasted until the marginal revolution, in the English-speaking literature[29] (Figure 1). Alfred Marshall's definition of 'Political Economy or Economics' as 'a study of mankind in the ordinary business of life' (Marshall, 1920 [1890]) would become one of the most known definitions. At this point, both terms were still interchangeable. The situation

[23] (Gärtner, 2000); (Snowdon, Vane, 2005)

[24] (Alt, Shepsle, 1990).

[25] (Bonilla, Gatica, 2005).

[26] (Jakee, 2021).

[27] (Mueller, 2003). Differentiating 'political economy' from 'public choice' demands its own discussion.

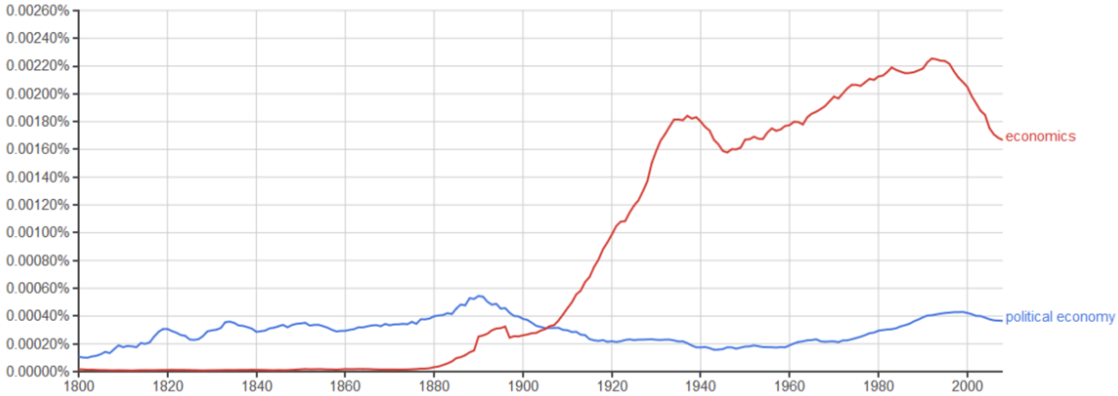
[28] Schumpeter offered the following *caveat* on defining 'political economy': '[…] political economy meant different things to different writers, and in *some cases* it meant what is now known as economic theory or "pure" economics.' (Schumpeter, 1954, p. 21, emphasis added). The caveat also definitely applies to NPE and its many synonyms; for example, the term 'political economics' means something different to Hirschman (1971) and to Persson and Tabellini (2000), but they were both related to the relationship between economics and politics. A dissertation in philosophy, for example, defines 'political economy' as a 'style of thought driven, at least in part, by an existential need to justify the world, to reconcile us with its imperfections, and to explain how to obtain good things in life' (England, 2016, p. 5).

[29] The trend should be different for other languages. As an anecdote, in Brazil the term "political economy" is related to courses in Marxist political economy (Almeida, Cavalieri, 2018); a friend of mine was surprised to see courses titled 'political economy' among the syllabi for the MIT and Yale graduate programs in economics, before realizing they meant something different than what he thought. Bonilla and Gatica (2005), writing in Spanish, use the term 'economía política neoclásica' ('neoclassical political economy') to refer to NPE, and, while writing in English (Bonilla et al, 2012), they just call it 'political economy', without the 'neoclassical' adjective. See Schefold (2014) for a German-speaking perspective.

started to change at the turn of the 20th century with the popularization of the dichotomy of economics as a science and economics as art, raised by William Nassau Senior and John Stuart Mill in the 1830s-1840s. This distinction is clear in John N. Keynes's work: while he wrote that 'Political economy or economics is a body of doctrine relating to economic phenomena' (Keynes, 1904, p. 2), he then separated the definition of economics as science, and political economy as art, related to economic policy (Keynes, 1904, p. 34-36).

This distinction would be used by Lionel Robbins to argue that economists should adopt the definition of economics as 'the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses' (Robbins, 1932, p. 15), while reserving the older name to applied issues, such as monopolies, protectionism, planning and policies (cf. Groenewegen, 2008). To Robbins (1961, p. 23), the ideas of political philosophers are *as important* to political economy as those of economists. This view was shared by Schumpeter (1954, p. 1141), who separates the term 'political economy' to refer to the 'practical questions' of the economy, crossing over into sociology. The term 'political economy' thus survived, even though restricted to specific contexts.

Figure 1: Comparing the uses of 'economics' and 'political economy', 1800-2000.



Source: NGram|Google/assembled by the author.

Internationally, the term continued to be used to designate Marxist and other similar approaches based on an objective value theory (Mohun, 1996; Groenewegen, 2008). Marx intended his analysis to be a *critique* of political economy, as it is written in the subtitle of *Das Kapital*. Then, as economics moved away of this term, Marxist analysis mostly inherited it. The term was later co-opted to refer to heterodox approaches, usually adding the qualifier 'radical' (Mata, 2005)[30]. For this reason, Gordon Tullock discarded the title 'Political Economy' to the journal that would become *Public Choice* (cf. Munger, Vanberg, 2016, p. 205), and the referees of the *Journal Economic Literature* discarded the term 'political economy' to label the code for 'Analysis of Collective Decision-Making' (Cherrier, 2017, p. 583).

'Political economy', then, was not a term associated with mainstream economists during the first half of the 20th century. This situation would be contested in the 1960s by different

[30] The term 'radical political economics' was intended to label the approach and to avoid conflation with Marxism, whose 'traditional' form was out of fashion, but a new perspective on economics, including 'Marxist analysis, institutional, left Keynesian and social economics' (Mata, 2005, p. 45).

movements, such as public choice and radical political economics, besides individual scholars such as Albert Hirschman[31]. They all agreed that, while classical political economy had the advantage of combining both economic and political analysis, it needed to be updated with the recent methods of social science. Therefore, economics would need a *new political economy*.

In probably the earliest use of the term 'new political economy' with the intention of singling out a new approach to economics (circa 1942), the Canadian theologian and philosopher Bernard Lonergan[32], argued that economics had lost the democratic spirit of the old political economy; economics, thus, could only achieve its objective of an efficient society through totalitarians means. 'The more economics endeavours to be an exact science,' wrote Lonergan (1988, p. 7), 'the more incapable it becomes to speak to men.' For that reason, Lonergan argued we need a 'new political economy', capable of fostering the democratic spirit with the improved tools developed by modern economics. Lonergan's NPE was a reaction to economic *and* moral issues of his time (Ormerod, Oslington, Koning, 2012, p. 392).

Economists showed little interest in Lonergan's ideas, but his example does show that some scholars were dissatisfied with how economics had ignored issues of political economy. Similar dissatisfaction was present in the writings of Marxist and Marxist-inspired scholars. Theodor Adorno (2000 [1968]), from the Frankfurt School of Critical Theory, claimed that losing the interdisciplinarity of classical political economy blinded social scientists from a holistic view of their subject. György Lukács (1968, p. 68), one of the most important Marxist literary critics of the 20th century, criticized economics for its 'petty specialization' and ignoring lessons from classical and Marxist political economy on politics.

Another relevant example of how the term 'new political economy' can mean different things to different people is the journal *New Political Economy*, a political science journal that attracts few submissions from mainstream economists due to its editorial line. Its aim is to combine 'the breadth of vision of the classical political economy of the 19th century with the analytical advances of twentieth-century social science' (Gamble et al, 1996, p. 5). One of the original editors of *New Political Economy* praised the new institutionalist approach for keeping alive the 'torch of political economy' in the neoclassical context (Payne, 2006, pp. 3-4). This admission emphasizes that 'their' new political economy is slightly different from 'ours'. Nowhere this is more visible than in Wikipedia (2021)[33], which, as of this writing, defines 'new political economy' as the study of ideologies in the economy, derived from the field of International Political

[31] The use of the term in English has controversies: for example, in Australia, 'political economy' is usually the term reserved to heterodox economics, and Australian heterodox economists consider it to be a better label to oppose mainstream economics, encompassing heterodox economics, economic history, history of economic thought and development economics (Stilwell, 2016; Thornton, 2017). The conflation of 'political economy' with 'heterodox economics' has been criticized as being harmful to both (Chester, Schroeder, 2015). This criticism has merit because conflating both terms has the disadvantage of being too parochial and removing the focus of political economy as an interdisciplinary approach.

[32] Lonergan was influenced by the Catholic Social Doctrine, a school of economic thought focused on ordering a market economy in conjunction with the Catholic thought, looking for a balance between liberty and assistance to the poor and the disenfranchised (see Leo XIII, 1891).

[33] I am citing Wikipedia because the online encyclopaedia is one of the main sources of *initial* knowledge, and thus has an important role in shaping the direction of knowledge, especially to laypeople (when people write "new political economy" in the search engine, chances are they go to Wikipedia, instead of academic sources). The information available on Wikipedia is thus not very useful to capture the definition of 'New Political Economy' that this article discusses.

Economy[34]. It has unclear direct relation to the definition explored in this article, that is the economic study of politics and the relationship between economics and the boundary disciplines of political economy.

The dissatisfaction was present among economists closer to the orthodoxy of its time as well. Public choice theorists were also concerned with the lack of an interdisciplinary view of economics and politics. Remembering that economics used to be called political economy, Ian McLean (1991, p. 777) wrote that 'if students of politics and economics would once again learn how to be political economists, both subjects would gain. They might even have more to contribute to the sum of useful knowledge'. For James Buchanan, one of the founding fathers of both the Virginia School of Political Economy and Constitutional Political Economy, public choice answered this demand; it brought a renewed interest in integrating economics and politics, continuing a long tradition that started with the classical political economy of Smith, Hume and the American Founding Fathers (Buchanan, 1988). Public choice thus, according to its supporters, recovered the holistic view of the world present in classical political economy 'that included politics, philosophy, law, and ethics' – in other words, 'a return to roots' (Yandle, 1990, p. 178).

We can see that, even if there is no unanimity on the definition of new political economy, relevant literature is being produced. The boundaries between economics and politics were relatively unexplored until the early 1960s, as evidenced by the prevalence of the Theory of Economic Policy (TEP) in economics, a purely economic and, arguably, technocratic view of policymaking. This approach would become a favourite target for the criticism of scholars who worked on this boundary.

2. The rise and fall of the theory of economic policy and economic planning

Before the Great Depression, the art of economic policy was a 'collection of examples' (cf. Acoccella, 2017). Only with the establishment of macroeconomics there was a definitive attempt to transform economic policy from art to theory. Macroeconomics always had a political vocation, i.e., that it could influence and select economic policies that would bring development to a country (cf. Acocella, Di Bartolomeo, Hughes Hallet, 2016). This is clear in *The General Theory of Employment, Interest and Money* (Keynes, 1996 [1936]), the foundational work of macroeconomics. Keynes argued the problem of economic depression consisted of insufficient effective demand. The government thus could (and needed to) step in to reverse the slump.

Although his influence is important, some scholars argued he is not the figure who should be associated with the post-war transformations in economic policy. To Patinkin (1972, p. 142), Keynes's work did not cause a revolution in economic policy, but only in economic theory. By then, the doctrine of the balanced budget was already being repelled due to the severity of the depression, citing Henry Simons and Arthur Pigou as examples. Interventionism was becoming the norm. However, Keynes is still associated with this change because, according to Adelman and Mack (2018, p. 78), his service at Cambridge and in the British government transformed him

[34] International Political Economy is a well-established field focused on bringing together not only political science and economics, but also international relations into an integrated social science (for an intellectual history, see Cohen, 2008).

from an outlier into the model of a 'professional, expert civil service'[35]. Furthermore, his work allowed macroeconomics to 'grow beyond studies of business cycles and money into a full-scale technical management of prices and output' at the cost of creating 'an increasing distance of economic thought from other intellectual domains. Economic analysis, even as it became more engaged in policymaking, got hived off from sibling social sciences' (Adelman and Mack, 2018, p. 78).

Meanwhile, econometrics flourished (Morgan, 1990; Louçã, 2007), and Oskar Lange (1936) published the first part of his article on economic planning in the same year Keynes published the *General Theory*. Lange effectively tipped the socialist calculation debate in favour of the socialists at that moment, showing there was no difference between a planned and a market economy in the general equilibrium model[36] – a planned economy should be, therefore, preferable for being easier to manipulate in pursuit of the intended macroeconomic results.

The environment was ripe for the development of a theory of economic policy, along with planning techniques. After the Second World War was over, the European countries needed to reconstruct their economies, many times almost from scratch. From that point on, planning in both capitalist and socialist economies would become the standard way of doing economic policy (Klein, 1947). Both Tanzi (2011) and Acoccella et al (2016) recognize that Northern Europe provided the perfect climate for the emergence of a theory of economic policy (TEP)[37].

Jan Tinbergen is usually associated with the development of TEP, along with Arthur Pigou, Ragnar Frisch, Erik Lindhal, Gunnar Myrdal, James Meade and others. He wrote the first theoretical treatment of the subject. Tinbergen proposed the fundamental concepts that 'the choice of instruments cannot be separated from the targets and hence from the form of the indicator' (Tinbergen, 1952, p. 4), and that the number of target variables must be equal to the number of instruments, so that their sum with the number of irrelevant variables is equal to the number of structural relations (Tinbergen, 1952, p. 27).

Later, Tinbergen (1956) would consider his theory of economic policy important to the elaboration of development policies, with the intention of both creating an environment favourable to active intervention in the economy in order to facilitate development, and offering the indispensable quantitative techniques for 'scientific planning'. He was blunt on its interventionist character: 'economic policy consists of the *deliberate manipulation* of a number of means in order to attain certain aims' (1956, p. 6, emphasis added). To intervene, therefore, is to be scientific[38].

[35] This is famously reflected on his obituary of Alfred Marshall, on the qualities of a good economist, who must be a 'mathematician, historian, statesman, philosopher' (Keynes, 1924, p. 322).

[36] See Levy and Peart (2008) for a summary of the debate.

[37] Tanzi (2011) called it the "Nordic European theory of economic policy" for this reason. Acoccella (2017) argued that the emergence of TEP is due to the openness of Scandinavia and the Netherlands to theoretical innovations by Wicksell, Ohlin, Myrdal, among others, the influence from Keynes' *General Theory*, the geographical proximity with the Soviet Union that allowed them having access to its planning techniques, experts and policymakers interacted through the inter-Scandinavian Marstrand Meeting and the meetings of the Dutch Economic Association. Therefore, there is evidence there was a creative community that allowed this theory to develop (see Medema (2011) for another example of creative communities in public choice theory, and Mata (2005) for radical political economics).

[38] The reason why these economists placed such emphasis on the scientific aspect was because planning was invariably associated with the 'left' and, potentially, with socialism and communism. As Acoccella (2017, p. 668) mentioned, 'the weight and the left-wing orientation of the "intelligentsia", as well as of the political parties supporting the governments or of some strong opposition parties and institutions (such as trade unions), together with the widespread idea that public happiness should be served by a visible hand' created

It should be noted that the *theory* of economic policy was something different from the planning techniques that emerged at the time, though they tended to be connected. Since they were perceived as a form of technology (Scarano, 2015), propagation of planning techniques was encouraged as a way to develop Third World countries (e.g. United Nations, 1963). The Indian prime-minister Jawaharlal Nehru proclaimed that “planning for development in independent India was supposed to lift the population from misery and build a new democratic spirit (*in* Adelman, Mack, 2018, p, 80). Coats thus summarized the achievements of the era:

> […] during the so-called Keynesian hegemony, the economics profession enjoyed a phase of rare consensus and confidence […] fears of a post war slump faded in the 1940s and early 1950s, many economists displayed what now appears as a naïve and unwarranted faith in the efficacy of their professional ideas and equipment, and concomitant confidence in the efficacy of economic management in the modernized economics and economic planning in the underdeveloped countries. (Coats, 1994, p. 16)

Sir Eric Roll (1968, p. 57), commenting on the widespread use of these techniques, wrote that ‘it is however, by no means clear that the next twenty years will produce so radical a change in basic approach as did the last twenty in comparison with the preceding pre-war period’. Roll wrote in 1968. Economic theory would indeed undergo a radical change, though not in the direction he expected.

Even though contemporaries such as Durbin (1949, p. 41) could claim that ‘we are all planners now’, the acceptance of this paradigm was not unanimous. Buchanan regarded the mind-set of the academy in the 1950s as ‘dirigiste or anti-libertarian socialist’ (*in* McLean, 1991, p. 760). W. Arthur Lewis, future Nobel memorial prize winner, and John Jewkes, a president of the Mont Pèlerin Society, themselves writers of treatises on economic planning, wondered if the euphoria of planning would be transitory – whether it was just a fad (Jewkes, 1950, p. 3), or part of a cycle where the importance given to the powers of the state in economic theory oscillated (Lewis, 1952, p. 21; Yandle, 1990, pp. 170-172).

Planning came under heavy criticism due to lack of results, especially in Third World countries (e.g. Hirschman, 1967; Killick, 1976). Ideologies hostile to interventionism started to become popular again, in what would be termed the rise of neoliberalism (e.g. Mirowski, Plehwe, 2009; Burgin, 2012). The idea of policymakers working for the ‘public interest’ became more and more contested.

In the original policy models, the policymaker was merely someone who enacted the policies proposed by the economist. Lange’s 1936 general equilibrium model envisioned the economy as a huge factory, where government could make production expand or retract to emulate the efficiency of the market economy. Policymakers were only needed to ‘order’ the

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this stigma. Scholars aimed to establish a ‘neutral’ scientific planning, in spite of their early socialistic leanings and the association of planning with socialism. Tinbergen himself had concrete examples of this dilemma. Although influenced by socialism (Alberts, 1991), he experienced conflicts during his time in the Dutch Central Planning Office, between those who wanted a Soviet-style economic planning and those who wanted policy guidelines. He had to rephrase ‘emotionally charged words, such as “planning” and “National Welfare Plan”, redrafting them into “organized foresight” and “Central Economic Plan”. Tinbergen also put considerable effort into promoting “The Third Road”, showing the alternative between no planning at all and Soviet-style planning.’ (Jolink, 2009, p. 395).

economy toward the desired point, in the name of rational 'public interest' (Burczak, 2006, pp. 31-33)[39]. This approach became untenable, as more and more economists began to realize the problems of political economy inherent in this economic treatment of the government. The crucial point was that policy models were treating politicians and statesmen as entities different from their subjects. Writing during the heyday of TEP, Baumol (1952) argued that the problem of the State was the same as that of any rational actor: incomplete economic knowledge[40]. Therefore, self-interested agents could exploit this incompleteness.

Similarly, Anthony Downs (1957) argued that policymakers should be considered just like any other economic agent, interested in maximizing their *own* wellbeing. The analysis of the politician in Downs' model represented a break with the idea of 'public interest', becoming a meaningless concept in rational choice. Downs ended up being associated with the Public Choice Theory (PCT) movement and would influence an entire generation of scholars, along with James M. Buchanan, Duncan Black, Kenneth Arrow and others writing in the 1950s. In macroeconomics, Downs inspired William Nordhaus (1975) to elaborate a formal political business cycle model[41].

Outside the rational choice paradigm, one should remember that inadequate concern with how politicians actually behave, the absence of *realpolitik* in economics – in other words, the lack of a political economy – had long been a point of criticism from Marxist and Marx-inspired social scientists (e.g. Kalecki, 1943; Lukács, 1968; Adorno, 2000 [1968]). In the 1960s, radical economists, comprising primarily such left-wing scholars, congregated themselves into the Union of Radical Political Economics. Its history has been told by many authors (e.g. Mata, 2005) and will not be a focus in this article. It is important to recognize, that, in spite of their ideological distinctions, they were all similarly concerned with the ignorance of political economy exhibited by orthodox economists.

3. A brief summary of the history of public choice theory and the dispute for the term 'political economy'

Public choice theory emerged from the combined influence of different sources: the Italian public finance tradition, Wicksell's work on public policies, Knight's scepticism concerning the capacity of democracy to promote choices that increase welfare, and the idea of government failure (Amadae, 2003; Mueller, 2003; Backhaus, Wagner, 2005; Medema, 2009; Burgin, 2012). Mercuro and Medema (2006, p. 158-159) listed the following advancements that played an important role in the development of the field and, essentially, constitute the foundations of the rational choice analysis of collective decision-making:

[39] Lange considered the general equilibrium model to be 'an appropriate description of the market economy' (Burczak, 2006, p. 32) and therefore open to manipulation.

[40] In his thesis (advised by Lionel Robbins), he wrote the following passage, that would seem odd to a modern economist, in terms of emphasis placement: 'To bring out their point more sharply some of the arguments have been so stated that they may seem to involve the implication that in a democratic government economic legislation can or even must always be advantageous to *all* members of the community' (Baumol, 1952, p. 142, emphasis added).

[41] See Almeida (in press) for a discussion on the role of the political business cycle model in the new political economy.

- Earlier work before the twentieth century on the analysis of voting rules (Charles de Borda, Marquis de Condorcet, Charles Dodgson, a.k.a. Lewis Caroll), analysis of tax and expenditure policies (Knut Wicksell), public goods (Erik Lindahl), and the Italian public finance tradition;
- Duncan Black's writings in the late 1940s[42], culminating with his path-breaking book *The Theory of Committees and Elections* (1958), on how committees can reach decisions;
- Anthony Downs's *Economic Theory of Democracy* (1957), which recasts political parties as acting analogously to profit-maximizing firms;
- Mancur Olson's *The Logic of Collective Action* (1965), which sets forth various theories of interest group behaviour, describing the factors that enable one interest group to prevail over another;
- The Rochester School of Political Economy, especially William Riker's *The Theory of Political Coalitions* (1962), which suggested that groups act to ensure minimally winning coalitions;
- Gordon Tullock's *The Politics of Bureaucracy* (1965), Anthony Downs's *Inside Bureaucracy* (1967) and William Niskanen's *Bureaucracy and Representative Government* (1971), which looked at the bureaucrat as another economic agent;
- Kenneth Arrow's *Social Choice and Individual Values* (1951), which explored the impact of voting rules on social welfare; and
- Paul Samuelson's many contributions to the theory of public goods (1955), which set out the conditions for efficient provision of collectively consumed goods and indicated circumstances under which provision below the optimal could occur in the market.

Public choice thus had a polygenic source. It would be more accurate speaking in terms of 'public choice movement'. The term 'public choice' itself, however, is sometimes associated with the Virginia School of Political Economy (VSPE), in reference to a handful of academic institutions located in the State of Virginia, United States (the University of Virginia, the Virginia Polytechnic Institute and the George Mason University) that became a creative community (Medema, 2011; Boettke, Marciano, 2015). The VSPE is also responsible for building a network of scholars, with Buchanan and Tullock being the main nodes connecting a host of co-authors and graduate students (Farvaque, Gannon, 2018). It would be incorrect, however, to conflate 'public choice' with the VSPE.

The one thing that unites all these traditions is the emphasis on the formation of groups and their interests, adopting the postulate that agents, *including* policymakers, act in a self-interested, economic manner (Mueller, 2003; Butler, 2012; Jakee, 2021).

It is important to emphasize that public choice emerged as a critique of the altruistic politicians' hypothesis implicit in the theory of economic policy (Boettke, Marciano, 2015). In the words of Butler (2012, p. 25), 'we should not assume that people behave differently in the marketplace for goods and services from how they behave when influencing government

[42] Duncan Black arguably produced the earliest studies on an explicit economic theory of politics. Ronald Coase, who had been his department colleague, wrote that Black started to work on his economic theory of politics as early as 1935, and only did not publish earlier because of lack of interest from the profession (Coase, 1994).

decisions. They saw themselves as part of 'a rebellion against a profession that they believed was overemphasizing the limits of markets and the prospects for welfare-enhancing government intervention' (Medema, 2011, p. 242). Public choice research thus focused on 'government failures' (Keech, Munger, 2015), arguing that most of the so-called market failures were actually brought about by the government itself (Marciano, 2013).

This is by no means an exhaustive account of the history of public choice, but it will suffice to show its basic tenets and illustrate how wide the applications of public choice are, giving public choice a near ubiquitous character in the context of new political economy. Precisely due to its wideness, 'public choice' can be a generic term, as admitted by Wagner (2016) himself. The reasons for such generality might have to do with the way the movement was initially organized. The Public Choice Society was originally called 'Committee for Non-Market Decision Making' and became 'the hub for scholars of disparate academic fields who met yearly to discuss academic papers. […] the fields represented in the society included economics, political science, public policy, sociology, mathematics, and philosophy' (Amadae, 2003, pp. 145-146). This evinces public choice both as a movement and as a 'place' for economists who were outside the main research topics in economics at the time. It remained an 'internal' critique of economics, since it did not abandon the rational economic agent model; on the contrary, rational choice became the basis of the critique.

PCT worked at the boundaries between economics and political science, helping to establish the rational choice approach in political science, which became one of the most important approaches in the discipline (Adcock, Bevir, 2010)[43]. Ever since the 1950s, there have been calls for cooperation between political scientists and economists, calls that public choice theory had been answering. Being one of the founders of PCT, Duncan Black (1950) claimed that political science could reach the same level of formalism as economics, which meant both disciplines would *return* to being one. Eldersveld et al (1952, p. 1005) wrote that research in political behaviour could use 'new theories, concepts, and research techniques developed in other fields of social science', having economics in mind. Downs (1957, p. 294) stressed the importance of developing models to unify politics and economics, a constant concern in the PCT and new political economy literature (Olson, 1990; Ordeshook, 1990).

Though complying with the rational choice approach, public choice is not considered part of the economic orthodoxy. As Paldam (1993, p. 177) wrote, public choice is both a branch and a sect of economics: it is a branch because it uses the same tools of mainstream economics (e.g. rational choice theory), but it is also a sect since it developed outside the institutional mainstream – at the periphery of the mainstream, so to speak. Public choice did not attract the attention of economists closer to the orthodox core of research, since it invites non-economic concerns and its empirical results lack robustness. The same applies to new political economy in general.

Among all the disciplines that claim the title 'political economy', scholarship on PCT has produced the largest amount of historiographical content, in the sense of both histories of public choice and histories of histories of public choice (e.g. Amadae, 2003; Backhaus, Wagner, 2005; Medema, 2009; Boettke, Marciano, 2015; McLean, 2015; Jakee, 2021). Again, some of its founders consider it a continuation of classical liberal political economy (Buchanan, 1988), and the term 'new political economy' was accordingly considered amid discussions on how to label the discipline.

[43] There is a lot of discussion within political science on the role of rational choice, and its conflicts with other approaches. See, for instance, Hall and Taylor (1996).

In the 1960s, William Mitchell equated 'public choice' with 'new political economy' (Mitchell, 1968). He, along with James Coleman, would later suggest the adoption of the term 'public choice' to name both the journal *Public Choice* and the Public Choice Society, over alternatives such as 'social choice', 'new political economy' and 'economics of politics' (cf. Mitchell, 1988, p. 117). David Johnson's public choice textbook exhibits the subtitle 'An introduction to the new political economy', to emphasize how PCT aimed to use market theory to analyse political economy (Johnson, 1991). In a recent survey, Jakee (2021) considers that 'modern political economy' to be interchangeable with 'public choice' and 'rational choice', while recognizing forms of political economy not affiliated with the public choice movement in footnotes.

The idea of public choice *as* new political economy still appears in certain works. In their textbook on the history of economic thought, Ekelund and Hébert (2007) consider that any economic analysis of politics can be equated with 'public choice', while Coats (1994) and Yandle (1990) also equate 'public choice' and 'new political economy'. A similar reasoning is adopted by Dennis Mueller, author of one of the most important PCT textbooks, who portrays himself as an ecumenist who does not care about labels (Mueller, 2015, p. 387). He defines public choice as 'the economic study of nonmarket decision making, or *simply* the application of economics to political science' (Mueller, 2003, p. 1, emphasis added). This is indeed a very broad definition, encompassing different traditions, and the new political macroeconomics literature - not to mention, of course, the other traditions of rational choice political economy that are not necessarily affiliated with the public choice movement, such as Chicago (Stigler, 1988). In contrast, Alesina defined 'new political economy' as a research agenda that started with the 'application of game theory to macroeconomics', that, *unlike* public choice, is 'very connected with "mainstream economic theory"' (Usabiaga Ibáñez, 1999, p. 8).

Others see the term 'new political economy' as not only focused on the boundary between economics and political science. In an even more general definition, Screpanti and Zamagni (2005, p. 475) consider that the term refers to a family or confederation of disciplines that consolidated during the 1970s, 'from public choice to new institutional economics and from behavioural economics to the economics of property rights'. In common, all the research fields mentioned involve some boundary work with other disciplines, such as political science, psychology and law. In contrast, Albert Hirschman rejected the term 'new political economy'. He considered that these economists were simply applying economic tools to analyse politics, instead of working in a truly integrative way (Hirschman, 1971, p. 3).

4. Discussion and disputes

Even though 'political economy' may have ceased to be the favoured term used by economists to refer to their own discipline, it became nonetheless a valuable label, that is sought for 'marketing' purposes (Stilwell, 2016). Not only that, but there is a pedagogical reason for the dispute, because Mueller (2003), Screpanti and Zamagni (2005) and Ekelund and Hébert (2007) are textbooks, therefore they would be interested in generalizing for a first view of the subject. Jakee (2021) wrote for the *Pathways to Research* service offered by EBSCO, therefore it is also pedagogical. Thus, the 'first contact' might shape the student on how to approach the subject. There is a contest for the label '(new) political economy' between political economists from different approaches[44]. Writing a historical rapport of NPE is thus a rather difficult enterprise, considering the many interpretations, distinctions, internal conflicts, and external criticism the field elicits.

Concerning the relation between political economics and public choice, depending on the context, one can easily morph into the other. Besley (2006, p. 29) wrote that ‘in some circles the term “public choice” is used to refer to any analysis that links economics and politics,’ a definition shared by Ekelund and Hébert (2007). Padovano (2004) argued that the only difference between both is that political economics uses a general equilibrium framework, while public choice uses a partial equilibrium framework; thus separating both is a waste of time. Mueller (2003, p. 471), commenting on Drazen (2000), wrote that it ‘is an excellent introduction to and overview of the literature, although the book is somewhat mistitled, since it discusses virtually all topics from the public choice literature,’ even though Drazen detailed the difference between NPE and public choice many times in the book. Blankart and Koester (2006) criticized the authors associated with NPE for not recognizing the importance of the public choice literature, claiming public choice theorists were researching the issues dear to NPE long before them. In their reply to Blankart and Koester’s article, political economists considered that ‘public choice and political economics are more labels than competing paradigms’ (Alesina et al, 2006, p. 201) and they ask: ‘Do Blankart and Koester classify anybody who was writing on the interaction between economics and politics before the mid-1980s as a member of the public choice school?’ (*ibid.*, p. 203). The authors may have asked this question rhetorically, but given Mueller’s preceding citation and his claim that ‘if [political economy] is defined as [economic study of politics], then it is not only encompassed by public choice, it is indistinguishable from it.’ (Mueller, 2015, p. 387), the answer to their question seems to be a ‘yes!’

Other reasons for these separations are ideological. Mueller (2015, p. 386) lamented that researchers avoided the term ‘public choice’ due to political correctness, just because its founders were often associated with the libertarian ideology[45]. The editors of the *Journal of Economic Literature* refused to adopt the name ‘public choice’ fearing an association with Tullock and Buchanan’s ideology (Cherrier, 2017). Gamble (1995, p. 530), on the other hand, celebrated the ‘the liberation of public choice from a laissez-faire straitjacket’, so it can be truly useful. McLean (1991, p. 776) celebrated the fact that public choice was becoming less and less ideological than it was in its earlier years. In the introduction of their book on the uses of public choice on Law, Farber and Frickey (1991, p. 11) claim to ‘steer a middle course between romanticism and cynicism’ towards government, which cynicism they associate with Riker and Buchanan.

Accusations of economic imperialism are inevitable at this point. However, supporters of New Political Economy do not see themselves as imperialistic; they consider they are merely trying to ‘put back’ economics and politics together (Ordeshook, 1990) or propose a unified approach to social sciences (Olson, 199019). However, Riker (1995) had a more imperialistic view and claimed that rational choice to be the only scientific way to analyse the social sciences.

As for practical applications, NPE ‘occasionally engages in debates about grand issues such as the role of states versus markets and the differences between democracy and

[44] Within the literature, De Araújo and Mendonça (2003) is one of the few works that try to make a direct comparison between the Marxist and neoclassical views of political economy, and it is telling this is a paper written in Portuguese.

[45] Mueller (2015, p. 386) mentions Anthony Downs, Mancur Olson and Elinor Ostrom as examples of ‘liberals in the American sense’, associated with the centre left. See Almeida (2021) for discussions on why political economists not affiliated to the public choice movement would rather avoid them for ideological reasons.

autocracy...The aim is to generate new, policy-relevant insights, particularly in areas where economists may have a comparative advantage' (Besley, 2007, p. 585). When giving an active role to the government, in Besley's view, NPE acts as a counterpoint to the influence of the Lucas critique, by incorporating elements that lacked to this last one, such as public choice theory and new institutional economics. However, others see the rational choice view of politics as an expansion of the neoliberal project of diminishing the role of State and for an increase on the role of markets in society, which critics consider it to be a project of submission. Bresser-Pereira (2009, p. 18), in a critique to rational choice in politics itself, writes that the term 'public choice' is 'Orwellian' for treating the State in a reductionist and criminalized view.

Conclusion

The article showed how that NPE emerged from various critiques of the post-war theory of economic policy, including public choice theory. It evolved to be a general economic analysis of politics, encompassing a wide scope of issues.

The idiosyncratic title of this paper is a personal summary of how my Ph.D. research changed through time. When I first started, I realized that, since nobody had written a 'History of New Political Economy', I could focus my thesis on this direction. I needed a delimitating definition of the term, but I realized that, due to all issues presented in the paper, defining it is a hard task. Economics has been trying to become a 'science of everything' (Mäki, 2012), while applying the economic method to social phenomena, and this is clear in the study of polity.

Even in spite of these issues, it can be said that *labels matter*, or at least, they matter for some people, as argued by Stilwell (2016). Some authors reject the label 'public choice' because they are not affiliated with the public choice *movement*, even if they research the same topics with a similar method. Thus, this paper aimed to contribute to a better definition of the terms, but it also to expose labelling problems in economics itself.

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SUGGESTED CITATION:

Almeida, Rafael Galvão de (2021) 'From "What New Political Economy Is" to "Why Is Everything New Political Economy?"' *Economic Thought*, 10.2, pp. 28-46.
http://www.worldeconomicsassociation.org/files/journals/economicthought/WEA-ET-10-2-Almeida.pdf

Economic Thought 10.2: 47-53, 2021

Agents, Equations, and Economics

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Abstract

Critiques of Neoclassical Economics extend, unsurprisingly, to its mathematical structure. The discussion has largely focused on General Equilibrium Theory (GET), a formalism developed by Léon Walras over a century ago. Internally consistent, but highly unrealistic, GET lacks predictive power, and has been a historical failure. As an alternative, this article proposes a methodology largely developed by Gräbner et al. (2019), in which Agent-Based Models (ABMs) are linked with existing Equation-Based Models (EBMs) as a means of developing a more powerful formalism. The approach is illustrated by application to the Arrow-Debreu (AD) model of Neoclassical theory, and the Kuznets Curve of Developmental Economics. Broader implications for the social and natural sciences are briefly considered.

Key Words: economic methodology, Agent-based modeling, Equation-based modeling

JEL Classification Codes: C02 C63 E13

Introduction

Criticism of neoclassical economic theory has frequently extended to its mathematical structure. The focus of these critiques, now and for over a century, has been equilibrium theory: a formalism which specifies (among other properties) rational actors, full employment, and perfect competition (Turk, 2012). These idealized assumptions were defended by Milton Friedman (1966, orig. 1953) who argued that lack of realism in economic models was acceptable if the models had high predictive power. Apart from the question of realism in scientific models, to be evaluated briefly below, it was precisely in the realm of prediction that neoclassical economics was a historical failure. As Alberto Ruiz-Villaverde et al. (2019) have shown, the elegant mathematical edifice did not anticipate economic crises from its inception to the present day nor generate policies to effectively counteract them. In a somewhat earlier historical study, Nobel laureate Paul Krugman reached essentially the same verdict, diagnosing a long tradition of “mistaking beauty for truth” (Krugman, 2009). But while it is all well and good – indeed essential – to critique the formalism, it is of course a more difficult prospect to develop an alternative.

One possible solution is to use computational models – in particular, Agent-Based Models (ABMs) – to simulate and refine economic equations (Gräbner et al., 2019). (For a valuable discussion of a similar methodological situation in ecology, see DeAngelis and Yurek, 2015). The rationale is twofold: Agent-Based Models (ABMs), the focus of the present essay, are more methodologically compatible with Equation-Based Models (EBMs) than they may first appear. The Church-Turing Thesis (CTT) stipulates (essentially) that any real-world computation can be converted into a Turing machine computation (orig. Church, 1935). In addition, innovations in computational power – including, in particular, the merging of logic and memory functions, thereby largely surmounting the von Neumann "bottleneck" problem (Peper, 2017) – should facilitate the modelling of complex, multi-scale economic systems. David Colander speculates:

> maybe this future historian [of economics] will also point out that eventually, economics returned to its classical roots, but modernized them to take into account enormous advances in analytic and computational power that changed the way empirical data could be integrated with the mathematics of complex systems involving interacting strategic agents. (2011, p.20)

The essay begins with a defense of realism in economic model-building. We briefly contrast realist and non-realist philosophies of science, and suggest that in analyses of complex systems (e.g., economies, ecosystems), realist approaches provide greater information regarding the systems under study, and thus yield higher predictive power (Gräber et al., 2019; Maki, 2009). We then examine the properties of Agent-Based Models (ABMs) and Equation-Based Models (EBMs). We emphasize the empiricism of ABMs, which is consistent with a realist epistemology. Also, in accord with CTT, we underscore the mathematical compatibility of ABMs with EBMs. We then turn to applications in economics. Two pioneering studies are addressed in some detail: Albin and Foley (1992) utilized the complementarity of ABM and EBM modeling to elicit the properties of a decentralized Walrasian auction, and thereby refine a key component of Neoclassical Formalism (Arrow and Debreu, 1954); Gräbner et al. (2019) apply the dialog of models to an evaluation of the Kuznets Inverted U-curve Hypothesis that stipulates the relationship between economic growth and income inequality (Kuznets, 1955). Finally, we examine the broader implications for scientific investigation. Risking exaggeration, we suggest that the structured interplay of equation and simulation will have wide applicability, and may prove paradigmatically significant in the social and natural sciences.

Model-building: Realism, agents, equations

Nearly 70 years ago, Milton Friedman defended a non-realist philosophy of economic interpretation (Friedman, 1953). His *Essays in Positive Economics* and, in particular, the essay on "Positive Methodology" asserted an epistemology that was, and remains, quintessentially neoclassical. In a widely quoted passage, Friedman stated: "Truly important and significant hypotheses will be found to have 'assumptions' that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory the more unrealistic the assumptions" (1966, orig. 1953, p.14). History has not been kind to this viewpoint, or to its larger context of neoclassical theory. However, the predictive failure of the framework did not lead, contra Friedman, to a questioning of its assumptions. Rather, the view was defended by an appeal to internal consistency: logico-mathematical agreement within its axiomatized structure. Gérard Debreu (1986, p.1265) was unequivocal: "According to the schema, an axiomatized theory has a mathematical form that is completely separated from its economic content. If one removes the economic interpretation of the primitive concepts, of the assumptions. . . its bare mathematical structure must still stand."

In marked contrast to this perspective, Leonardo Ivarola (2018) seeks to restore realism to economic theory. Ivarola critiques Friedman (1953) – and, implicitly, the "general impossibility of neoclassical economics" (Fine, 2011) – on both ontological and epistemological grounds. In the first of these, Friedman's notion of invariant relations among economic phenomena is replaced with an open-ended, decision-tree approach. Economic actors are, after all, humans; their economic decisions are shaped by their histories, their (emotionalized) personalities, and their sociocultural surroundings. Second, the flawed ontological assumption of invariance yielded a flawed epistemology that did not admit anomalies; as a result, and for nearly a century, the neoclassical framework has not predicted bubbles and crashes (Ruiz-Villaverde at al., 2019). Apart from theoretical critique, this history alone would suggest the need for a realistic, empirical economics.

Agent-based modeling (ABM) and equation-based modeling (EBM) can be creatively linked to address the conceptual flaws in neoclassical theory and, importantly, to develop a more realistic formalism. ABMs are simulations in which abstract entities (agents), governed by programmed rules, interact with one another in an artificial micro-world (Bruch and Atwell, 2015). Not infrequently, this process generates aggregate – emergent – properties unforeseen by the investigator. These, in turn, may contribute to the development of novel hypotheses. In contrast, and complementarily, EBMs typically model general systemic properties, and not individual features; EBMs are much less granular (Gräbner et al., 2019). More exactly, EBMs utilize ordinary differential equations (ODEs) to express the change in state of a many-component system over some specified period of time (Daun et al., 2008). That these highly contrastive approaches could be productively deployed together was recognized nearly 25 years ago. In population ecology, William Wilson (1998) demonstrated the complementarity of an ABM with an EBM in modeling the dynamics of predator-prey relations. The ABM simulated the aggregate effects of individual animal decisions, while the EBM expressed predator-prey population dynamics based on a traditional reaction-diffusion (RD) model of interacting chemical species (Kordo and Miura, 2010). Use of both approaches led to the mathematical refinement, and increased ecological realism, of small, dispersed populations, stochastic contact, and occasional extinctions.

Economic Applications: Addressing the Arrow-Debreu and Kuznets Models

In economics, as in population ecology, the interaction of an ABM with an EBM typically begins with the latter, which may be highly stylized (Gräbner et al., 2019). The equation is then converted into a preliminary ABM, which may be equally non-realistic. The prototype ABM is then tested to determine if it yields results equivalent to the EBM. What follows is a stepwise process – informed to no small extent by the creativity of the investigator – in which the ABM is enriched by empirical data, and the more "transparent" model may be expressed as a refined equation. The procedure is not without risk. As several advocates of the method have noted (e.g., Gräbner et al., 2019; Marilleau et al., 2018; Leombruni and Richardi, 2005), increasing an ABM's empirical richness through, for example, increased agent heterogeneity or greater number of mechanisms can cause the model to be unwieldy. However, as Gräbner et al. note, "starting with a simple, equation-based version and increasing the model's complexity stepwise helps to preserve its clarity. Also, the fundamental mechanisms of the model can usually still be communicated easily via precise equations" (Gräbner et al., 2019, p. 765).

An early application to economics addressed the Arrow-Debreu (AD) model, a core concept of neoclassical theory. The AD model, a formal proof of General Equilibrium Theory (GET), had posited an auction-like setting in which there exists some set of prices that would generate a balance of aggregate supply and demand (Arrow and Debreu, 1954). The proof has been controversial since its inception. Most notably, János Kornai, in an expansive critique, questioned the realism and scientific value of an "invisible hand" (Smith, 1776) – as in the AD model – that guided a capitalist economy toward equilibrium (Kornai, 1971; Schlefer, 2012). Contrasting with these critiques is the approach of Peter Albin and Duncan Foley, who view the AD proof as a starting point for devising a more powerful formalism (Albin and Foley, 1992). What would AD be if refined by realistic assumptions? Peter Albin and Duncan Foley (1992) developed an ABM in which the "auctioneer" was replaced by geographically dispersed agents in a system of decentralized exchange. Other key assumptions included costly advertising and bounded rationality. The changed assumptions resulted in unequal wealth endowments, with possible implications for mathematical modeling and economic policymaking.

In a more recent application, Gräbner et al. (2019) designed an ABM which simulated an enriched variation of the Kuznets Curve (Kuznets, 1955) and its underlying EBM, a formalism widely used in Development Economics. Simon Kuznets proposed that a rise in per capita income in a developing country was correlated with a sharp initial rise in economic inequality, which then plateaued, and ultimately declined, thus describing an inverted u-curve. (In an important variation – the Environmental Kuznets Curve (EKC) – directly relevant to sustainability, environmental deterioration is substituted for inequality. See Carson, 2010). The pattern was largely driven by rural-to-urban migration in an industrializing society. At the outset, financial opportunities were exploited only by a wealthy elite, but subsequently expanded to the larger society. Like its contemporary AD, the Kuznets model gave rise to an extensive critical literature (Lyubimov, 2017). The hypothesis, it has been widely noted, considered the West but not "the rest": it analyzed historical accounts of industrialization in Germany, Great Britain, and America, and then extrapolated – in an admittedly speculative spirit – to underdeveloped countries. Kuznets, to his credit, acknowledges this limitation, observing that the agrarian-to-industrial transformation may be markedly different in many developing countries, especially with regard to capital formation (Kuznets 1955, p. 26).

Is the Kuznets model valuable despite its flawed inception? The question motivated a recent study by Gräbner et al. (2019) utilizing a coupled ABM-EBM approach. Because Kuznets' original model (1955) did not contain equations, (although it included tabular data), Gräbner et al. developed a hypothetical EBM. The proposed mechanism underlying the inverted U curve was an initially wide wealth gap which impeded poor agents from transmitting resources (bequests) to their offspring. More exactly, Gräbner et al. specify an agent type $_{it}$ $\{p,r\}$ where p designates poor and r designates rich, and each agent possesses an asset $h_{it}>1$which can produce a unique consumption good y. A fraction of the revenue deriving from the latter may be saved for bequests, designated by e. If $e<1$, the offspring will receive nothing, and the good y will be consumed. The persistence of the latter situation, as for example in an autocratic, exploitive economy, increases the threat of revolution, thereby generating reform, and a redistributive system. (For a broader discussion of the political economy of the Kuznets Curve, including – importantly – alternative trajectories (e.g. "autocratic disaster") see Acemoglu and Robinson, 2002). The model was then incorporated into an ABM, which also included cultural regulations regarding marriage and inheritance, both of which are significant in the economics of developing societies. The resulting simulation found that "the time horizon of the Kuznets curve will vary with differences in initial distribution of wealth, differing degrees of social mobility, and alternative inheritance institutions" (Gräbner et al., 2019, p. 777).

Conclusion

Time has not been kind to Neoclassical Economics. From its inception to the present, the framework has failed to predict, or correct in a timely manner, economic dislocations in western capitalist societies. The consequent, and continuing, heterodox critique has frequently emphasized the formal axiomatic assumptions that lie at the heart of the paradigm. General Equilibrium Theory (GET) was, and has remained, a set of highly idealized constructs – e.g., rational agents, full employment, an optimized balance of supply and demand. This essay has suggested an alternative approach. We have proposed a computational strategy in which traditional economic equations can be coupled with agent-based models as a basis for developing a more scientifically powerful formalism. We then illustrated this viewpoint by sketching two pioneering studies. These applications showed how significant dynamic properties, which are concealed by an EBM, can be modelled by a realistic, empirically grounded ABM, and thereby furnish a basis for formulating a revised equation.

Importantly, the endeavor will involve significant challenges, some touching the basic relation of the scientist to reality. How, for example, does one choose the simulation approach to be coupled to a given equation? We have emphasized ABMs because of their descriptive richness, but other methods exist (e.g., cellular automata; mean-field game theory). Accordingly, a considerable effort is underway to devise a metalanguage which captures the optimally predictive relation between the scientist, the simulation, and the real world referent. A recent example along these lines is the framework developed by Gräbner (2018): from an infinite number of referent (target) properties, a selected set is instantiated by software agents, to which they are conceptually linked by a "key" (like the legend of a map). Such frameworks, as Vallverdu (2014) suggests, are simultaneously retrospective and anticipatory. Their notion of a cognitive and instrumental editing of reality has a distant antecedent in Kant, while the computational properties of interacting artificial agents are introducing a changed understanding of scientific experimentation. On the latter aspect, Schiaffonati (2016) has noted, a computational investigation will often generate unanticipated results. The epistemology is thereby shifting. It is a posteriori rather than a priori.

We conclude by asking if there are broader implications for the method outlined here. Does the "dialog of models" have significance not only for economic analyses, but for the social sciences generally, and indeed for any natural science that examines large, complex systems (e.g. population ecology and molecular cell biology)? Donald DeAngelis and Simeon Yurek (2015), influenced by the modeling of nonlinear dynamics in ecosystems, have recently considered this question. Defending the importance of computational models such as ABMs and cellular automata in the study of complexity, and presenting an argument largely consistent with the essay, they further note: "[S]cience may be moving into a period where equations do not play the central role in describing dynamic systems that they have played in the last 300 years" (p.3857). Perhaps they are right, but this may be a bridge too far. We would suggest that equations will probably never be less important – in economics, ecology, or any other science – than computational modeling. The approaches are interdependent, equally essential, and will share center stage. The dual strategy – a methodological pluralism – will hopefully promote a more realistic understanding of the dynamics of complex systems, as well as increased precision in prediction and application.

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SUGGESTED CITATION:

Wallace, Ron (2021) ‘Agents, Equations, and Economics’, *Economic Thought*, 10.2, pp. 47-53.
http://www.worldeconomicsassociation.org/files/journals/economicthought/WEA-ET-10-2-Wallace.pdf