

ECONOMIC THOUGHT

History, philosophy, and methodology

Volume 8, Issue 1, 2019

Table of Contents

Judging Heterodox Economics: A Response to Hodgson's Criticisms Lynne Chester	1
The Meaning and Future of Heterodox Economics: A Response to Lynne Chester Geoffrey M. Hodgson	22
Was Smith A Moral Subjectivist? Kevin Quinn	30
Addressing the Malaise in Neoclassical Economics: A Call for Partial Models Ron Wallace	40
Commentary on 'Addressing the Malaise in Neoclassical Economics: A Call for Partial Models' David Orrell	53

ISSN 2049-3509

Published by the World Economics Association

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

In line with the objectives of the World Economics Association, this journal seeks to support and advance interdisciplinary research that investigates the potential links between economics and other disciplines as well as contributions that challenge the divide between normative and positive approaches. Contributions from outside the mainstream debates in the history and philosophy of economics are also encouraged. In particular, the journal seeks to promote research that draws on a broad range of cultural and intellectual traditions.

Economic Thought accepts article submissions from scholars working in: the history of economic thought; economic history; methodology of economics; and philosophy of economics – with an emphasis on original and path-breaking research.

 Website
 http://et.worldeconomicsassociation.org

 Contact
 eteditor@worldeconomicsassociation.org

Editors

Eithne Murphy, National University of Ireland Galway Constantinos Repapis, UK, Goldsmiths, University of London Michel Zouboulakis, Greece, University of Thessaly

Managing editor

Kyla Rushman

Editorial board

Richard Arena, France, University of Nice-Sophia Antipolis Robert U. Ayres, France, INSEAD Daniel W. Bromley, USA, University of Wisconsin at Madison Bruce Caldwell, USA, Duke University Victoria Chick, UK, University of London David C. Colander, USA, Middlebury College John B. Davis, Netherlands, Universiteit van Amsterdam Jean-Pierre Dupuy, France, École Polytechnique and Stanford University Donald Gillies, UK, University of London Tony Lawson, UK, University of Cambridge Maria Cristina Marcuzzo, Italy, La Sapienza, Università di Roma Stephen Marglin, USA, Harvard University Manfred Max-Neef, Chile, Universidad Austral de Chile Deirdre McCloskey, USA, University of Illinois at Chicago Erik S Reinert, Norway, The Other Canon Alessandro Roncaglia, Italy, La Sapienza, Università di Roma Irene van Staveren, Netherlands, Erasmus University

Judging Heterodox Economics: A Response to Hodgson's Criticisms¹

Lynne Chester, Department of Political Economy, University of Sydney, Australia lynne.chester@sydney.edu.au

Abstract

The renowned institutionalist Geoffrey Hodgson has claimed *inter alia* that heterodox economics has failed to define its nature and scope, does not take pluralism seriously, and lacks expertise concentration to ensure quality which means it has made limited progress and is held in variable esteem. To address these alleged problems, Hodgson proposes four alternative strategies: the creation of heterodox economics academic departments; for heterodox economists to enter non-economics academic departments; for heterodox economists to 'organise' around a successful approach with future potential; or, to encourage the study of economic institutions by other social science disciplines or by using prominent mainstream techniques and approaches.

A response to these criticisms and proposed strategies is warranted for several reasons. These criticisms are not trivial and, as an assemblage the import is much greater than a singular criticism. Hodgson is very influential within the economics discipline and he reiterates, in part, past criticisms from the mainstream as well as presenting his criticisms to a wide range of audiences. These criticisms intersect with longstanding debates within heterodox economics about the role of pluralism, the definition and project of heterodox economics, its relationship to the changing form of mainstream, and the merit of synthesis or convergence of different heterodox schools of economic thought. The suitability of mainstream measures to judge heterodox economics, and the relationship of ideology and economic theory, are also raised by these criticisms as well as the feasibility of proposed strategies to support heterodox economics within the academy.

It is argued that several fallacious claims lead Hodgson to misconstrue the nature and evolution of heterodox economics, and inherent flaws in each of his proposed alternative strategies will further marginalise – not advance – the project of heterodox economics.

Keywords: heterodox economics, Geoffrey Hodgson, methodology, ontology, pluralism

JEL codes: B40, B50, B59

1. Introduction

The intellectual and historical origins of the many schools that comprise heterodox economics have been complex and diverse with notably different development paths during the second half of the 20th century in the United States (US), the United Kingdom (UK), Australia and throughout Europe, Asia and Latin America (Jo et al., 2018b, pp. 11-14.). Heterodox schools of economic thought have been progressively displaced from the mainstream (the orthodoxy)

¹ An earlier version of this paper was presented to the 30th annual conference of the European Association for Evolutionary Political Economy (EAEPE), 6-8 September 2018, Université Nice Sophia Antipolis, Nice, France.

of the economics discipline that has become dominated by one school of economic thought – neoclassical economics – and its monist methodology since the 1970s (Lee, 2009). Mainstream economists generally have limited or little engagement with, or even awareness of, alternative (heterodox) schools of economic thought or countenance alternative methodologies which reinforces the theoretical and methodological monist authority of the orthodoxy (Colander, 2010; Courvisanos, 2016).

In a draft essay the renowned institutionalist Geoffrey Hodgson (2017) conveys a negative view of the progress, success and prospects of heterodox economics. I view these comments by Hodgson as criticisms of heterodoxy and it is to these that this paper responds. Why, one may ask, is a response warranted to criticisms initiated in a draft essay?²

First, these criticisms are not trivial and, as an assemblage, the import of multiple criticisms is much greater than a singular one.

Second, Hodgson, a prolific author and editor, has a distinguished academic career of more than 40 years, and is held in high regard having been very instrumental to the contemporary development of two heterodox schools – institutional (in the original tradition) economics and evolutionary economics. In addition, he has been instrumental in the establishment and development of associations (and respective annual conferences) in which heterodox economists are very active participants such as the European Association for Evolutionary Political Economy (EAEPE) and the World Interdisciplinary Network for Institutional Research (WINIR). Thus, Hodgson exercises – through a well-established high public and social media profile – considerable influence within the economics discipline, across a range of discourses and amongst the heterodox economics community. His comments and opinions are widely read and listened to and hence, are influential.

Third, Hodgson reiterates, in part, past criticisms of heterodox economics such as a high level of theoretical diversity, and the 'poorer quality' of scholarship *vis-à-vis* the mainstream as indicated by the volume of publications in highly-ranked (mainstream) journals or research excellence rankings.³ These past criticisms, and proffered strategies to address, generated substantive responses – and continue to do so – yet Hodgson's argument is presented without reference to this ongoing discourse (see, for example: Boyer, 2017; Colander, 2010; Colander et al., 2004; 2007/08; 2010; Lee, 2011/12; 2012; Lee and Elsner, 2011; Lee and Lavoie, 2013; Rosser et al., 2013). Consequently, some of Hodgson's criticisms are not novel and are better understood if situated within their 'intellectual-historical' context.

Fourth, Hodgson's criticisms intersect with a longstanding debate – that has come, in part, to define heterodoxy and one with which scholars of heterodox economics are acutely aware – about the relevance and role of pluralism to the discipline of economics. This debate has generated a diversity of views and a considerable corpus although this is not acknowledged by Hodgson. In fact, Hodgson treats the issue of pluralism as some sort of 'quality control' measure needed by the heterodox economics community.

² These criticisms were first made by Hodgson in a draft essay entitled 'The pathology of heterodox economics and the limits to pluralism' presented (under the title 'Social sciences and the open society: The limits to pluralism) to the 4th annual World Interdisciplinary Network for Institutional Research (WINIR) Conference, 14-17 September 2017, Utrecht University, The Netherlands. The same paper was presented to a 13 November 2017 University of Cambridge Critical Realist Workshop, United Kingdom. Similar criticisms were subsequently presented by Hodgson, in his keynote lecture 'Heterodox economics as a scientific community? Problems, prospects and alternative strategies', to the 15th annual STOREP (Associazione Italiana per la Storia dell'Economia Politica) Conference, 28-30 June 2018, Università di Genoa, Italy.

³ Research excellence rankings refer to, for example, the UK's *Research Excellence Framework* (REF) for assessing the quality of research in higher education institutions and Australia's national research evaluation framework, *Excellence in Research for Australia* (ERA).

Fifth, Hodgson judges the 'success' of heterodox economics in terms of mainstream 'measures' (e.g. publication in journals ranked highly by the mainstream). This is paradoxical because these measures have contributed to the marginalisation of heterodox schools of economic thought from the teaching of economics and application by policymakers, and their practitioners from academic appointments and competitive funding grants. Hodgson's criticisms of heterodoxy therefore act to reinforce the hegemony of the mainstream paradigm.

Sixth, Hodgson's strategies are presented as solutions to alleged problems of 'no consensus about fundamentals', 'quality', and 'engagement with pluralism' which, in his view, need to be addressed if heterodox economics, as a scientific community, is to 'advance or extinguish' approaches to understanding. Thus, although not explicitly, Hodgson ascribes an epistemological role to heterodox economics. Many heterodox economists would articulate the project of heterodox economics as having a plurality of purpose: to perform an epistemological role by providing robust alternative methodologies for new understandings of complex, pervasive and persistent problems, *and* to cause a paradigmatic change within the economics discipline by replacing the prevailing mainstream paradigm, *and* to perform a pedagogical role by teaching how economics should be practiced using multiple methodological approaches, *and* to promote tolerance and application of methodological pluralism, *and* to provide a robust critique of, and alternatives to, mainstream economics.

Seventh, Hodgson's proposed strategies to address the 'impasse' he claims afflicts heterodox economics are not novel propositions, many having been debated for some time within heterodoxy, and by some within the mainstream (e.g., Colander, 2010). Moreover, the feasibility of these strategies has rapidly waned given the contemporary realities of higher education systems and degree structures, of the employment and research funding environment for academic heterodox economists, and the measures that the mainstream deploys to maintain its dominance.

Eighth, Hodgson summarily ascribes 'leftist political leanings' to all heterodox economists based on his view of different heterodox policy positions and without explanation of his conceptualisation of the term 'leftist'.⁴ Notwithstanding its contestability, the attribution is presented in such a negative way that it infers all heterodox economists are ideologically-driven and all mainstream economists are politically neutral.⁵ The ideological (political) position of different schools of economic thought – or what Dowd (2000) calls the 'incestuous' and 'hypnotic' dynamic interdependence between capitalism ideology and the prevailing orthodoxy – was recognised as far back as the late 1920s (Myrdal, 1965) and subsequently discussed by Schumpeter (1949), Samuels (1971), Foley (1975; 2003) and Boyer (2017), amongst others. As Colander wrote:

'The questions one asks, the frame that one uses in structuring an approach to an issue, and even the language one uses, all embody ideological content and thus every researcher is in some sense ideologically biased... to condemn any field for being ideological is useless; all fields of inquiry are guilty as charged' (Colander, 2004, pp. 2-3).

⁴ Although the 'left ideological position' of heterodoxy is mentioned on several occasions, Hodgson (2017, p. 17) does acknowledge that 'this hypothesis is not proven here: it would require an extensive opinion survey of the academics involved'.
⁵ Throughout the essay Hodgson assiduously avoids any self-identification as an economist, heterodox

⁵ Throughout the essay Hodgson assiduously avoids any self-identification as an economist, heterodox or otherwise, notwithstanding his unparalleled contributions to institutional and evolutionary economics and his seminal roles in EAEPE and WINIR.

Different methodologies will reflect different epistemologies which, in turn, reflect political philosophies and ontologies. This is a fundamental point which many heterodox scholars openly recognise, and upon which Hodgson is silent in his 2017 essay.

Ninth, the views expressed by Hodgson in this essay implicitly or explicitly engage with an array of issues, apart from pluralism, which have infused past and contemporary heterodox debates and led, on occasion, to specific publications by heterodox scholars (for example, see: Lawson, 2006; 2013; Lee and Elsner, 2010; Lee and Lavoie, 2011; Mearman 2011; 2012; Morgan, 2014; 2016). These issues include *inter alia*: how should heterodox economics be defined? What is the project (purpose) of heterodox economics? What is the relationship of heterodox economics to the mainstream? Should heterodox economics engage with the mainstream, and if so, how? Has the mainstream become more 'heterodox'? Is synthesis or convergence of different heterodox schools possible and/or warranted? What are the prospects for heterodox economics? Should heterodox economics be taught within or outside mainstream economics departments? Can journal rankings and citation metrics deal equitably with all contributions from the economics discipline?

Finally, these criticisms have been presented – without a rejoinder – to a wide range of audiences at two international conferences in the Netherlands and Italy, and a reputable regular event at the UK's University of Cambridge, during the nine-month period September 2017 to June 2018.⁶ Critique plays an important role in all disciplines. For heterodox economics, critique has been instrumental to stimulating debates, and articulation, about the evolving nature of heterodox economics. Hodgson's criticisms are a further intervention to those debates.

It is for these ten, and interrelated, reasons that, in my view, a response to Hodgson's criticisms is warranted. 7

The paper is structured as follows. Section Two outlines Hodgson's analytical approach to 'judge' the progress and success of heterodox economics, and discusses three substantive criticisms he makes: heterodoxy's failure to define its nature and scope; heterodoxy's failure to take pluralism seriously; and, heterodoxy's lack of expertise concentration has led to quality issues. These criticisms, it is argued, are based on spurious claims and promote mainstream economics as the 'benchmark' against which heterodox economics should strive to improve its 'quality'.

Section Three discusses the four (alternative) strategies put forward by Hodgson to address the alleged weaknesses of heterodox economics. It is shown that each strategy is flawed, two strategies are directed at some form of 'survival' for heterodox economics within the academy, one strategy creates an epistemological division and runs contrary to heterodoxy's pluralism, and the remaining strategy is similarly problematic in terms of the privileging of one analytical concern – institutions – above all others.

Section Four concludes that – contrary to his ideas from the 'philosophy-sociologysocial epistemology' of science – Hodgson applies an 'ill-fitting' approach to judge the progress and success of heterodoxy. Fallacious claims lead him to misconstrue the multilayered and multi-faceted evolutionary nature of heterodox economics. His argument to achieve 'quality' in heterodox economics is based on measures such as journal and research rankings constructed and deployed by mainstream economics which maintain the mainstream's hegemony and marginalise heterodoxy. In addition, there is not a common

⁶ See Footnote 2.

⁷ My focus in this paper is the substantive claims presented in Hodgson's 2017 essay and not the generalised statements in this essay which he attributes to heterodoxy without supporting evidence (e.g. the majority of heterodox economists are macroeconomists; self-identified heterodox economists widely use mathematical modelling and econometrics; leftist political leanings provide greater unity amongst Post-Keynesians, Sraffian and Marxist economists than anything else).

objective underpinning each of his inherently-flawed alternative strategies which reinforce dominance of the orthodoxy and further marginalise heterodoxy.

1. Hodgson's Analytical Approach and Substantive Criticisms

1.1 Analytical Approach and Assumptions

Drawing particularly on Kitcher's (1993) analysis of the role of scientific communities in the development of science, Hodgson is of the view that social epistemology – the collective acquisition of knowledge through a range of social practices (patterns of social interaction) by a scientific community – is an appropriate framework to assess the development, problems and prospects of heterodox economics: 'it highlights issues that may help explain its limited cumulative advance and its waning influence within departments of economics' (Hodgson, 2017, p. 14).⁸

Hodgson suggests that the following logic and assumptions can be applied to assess the progress and success, and thus future potential, of heterodox economics:⁹

- scientific enquiry is socially embedded;
- a community of interacting researchers is a prerequisite for 'effective enquiry';
- knowledge is created and advanced by 'institutionalised' communities of researchers through scrutiny of each other's work;
- a scientific community's social relations and institutions will 'screen' (scrutinise) research and thus depend on 'trust' from community members and 'are vital to establish a degree of consensus and authority upon which the progress of science depends' (Hodgson, 2017, p. 11);
- the social and material environment impacts on understanding and thus knowledge;
- the progress of any science (and thus in the growth of knowledge) requires social institutions (including incentives) to establish sufficient, but not absolute, consensus across a critical mass of scholars as well as expertise concentrations;
- consensus will necessitate control through screening by a 'group' using criteria such as the institutional affiliations of scholars or journal and research excellence rankings (and journal and research impact factors); and
- 'screening criteria' may be 'rough-and-ready' but are necessary for a scientific community to process complex and unwieldy amounts of knowledge and 'maintain the boundaries of a viable, expert, scientific community' (Hodgson, 2017, p. 14).

There is much in the above, albeit broad, outline of Hodgson's analytical approach that seems logical and reasonable *if* one accepts this is the role of scientific communities and how knowledge is created and advanced.¹⁰

⁸ An alternative approach to judge the success and prospects of a school of economic thought is proffered by Boyer (2017) who contends that the capacity of an 'intellectual current' to become the orthodoxy requires four conditions to be evident: theoretical coherence, empirical relevance, economic policy simplicity and 'ownership by a hegemonic bloc'.

⁹ Hodgson (2017, p. 2) describes his approach as 'apply[ing] some ideas from the philosophy, sociology and social epistemology of science to "heterodox economics" as a community'. The points listed as his logic and assumptions were discerned from Sections Seven, Eight and Nine of his 2017 essay.

¹⁰ This approach also assumes that knowledge is advanced in *all* sciences in the same way. Lee et al. (2010, p. 1410) suggest that it is possible the processes of advancing scientific knowledge differ because heterodox and mainstream economics are distinct bodies of knowledge that 'generate distinctly different referencing and citation practices'.

For present purposes, application of Hodgson's framework to determine the success, progress and prospects of heterodox economics would seem to require the identification of the following elements:

- the members of the community of interacting (heterodox) researchers;
- the social institutions creating and sustaining the interaction of researchers:
- the social and material environment of this community of researchers;
- the social institutions establishing consensus and expertise concentrations;
- the members of the group whose role is to screen to achieve consensus;
- the form of consensus reached; and
- the social practices (screening criteria) used to scrutinise each other's work.

A key issue is the identification of the *actual* real-world social practices, interactions and institutions – particularly in terms of achieving interaction, screening criteria and the change processes in the social and material environment – that Hodgson applies to reach his conclusions of heterodoxy's limited progress and variable esteem because of 'poor quality'.

As will be shown, Hodgson does not systematically and clearly identify these elements. In addition, documented social practices, interactions and institutions of heterodox economics are not considered (Lee, 2008; 2009).

1.2 Failure to Define Nature and Scope, and thus the Purpose of Heterodox Economics

Hodgson's (2017, p. 1) stated purpose 'is not to provide a definition of heterodox economics but to show that controversy over its nature is unresolved, without any clear consensus on its meaning'. However, he does propose that a reference point for the meaning of heterodox economics is the orthodoxy (and vice versa), and, invoking Lawson (2006), suggests that heterodoxy defines itself primarily as being in opposition to mainstream views. The notion of 'a simple mapping of theoretical positions onto policy outcomes' (Hodgson, 2017, p. 4) is rejected as a means to a meaningful definition.

Hodgson turns to the works of the late Fred Lee and Tony Lawson to marshal evidence in support of his claim that the progress of heterodox economics has been marred by a 'definitional schism in the heterodox camp' and 'unresolved controversy about its nature'.¹¹

In the second edition of *The New Palgrave Dictionary of Economics*, the abstract to Lee's entry on heterodox economics states *inter alia*:

'heterodox economics refers to a body of economic theories that holds an *alternative position* vis-à-vis mainstream economics; to a *community* of heterodox economists who identify themselves as such and embrace a *pluralistic attitude* towards heterodox theories without *rejecting contestability and incommensurability* among heterodox theories' (Lee, 2008, p. 5790, emphasis added).

Lee's (2009, pp. 6-7) subsequent articulation similarly refers to a 'concatenation of critiques' that are an alternative to the mainstream, a 'multi-level term' for a group of economic theories, a 'community' of scholars with a 'pluralistic attitude' who do not reject 'contestability and incommensurability' among the theories, and who are unified in their dismissal of the asocial,

¹¹ Lee and Lawson have led the debate over the last few decades about the nature and definition of heterodox economics.

ahistorical individualistic mainstream economics approach and 'deductivist, closed-system methodology'. ¹²

Contending that Lee's (2008; 2009) 'attempt to define "heterodox economics" bears the marks of personal and political preference', apart from some scientific criteria, Hodgson (2017, p. 7) argues that Lee uses 'demarcation criteria' to classify heterodoxy from orthodoxy if a school of economic thought does not use, for example, the mainstream's core propositions. He posits that if the mainstream adopts, for example, closed-systems of analysis then Lee would propose that heterodoxy uses the opposite – open-systems analysis. In other words, Hodgson is interpreting Lee's definition as deliberately constructed to be contrary to the mainstream.

Hodgson criticises Lee for not including as heterodox the schools of Austrian, institutional, evolutionary or Sraffian economics.¹³ This is not correct. These four schools of economic thought are classified by Lee as heterodox economics (see, Lee 2008, pp. 5790-5791; 2009, pp. 6-7).¹⁴ Critical realism, according to Hodgson (2017, p. 5), was classified by Lee as heterodox 'to allow for no dissent from "critical realism" to qualify as heterodox'. This is at odds with Lee's (2002; 2016) statements about the impact of critical realism upon the development of Post-Keynesian economics (discussed further below).

Three points are warranted about Lee's (2008; 2009) definition considering Hodgson's criticisms.

First, Lee's definition is not a list of schools *per se* but a combination of attributes or characteristics which are inherent to some schools of economic thought despite their different analytical foci or different methodologies. This definition points to the characteristics of belonging to a community, and being an alternative to the orthodoxy, which engages in pluralism and does not reject differences between theories nor does consider them incommensurable (Morgan and Embery, 2018).¹⁵ These are 'shared elements of difference' (Morgan and Embery, 2018, p. 529) with orthodoxy and are far more than a multiplicity from the mainstream.¹⁶

Second, the list of schools classified by Lee as heterodox is indicative of the theoretical and methodological breadth of the heterodox community – with the aforementioned characteristics – and does not have to be exhaustive for a definition, framed around characteristics, to be coherent or credible.

Third, Lee's definition points to the social practices of pluralism and community. The pluralism referred to is one which not only recognises or tolerates multiple analytical frameworks but seeks an active engagement with, and debate about, the different insights

Similarly, Mearman concluded that any definition of heterodox economics reflects:

¹² D'Ippoliti and Roncalglia (2015, p. 29, original emphasis) suggest that it is useful to refer to this definition by Lee is one of 'heterodox *economic analysis*'.

¹³ Hodgson also states that Lee's definition omitted the work of Amartya Sen. Lee's definition refers to schools of economic thought with specific characteristics not individuals.

¹⁴ Rosser et al. (2013) also incorrectly claim that Lee does not classify Austrian economics as heterodox.

¹⁵ Morgan and Emery (2018) expand on this point to posit that the definition of heterodox economics is not reducible to a critique of the mainstream but needs to take into account the 'activity' of heterodox economics which is 'real world relevant', has a 'focus on processes', 'engages with the history of economic thought', 'is a living body of knowledge', 'continues to develop', is 'open to theoretical change', and pursues 'critique' and 'methodological and philosophical issues'.

^{&#}x27;a number of dimensions. These are: theoretical concepts; methodology, taken to include ontology, epistemology and methods; location within social groups within economics; areas of interest; politics and the relation to agents with economic power and finally, mindset, *i.e.* how one perceives one's own approach to economics. HE [heterodox economics] is a fuzzy set in those dimensions. Any single economic idea, and/or the economist which holds it, will lie somewhere within that set, and somewhere on a scale on each of these dimensions' (Mearman, 2012, p. 421, emphasis added).

and explanations of social reality that arise from the application of different methodologies. Lee (2008, pp. 5791-5792; 2009, pp. 189-206) presents evidence of the networks, conferences, journals and organisations – social interactions and institutions – which have fostered the development of heterodox economics, as a community and through individual schools of economic thought. These are, however, not considered by Hodgson despite his analytical approach outlined above.

Hodgson is equally critical of Lawson's (2006) endeavours to define heterodox economics although, in his view, this is a more 'systematic' approach than Lee (2008; 2009). Nevertheless, Hodgson's discussion of these endeavours is 'selective' and, in so doing, the cogent philosophical reasoning of Lawson's (2006) argument is obscured.

Lawson (2006, p. 502) sets out to distinguish heterodox traditions 'collectively from the mainstream and individually from each other'.¹⁷

First, he establishes the common feature of heterodox traditions is that, other than 'projects of academic economics', all are in explicit, widespread and sustained 'opposition' to the mainstream orthodoxy.

Second, he establishes that the assumptions of rationality and individualism, along with its questionable abilities as an ideological defender, do not define the mainstream position, and its only enduring, largely unquestioned, essential core feature is its insistence on mathematical-deductivism. This methodology 'is considered so essential that worries about its usefulness, or dispensability... tend to be summarily dismissed rather than seriously addressed' (Lawson, 2006, p. 489). This methodological monism of mainstream economics, Lawson argues, has withstood all its 'theoretical fads and fashions' over the last 50 years or so.¹⁸

Third, Lawson establishes that heterodoxy's opposition to mainstream economics is ontological. Any methodology is underpinned by preconceived ideas about the nature of social reality. The mainstream's monist methodological view of social reality – denoted by ubiquitous closed systems in which event regularities occur and events have causal sequence like 'sets of isolated atoms' – does not accord to the real-world situations to which its mathematical models are applied and thus, is not fit-for-purpose. On the other hand, heterodox traditions – dominated by emphases such as uncertainty, evolutionary change, caring, interdependence – presuppose a different social ontology (view of social reality) of all phenomena being open, structured, dynamic due to transformation, and with a high internal social relationality.

Finally, Lawson establishes that heterodox traditions are distinguishable from each other by their concerns, emphases and questions asked, not by answers or methodologies.

Lawson's (2006) logic identifies heterodox schools of economic thought, like Lee (2008; 2009), as 'a coherent collective project' in opposition to the mainstream. Lawson's identification is ontological in nature whereas Lee deploys a set of characteristics. Hodgson, however, reduces Lawson's identification to a binary: either using or not using mathematics and *ipso facto* using open or closed systems of analysis;¹⁹ and, thus suggests that many

¹⁷ Lawson (2006) uses the term 'traditions' to refer to different schools of economic thought.

¹⁸ Colander et al. (2004), Coyle (2007), Davis (2005, 2006, 2008), Cedrini and Fontana (2017), for example, have argued that the mainstream has 'changed', 'diffused', 'specialised' and even 'heterodoxised'. However, as Dow (2011, p. 1163, emphasis added) points out, although there have been some mainstream methodological changes:

core deductivist principles remain as the exclusive methodological approach [because] the agenda is to *improve the deductivist system* rather than to replace it... the mainstream *absorbs ideas from elsewhere* that can be incorporated into its system of thought but *not* the alternative systems of thought themselves.

¹⁹ The view of reality presupposed by the mainstream's method of mathematical-deductivism is, according to Lawson, denoted by ubiquitous closed systems, ones in which event regularities occur and

mainstream icons could be classified as heterodox and many heterodox economists could be classified as orthodox.

Lawson has repeatedly refuted the notion that his definition suggests blanket opposition by heterodox economists to the use of mathematical formalism. He states:

'this does not amount to a rejection of all mathematical–deductive modelling. But it is a rejection of the insistence that we all always and everywhere use it. In other words, heterodox economics, in the first instance, is a rejection of a very specific form of methodological reductionism. It is a rejection of the view that formalistic methods are everywhere and always appropriate' (Lawson, 2006, p. 492).

And,

'[this is] not an accurate statement of my position... [and] will mostly encourage the uniformed reader to suppose that I am after all opposed to the use of mathematical formalism per se... I am *opposed to the abuse of mathematical formalism*, and such abuse, is I believe, typical of the situation in much of modern formalism' (Lawson, 2009, p. 190, emphasis added).

Hodgson (2017) does not refer to these refutations from Lawson. Nor does Hodgson refer to the debate about whether open-system analytical methodologies involve various combinations of openness and closure (see, for example: Bigo, 2006; Chick and Dow, 2005; Dow, 2004; Lawson, 2004).

Hodgson concludes that the different forms of heterodox identification by Lawson and Lee reveal a 'schism', 'clashes and contradictions' within heterodoxy as evidenced by two factors.

First, Hodgson suggests that many economists choose to self-identify as Marxist, Post-Keynesian or Sraffian rather than heterodox. This may be the case. 'However, one might equally note that many mainstream economists do not recognize or self-identity as mainstream... In neither case does the relevance of the term fully reduce to acceptance as interpellation or common usage' (Morgan and Embery, 2018, p. 518).

Second, Hodgson claims there is no consensus about the purpose of heterodox economics in terms of theory, methodology, analytical focus or policy prescriptions whereas consensus across a critical mass will be evident in any 'viable' discipline or school. On the contrary, Lee (2012) observes theoretical engagement, during the first half of the 20th century, between American institutionalists and Keynesian economics and Marxism, and subsequently between the latter and Post-Keynesians, and further integration and synthesis across heterodox schools during the latter part of the 20th century and into the new millennium. Lee (2002: 2016) is also of the view that the integration of critical realism and grounded theory with Post-Keynesian theories was beneficial for this school's development. O'Hara (2007, p. 3) details evidence of convergence between heterodox scholars vis-à-vis the principles of inquiry given the emphasis placed on 'realism, holism, circular and/or cumulative causation, institutions, and the role of values and social factors in economic life' by those applying an institutional-evolutionary political economy approach. In addition, Elsner (2017, p. 53) considers the potentiality for further convergence between the 'theoretical and methodological elements... for current social economics and evolutionary (Veblenian) institutional economics.'

events have casual sequence. These closed systems, in turn, presuppose formulations in terms of isolated atoms e.g. under the conditions of x, y will always follow.

Others view the definitions of Lee and Lawson as being complementary, contributing alternative ways of conceptualising heterodox economics, rather than standing in opposition to each other (see, for example: Jo et al., 2018b, pp. 3-26).

1.3 Need to Take Pluralism Seriously

According to Hodgson, pluralism is vital for theoretical innovation and advance although this:

'must be housed within some kind of consensus over what common problems are to be faced and what is within or beyond the scope of the group of researchers. Otherwise progress is impaired by endless dispute over fundamentals' (Hodgson, 2017, p. 10).

This is one of two views of pluralism implicitly expressed by Hodgson (2017). This first view suggests that pluralism, as a form of diversity, is necessary for knowledge to advance although Hodgson (2017, pp. 15-16) does add a caveat: 'unrestricted tolerance of diversity leads to a failure of quality control: anything goes... new ideas need to be developed in a climate of intense expert scrutiny, based on teams of well-informed specialists'. So, 'bounded diversity' is required, although Hodgson is silent on how these boundaries should be decided other than to suggest that 'restricted' diversity will mean rigorous criticism leading to arguments publishable in 'high-quality' (mainstream or heterodox) journals; that is, limit the extent of pluralism to that which can be aligned to the focus of highly-ranked journals and these are overwhelmingly mainstream economics journals.

Hodgson is also silent on the meaning he ascribes to pluralism other than some form of 'diversity'. We therefore infer that he is not distinguishing between epistemological, theoretical and methodological pluralism.²⁰

The second view of pluralism implicitly expressed by Hodgson (2017) involves 'engagement with the orthodoxy and other disciplines' by heterodox economists. Here Hodgson sees pluralism as more than a recognition of different analytical frameworks, and involving an active engagement and debate about the different insights and explanations that arise from the application of different theories or methodologies. However, according to Hodgson, that debate – within the economics discipline – should be based within the 'terms' of the orthodoxy. Although conceding that the mainstream has little 'incentive' for such engagement, Hodgson (2017, p. 16) maintains that 'dialogue with expert outsiders is needed to obtain critical feedback and to refute, sharpen or reinforce the dissenting position'. In other words, heterodox economists can only improve the 'quality' of their arguments through feedback from the orthodox 'experts'. This approach means the mainstream's monist methodology is 'privileged' as the arbiter of the merit of alternatives which further reinforces the hegemony of the mainstream.

The notion of pluralism, as expressed by Hodgson (2017) to be practiced by heterodox economists that involves engagement with other disciplines, infers that this does not already occur. This is incorrect given the evidence of engagement with, for example,

²⁰ The 'Roundtable dialogue on pluralism' published in the *International Journal of Pluralism and Economics Education* (Reardon, 2015) illustrates the many different conceptualisations of pluralism. This Roundtable also highlights several important questions to be considered when discussing and defining pluralism in economics. For example: Is it epistemological, theoretical or methodological pluralism? Should neoclassical economics be included? What is the relationship with other social science disciplines? Is pluralism consistent with heterodox economics? How is pluralism to be practiced?

sociology, political science, human geography and other social science disciplines.²¹ Most heterodox economists would refer to this engagement not as pluralism *per se* but as a commitment to interdisciplinarity which recognises the insights and understandings that other social science disciplines may add to that provided through the practice of economic pluralism.

Many have advocated the case for pluralism in economics over the last three decades (see, for example: Fullbrook, 2009; Garnett et al., 2009; Salanti and Screpanti, 1997). Hodgson was one of the authors of a 1992 'plea', published as a one-page announcement in the *American Economic Review*, expressing concern:

'with the threat to economic science posed by intellectual monopoly. Economists today enforce a monopoly of method or core assumptions, often defended on no better ground that it constitutes the "mainstream" ... we call for a new spirit of pluralism in economics ... an economics that requires itself to face all the arguments will be a more, not a less rigorous science' (Hodgson, Maki and McCloskey, 1992, p. xxxv).

Hodgson (2017) does not mention this longstanding debate nor is there any mention of his conceptualisation of pluralism or differences with others (see, for example: Caldwell, 1988).

1.4 Lack of Expertise Concentration so Quality Suffers

The third substantive criticism made by Hodgson follows from the first two criticisms.

The purported lack of consensus about the nature and purpose of heterodox economics, and the high level of theoretical diversity, means – according to Hodgson's argument – that there is no "expertise concentration(s)" to provide critical feedback to enhance quality.

The declining influence of heterodoxy within economics departments has also meant 'diminishing incentives' for early career academic economists to be 'recruited' to the heterodoxy community and the 'strategic response [of the broad heterodox community] has been limited' (Hodgson, 2017, p. 15) to the founding of a 'few generalist' journals. After noting the relative success of these journals as a counter to the mainstream, like the *Cambridge Journal of Economics*, Hodgson chides heterodox scholars for 'a frequent tendency to disregard established rankings or conventional citation impact data' (Hodgson, 2017, p. 15) for mainstream journals. This 'disregard' is not evidenced other than a reference to so-called unsuccessful efforts to develop heterodox rankings and suggests, again, that – in Hodgson's view – quality for heterodoxy will only be achieved by reference to the 'standards' and 'debate parameters' of the mainstream.²²

The corollary is that by continuing to eschew mainstream measures – such as journal and research (e.g. REF, ERA) rankings – the marginalisation of heterodox economics will continue until its 'quality' can be improved according to mainstream standards. Yet it is these very measures that have progressively displaced heterodox economists from the academy.

²¹ See, for the example, the following journals: *American Journal of Economics and Sociology, Ecological Economics, Economy and Society, Environment and Planning A: Economy and Space, Feminist Economics, Forum for Social Economics, Geoforum, International Journal for Pluralism and Economics Education, Journal of Australian Political Economy, Review of International Political Economy, Review of Social Economics, Science and Society and the Socio-Economic Review.*

²² Although not specifically stated, Hodgson is referring to Lee and Cronin's (2010) work to establish comparative research quality-equality rankings of 62 heterodox and 192 mainstream journals.

2. Hodgson's Alternative Strategies to Address the Alleged Weaknesses of Heterodox Economics

To address these alleged weaknesses, Hodgson proposes four alternative strategies: the creation of heterodox economics academic departments; for heterodox economists to enter non-economics academic departments; for heterodox economists to 'organise' around a successful approach with future potential; and, to encourage the study of economic institutions from other social science disciplines or by using prominent mainstream techniques and approaches.

For Hodgson, a strategy to redress the demise of heterodoxy's influence in the academy will only be 'viable' if it meets four criteria, namely: establishes the *raison d'être* of heterodox economics; encompasses 'in the field' researcher incentives; provides quality control; and, ensures consensus 'to avoid endless dispute over fundamentals, to help build cumulative knowledge and in turn to reinforce' (Hodgson, 2017, p. 17) researcher incentives and quality control. In other words, a successful strategy must overcome Hodgson's alleged 'purpose, quality and consensus' weaknesses of heterodoxy as discussed in Section Two.

Of the four alternative strategies put forward by Hodgson two resemble 'straw persons' because he proceeds to quickly dismiss each. He also does not systematically assess the four alternatives against his criteria for a viable strategy.

Each of Hodgson's proposed strategies are now discussed in turn.

2.1 Creation of Separate Academic Departments

The first alternative proposed is the creation of separate heterodox economics (political economy) departments. The Australian University of Sydney's Department of Political Economy is the sole example cited but the 'difficult' and unique history of its establishment, ongoing institutional struggle to survive a 'separate' existence and the dilution of heterodox knowledge and skills through replacement with scholars from other social science disciplines are not mentioned (see, for example, Butler et al., 2009; Chester, 2016; Thornton, 2017).

Hodgson (2017, p. 17) states that new departments will require funding and new positions although 'few universities can afford the luxury of both [mainstream and heterodox departments]'. This statement assumes only one possible funding model, and one that does not align with the cited Australian example which was created by transferring incumbents from the then economics department, new academic positions being created as student enrolments increased and funding provided through the federal government's imposition of (ever-increasing) student fees.

Furthermore, this strategy is devoid of contemporary higher education institutional realities. Today's universities are operated and managed like large corporations. Degrees and curricula are designed to deliver generic graduate attributes and learning outcomes, and disciplinary-specific knowledge and skills, that meet the needs of business and the 'market'.²³ University management decisions about degree and curriculum changes will be taken in this context along with assumptions about the 'price' (domestic and international) students are willing to pay. It is these realities that will determine if a new department of heterodox economics is formed not the need for new knowledge creation or an epiphany about the need for economic (methodological) pluralism.

According to Hodgson (2017, p. 17), the 'Political Economy' option is 'risky and unproven'. This may be the case in the UK; the cited Australian example is, however,

²³ For example, Australia's third largest export is education services, delivered by 43 universities, and in 2017 was valued greater than tourism.

'proven'. Created in 1999 as a 'discipline' within the Department of Economics (Faculty of Business) and transferred to the Faculty of Arts and Social Sciences in 2007, enrolments in the Department of Political Economy's teaching program quickly escalated and remain historically high relative to pre-2007; more recent lower enrolment levels mirror those of all Australian higher education social science and humanities enrolments.

Hodgson suggests that this alternative will not establish a 'positive' *raison d'être* which will preclude the building of a 'sufficient consensus'. There is no mention of what this strategy does or does not offer in terms of Hodgson's criteria of researcher incentives and quality control.

2.2 Attain Academic Positions in Non-Economics Departments

Hodgson's second alternative strategy is for heterodox economists to seek positions in noneconomics departments such as business schools which he claims, 'has allowed heterodox approaches to survive ... although it has done little to solve the problems of dissenting identity and *raison d'être'* (Hodgson, 2017, p. 18). He also asserts that if you identify as an economist, within a 'competitive' business school, the quality of your research will need to be judged against mainstream economics criteria especially journal rankings to ensure your 'survival'.²⁴

Business schools may offer some units of study that fall under the rubric of heterodox content although the likelihood of heterodox majors within business degree programs is remote. Thus, Hodgson's 'survival' in a business school does not mean the widespread embedding of heterodox economics content in the teachings of those schools rather a tolerance of individuals who identify as belonging to a school of economic thought 'contrary to the mainstream' and whose research practices are informed by a heterodox school(s) of economic thought.

It is well known, within the heterodox community, that academic heterodox economists are now most commonly found dispersed across schools and departments for business, finance, accounting or statistics and the social science disciplines of sociology, anthropology, political science, human geography, and education, amongst others. Thus, the levels of density (expertise concentration) of academic heterodox economists outside mainstream economics departments provide no scope within a higher education institution for sustained debates and arguments which, according to Hodgson is required to screen quality and claims for a science to develop its *raison d'être*. Thus, this strategy is flawed from the outset because it does not meet the first of Hodgson's criteria for viability and *ipso facto* cannot meet the other three criteria of researcher incentives, quality control and a degree of consensus.

2.3 Organise Around One Approach with Success and Potential

Hodgson's (2017, p. 18) third alternative strategy – which he considers will meet all four of his 'viability' criteria – is for a group of heterodox economists to 'organize around a particular approach that has evident success and future potential'. Modern monetary theory and Minsky's financial fragility work – both inspired by Post-Keynesian economics – are cited as successful examples of this 'concentration and consensus' strategy which needs to 'focus on ongoing, empirically-grounded research that repeatedly demonstrates to others the superiority of the core principles and ideas' (Hodgson, 2017, p. 18). Nevertheless, Hodgson advises that this strategy will only succeed through publications in leading (i.e. highly ranked) economics

²⁴ In other words, business schools use the rankings deployed by mainstream economics.

journals or the 'quick' establishment of influential 'own' journals to persuade the mainstream to pay attention.

The first point about this strategy is that it is the equivalent of 'strategic monism', the practice of the mainstream. Jackson (2018, p. 243) is supportive of such an approach contending that 'a cacophony of critical arguments can easily be swept aside by orthodoxy, whereas a single well-articulated alternative would be harder to ignore'.²⁵

However, what is the objective of heterodox economics that this third strategy presupposes? The first and second alternative strategies put forward by Hodgson are directed at trying to ensure some form of heterodox economics survival within the academy whereas the third strategy is framed to provide a single empirically-grounded approach as a counter to the mainstream. It is inconceivable how this strategy could ensure heterodoxy's survival given it is based on one approach drawn from the many schools comprising heterodox economics. It seems that Hodgson is now proposing 'survival' for one theoretical (and methodological) approach because of its perceived success and potential. Or does he think that the 'survival of the fittest' will generate spin-off benefits for the acceptance (legitimacy?) of the epistemic frames provided by all other heterodox schools?

The second point about this strategy is Hodgson's silence on which criteria should be used to assess 'evident success' and 'future potential' of the range of heterodox approaches, and who should be deemed appropriate to apply these criteria. No insights about these criteria are offered from the two cited successful strands of Post-Keynesian economics other than phrases such as 'some recognition' (by who?), 'impressive body' and 'huge potential impact'. One wonders why Hodgson considers that a group of heterodox economists could reach consensus and cooperate to promote one approach given his clearly stated view that heterodoxy has failed to reach a consensus about its definition and *raison d'être*. Maybe he considers that the self-interests of those in the group will suffice because selection of one approach will generate researcher incentives and quality control?

A third point about this strategy of 'picking a winner' is that it creates an epistemological division within heterodox economics; one approach or perspective is assessed as the most legitimate – because of its success and potential – to counter the position of the mainstream and all other schools are designated as not successful and/or lacking potential. This is certainly contrary to pluralism which promotes a diversity of views and understandings not a hierarchy or ranking of which is the 'best' epistemological approach to challenge, or provide an alternative to, the mainstream.

A further point about this strategy is the projected role of either mainstream or heterodox journals to ensure its success. Reardon (2008) demonstrates that the leading mainstream journals are not accessible to heterodox scholars. To be 'accessible' would mean the 'strategically selected' heterodox approach needs to be subservient to, and directly engage with the logic, concepts, frameworks and lexicon of the mainstream while also fitting a mainstream journal's aims and objectives otherwise the likelihood of being considered for publication is negligible. To follow this path places the selected strategic heterodox approach within the mainstream, not independent of but 'subsist[ing] parasitically' (Morgan and Embery 2018, p. 529), and echoes Colander's (2010) 'inside-the-mainstream' approach to blend heterodox and mainstream ideas.

²⁵ Jackson (2018, pp. 243-244) also advances several arguments in favour of strategic monism: heterodox identity is preserved; heterodox cooperation is encouraged; a divide-and-rule outcome is avoided; capture by the mainstream is prevented; and, enables communication with non-economists. These arguments are not mentioned by Hodgson (2017) possibly because he is seemingly preoccupied with 'quality, purpose and consensus'.

The establishment of new journals is equally problematic. Publishers are reluctant to consider new journals unless sponsored/supported by a learned society, academic or professional association to provide a substantive base for institutional and individual subscriptions. In addition, a new journal will only progress if the discourse space to be filled is clearly established. The 2016 *Heterodox Economics Directory* listed 145 journals (classified as general heterodox or in specialised categories).²⁶ To carve out a new heterodox journal niche presumes there is a considerable discourse gap not covered within existing journals. Why not instead focus effort on increasing the profile and impact of an existing heterodox journal that aligns with a 'strategically selected' heterodox approach such as the *Cambridge Journal of Economics*, the *Journal of Post Keynesian Economics*, the *Journal of Economic Issues* or the *Review of Political Economy*? Hodgson does not canvas such an option.

Furthermore, why would the mainstream pay attention to a non-mainstream journal? 'Orthodox economists feel no pressure to understand heterodox theory and will not be rebuked for their ignorance of it' (Jackson, 2018, p. 239). The mainstream is generally unaware of the existence of heterodoxy (Colander, 2010, fn. 7).

2.4 Privilege Institutions as the Analytical Concern

The fourth alternative strategy proposed by Hodgson, and probably not unsurprisingly given his institutionalist intellectual heritage, is to make economic institutions the object (*raison d'être*) of study which he proposes could be analysed in two ways: [a] from a broad range of disciplinary perspectives (e.g. law, sociology, philosophy, political science); or, [b] using 'techniques and approaches that are prominent in mainstream economics' (Hodgson, 2017, p. 19).

This strategy suffers similar flaws as Hodgson's other alternatives. Privileging institutions as the analytical concern consigns ideology, history, political decisions, path dependency, uncertainty, and other aspects – common to heterodox enquiry – to some form of second-order intellectual enquiry.

The application of prominent mainstream techniques and approaches could well demonstrate the limitations of mainstream theory to explain the complexities of the real world as well as heterodox scholars having the requisite technical skills. But will this be acknowledged by the mainstream given that:

'Mainstream economists have found it all too easy to find closed-system substitutes for heterodox claims or emphasises, once it is admitted that heterodox economists have made a point. Thus, uncertainty is mapped onto risk; evolutionary concepts are shorn of their Darwinianism and reinterpreted in terms of the requirements of non-linear or game theory modelling; care for others becomes a variable in a utility function; and so on' (Lawson, 2006, p. 497).

And, how will this advance the understanding and development of heterodox economics?

In addition, how could other disciplinary perspectives be 'encouraged' to analyse economic institutions? Why would theoretical perspectives from other social science (or maybe humanities?) disciplines want to shift their analytical focus to economic institutions? How will the application of multiple non-economic disciplinary approaches to the study of institutions advance the project of heterodox economics? Why are only economic institutions the proposed object of analysis?

²⁶ <u>http://heterodoxnews.com/directory/</u>

Furthermore, how does this strategy help heterodox economics overcome its purported consensus, quality and researcher incentive issues as portrayed by Hodgson? Answers to these questions cannot be gleaned from Hodgson's (2017) essay.

3. Judging Heterodox Economics

Claiming to apply ideas from the philosophy-sociology-social epistemology of science, Hodgson (2017) has sought to judge the progress of heterodox economics. Hodgson (2017) conveys the view that heterodox economics has failed because there is no 'definitional consensus' and if heterodoxy, as a scientific community, is to advance there needs to be 'effective enquiry', the quality of which will be ensured by 'screening' through social practices and institutions. In making these contentions, however, Hodgson (2017) does not identify all 'elements' of his analytical approach (refer Section 2.1), and applies social practices and institutions of the mainstream not those of heterodox economics.

Hodgson (2017) does not indicate which schools of economic thought he considers comprise the community of 'interacting' heterodox researchers, nor the social institutions that underpin this interaction, nor the social and material environment of the heterodox community. Perhaps this means that he does not consider there is a heterodox community of interacting researchers. Such a proposition, however, sits oddly with his considerable involvement over many years in, for example, the two international associations of EAEPE and WINIR, as founding editor of the *Journal of Institutional Economics* and active editorial involvement in the *Cambridge Journal of Economics* and many other journals, all of which could be classified as heterodox social institutions, interactions and practices.

The 1999 publication of the *Encyclopaedia of Political Economy* (O'Hara, 1999) and establishment in the same year of the UK-based Association for Heterodox Economists, the 2004 creation of the online *Heterodox Economics Newsletter* followed by the 2011 launch of the World Economics Association, are a few pivotal examples of the social practices and institutions of the heterodox economics community over recent decades.

The 2016 *Heterodox Economics Directory* lists more than 100 associations, over 150 journals, 10 regular international conferences, 25 publishers and 31 book series, 125 study programs and a range of newsletters, social media, mailing lists, and blogs. By end 2018, the *Heterodox Economics Newsletter* will have published nearly 240 issues.²⁷ This is further indication of the 'activity' – the social practices, interactions and institutions – of heterodox economics.

These heterodox social practices, interactions and institutions – and heterodox debates about, for example, pluralism, or closed and open systems of analysis, ideology and economics – are not considered by Hodgson (2017) who not only applies mainstream social practices and institutions (e.g. journal rankings) to assess the progress of heterodox economics but advocates the use of the same mainstream social practices for 'quality improvement' of heterodox economics. This is an 'ill-fitting' approach to judge the development and prospects of heterodox economics. Mainstream practices, such as the application of journal and research rankings to judge 'quality', have led to the marginalisation of heterodox economics. Yet Hodgson (2017) proposes that the standards, debate parameters and monist methodology of the mainstream be the practices and institutions – the benchmark – to improve the quality of heterodox.

²⁷ http://www.heterodoxnews.com/HEN/archive.html

²⁸ To paraphrase Terry McDonough's observation at the 2018 EAEPE Conference, this was advocated some decades ago by then leading heterodox scholars, such as Sam Bowles, but 'it did not work'.

This is not a view shared by heterodox economists. Putting aside the considerable difficulties for heterodox economists to achieve a sufficient 'density' of publications in highly-ranked mainstream, or gain competitive research grants, the response of the orthodoxy – at best – will be to 'selectively integrate' contributions without altering its foundations and give the semblance of 'openness' to criticism (see, for example: Boyer, 2017; Dow, 2011; Lawson, 2006).

Drawing on what I consider to be fallacious claims, Hodgson (2017) misconstrues the multi-layered and multi-faceted project of heterodox economics reflected through its evolving nature and the range of its analytical concerns and methodological approaches. The works of Fred Lee and Tony Lawson have provided compelling and complementary – not contradictory or unaccepted – definitions of the nature and scope of heterodox economics which is not reducible to critiques of the mainstream, is conducive to change, and, as such, is 'fit-for-purpose' to provide a cogent articulation of the context and outcomes of social inquiry, and the policy prescriptions arising.

In the early stage of its development, heterodox economics was in 'opposition' to the theory, methodology and policies of the mainstream. As one of the original institutional economists points out, 'critical' thinkers needed to position themselves *vis-à-vis* the mainstream (Ayres, 1936). Since then the purpose of heterodox economics has moved beyond 'dissent'. The recent publication of *The Routledge Handbook of Heterodox Economics* (Jo et al., 2018) demonstrates this evolution. The *Handbook's* contributions present a 'living body of knowledge' with real-world relevance, an on-going openness to theoretical and analytical developments, and coherent and logical policy alternatives.

The nature and scope of heterodox economics is not static and will continue to evolve. Colander et al. (2004, p. 486) described the economics discipline as 'a dynamic entity, which generates a self-reproducing, evolving, complex system of interacting ideas'. The same description can be applied to heterodox economics and does not preclude the insights provided by Lawson (2006) and Lee (2008; 2009). Hodgson, however, seems to want the description of heterodoxy to be fixed and universal, and exorcised of any dynamic, evolving nature.

Hodgson's (2017) proposed 'either or' strategies are, in my view, inherently flawed. There is no objective common to all four alternatives presented – two strategies are directed to some form of academic 'survival' for heterodox economics; the other two strategies create 'epistemological divisions' by privileging either a theoretical (and methodological) approach or analytical concern which is the antithesis of the constructive methodological pluralism underpinning heterodox economics. Moreover, I consider that Hodgson's (2017) strategies are devoid of the contemporary employment and research funding environment for heterodox economists and fail to account for the historical, geographic and cultural specificities for each of the different schools of economic thought that comprise heterodox economics. Each of his alternatives, in my view, will further marginalise heterodox economics.

To advance the project of heterodox economics – be it to construct an alternative paradigm to replace the prevailing mainstream paradigm, to promote tolerance and application of pluralism, to provide alternative understandings, and/or to teach economics through a range of methodological approaches – a diverse range of strategies, not a single strategy as proposed by Hodgson (2017), are needed given these realities and specificities.

Heterodox activity does not have the critical mass of the orthodoxy. Nevertheless, its 'quality', 'success' or 'progress' should not be judged against criteria constructed and deployed by the orthodoxy as Hodgson (2017) has done. But this does raise an important question for heterodoxy: how should we judge the progress and success of heterodox

economics? I suggest that the project of heterodox economics – however one chooses to define – provides one possible 'judge'.

Hodgson's (2017) claims also, I contend, raise other questions which are critical to the future of heterodoxy – Should strategies be deployed to counter perennial criticisms of heterodox economics and if so, what should be these strategies? What roles do, and should, pluralism and interdisciplinarity play *vis-à-vis* heterodox economics? What strategies may advance the project(s) of heterodox economics, including its teaching and policy authority, and overcome existing barriers? These are important questions which require renewed (and ongoing) public dialogue and debate for the evolution of heterodox economics if – as Jamie Morgan reminds us in the open peer review comments to this paper – we are to collectively improve the understanding of the forces driving the functioning of contemporary economies, and solutions for 'better' outcomes.

Acknowledgements

I wish to thank the following for their comments, suggestions and advice which have informed the drafting of this paper: Bernard Chavance, Carlo D'Ippoliti, Claudius Gräbner, Tae-Hee Jo, Robert McMaster, Terry McDonough, Jamie Morgan, Paolo Ramazzotti, Alice Sindzingre, Bill Waller and the participants in the 2018 EAEPE Conference sessions on the 'History, definition and theoretical cores of heterodox economics'. Any errors or omissions are, of course, those of the author.

References

Ayres, C. (1936) 'Fifty years' development in ideas of human nature and motivation.' *American Economic Review*, 26(1), pp. 224-236.

Bigo, V. (2006) 'Open and closed systems and the Cambridge School.' *Review of Social Economy*, 64(4), pp. 493-514.

Boyer, R. (2017) 'Orthodoxie, heterodoxies et capitalismes contemporains.' *Revue de la Régulation: Capitalisme, Institutions, Pouvoirs,* 22, Autumn. Online. Available at: <u>https://journals.openedition.org/regulation/12337</u>

Butler, G., Jones, E. and Stilwell, F.J.B. (2009) *Political Economy Now! The Struggle for Alternative Economics at the University of Sydney.* Darlington: The University of Sydney.

Caldwell, B. (1988) 'The case for pluralism.' In de Marchi, Neil (ed), *The Popperian Legacy in Economics*, Cambridge: Cambridge University Press, pp. 231-244.

Cedrini, M. and Fontana, M. (2017) 'Just another niche in the wall: How specialisation is changing the face of mainstream economics.' *Cambridge Journal of Economics*, 42(2), pp. 427-451.

Chester, L. (2016) 'The state of Australian heterodox economics.' International Conference 'Governing Economics: Institutional Changes, New Frontiers and the State of Pluralism' organised by CRIISEA (Université de Picardie, Amiens) and IRISSO (Dauphine Université Paris), Amiens, 7-8 December 2016.

Chick, V. and Dow, S. (2005) 'The meaning of open systems.' *Journal of Economic Methodology*, 12(3), pp. 361-381.

Colander, D. (2010) 'Moving beyond the rhetoric of pluralism: Suggestions for an "inside-themainstream" heterodoxy.' In Garnett, R., Olsen, E.K. and Starr, M. (eds), *Economic Pluralism,* Abingdon, Oxon: Routledge, pp. 36-47.

Colander, D. (2004) *Economics as an Ideologically Challenged Science*. Middlebury College Economics Discussion Paper No. 04-11, September 2004. Online. Available at: http://ftp.repec.org/opt/ReDIF/RePEc/mdl/ancoec/0411.pdf

Colander, D., Holt, R. and Rosser, J.B. (2010), 'How to win friend and (possibly) influence mainstream economists', *Journal of Post Keynesian Economics*, 32: 397-408.

Colander, D., Holt, R. and Rosser, J.B. (2007/08) 'Live and dead issues in the methodology of economics.' *Journal of Post-Keynesian Economics*, 32(3), pp. 303-312.

Colander, D., Holt, R. and Rosser, J.B. (2004) 'The changing face of mainstream economics.' *Review of Political Economy*, 16(4), pp. 485-499.

Courvisanos, J. (2016) 'In from the cold: From heterodoxy to a new mainstream pluralism.' In Courvisanos, J., Doughney, J. and Millmow, A. (eds), *Reclaiming Pluralism in Economics: Essays in Honour of John E. King*, London: Routledge, pp.303-316.

Coyle, D. (2007) *The Soulful Science: What Economists Really Do and Why it Matters.* Princeton: Princeton University Press.

Davis, J.B. (2008) 'The turn in recent economics and return of orthodoxy.' *Cambridge Journal of Economics*, 32(3), pp. 279-302.

Davis, J.B. (2006) 'The turn in economics: Neoclassical dominance to mainstream pluralism.' *Journal of Institutional Economics*, 30(2), pp. 1-20.

Davis, J.B. (2005) 'Heterodox economics, the fragmentation of the mainstream and embedded individual analysis.' In Garnett, R. and Harvey, J. (eds), *The Future of Heterodox Economics,* Ann Arbor, MI: University of Michigan Press, pp. 53-72.

D'Ippolito, C. and Roncaglia, A. (2015) 'Heterodox economics and the history of economic thought.' In Jo, T-H and Todorova, Z. (eds), *Advancing the Frontiers of Heterodox Economics: Essays in Honor of Frederic S. Lee,* London: Routledge, pp. 21-38.

Dow, S. (2011) 'Heterodox economics: history and prospects.' *Cambridge Journal of Economics*, 35(6), pp. 1151-1165.

Dow, S. (2004) '*Reorienting Economics:* Some epistemological issues.' *Journal of Economic Methodology*, 11(3), pp. 307-312.

Dowd, D. (2000) Capitalism and Its Economics: A Critical History. Sterling, Virginia: Pluto Press.

Elsner, W. (2017) 'Social economics and evolutionary institutionalism today.' *Forum for Social Economics*, 46(1), pp. 52-77

Foley. D. (2003) 'Rationality and ideology in economics.' Talk to New School Graduate Class. Online. Available at:

https://pdfs.semanticscholar.org/0c54/60b46cde35edb969f4d38f6205b8460fff54.pdf

Foley, D. (1975) 'Problems vs. conflicts: Economic theory and ideology.' *American Economic Review*, 65(2), pp. 231-236.

Fontana, G. and Gerrad, B. (2006) 'The future of Post Keynesian economics.' *Banca Nationale del Lavoro Quarterly Review*, 59(236), pp. 49-80.

Fullbrook, E. (2009) *Pluralist Economics*. London: Zed Books.

Garnett, R., Olsen, E.K. and Starr, M. (eds) (2009) *Economic Pluralism*. London: Routledge.

Goldman, A. and Blanchard, T. (2018) 'Social Epistemology.' In Zalta, E.N. (ed), *The Stanford Encyclopedia of Philosophy* (Summer 2018 Edition). Online. Available at: https://plato.stanford.edu/archives/sum2018/entries/epistemology-social/

Hodgson, G. (2017) 'The pathology of heterodox economics and the limits to pluralism.' Draft of 12 July 2017. Online. Available at: <u>https://winir.org/content/uploads/Conference_Papers_2017/WINIR2017_Geoffrey-</u> Hodgson_989462_1504184217.pdf

Hodgson, G., Maki, U. and McCloskey, D. (1992) 'A plea for a rigorous and realistic economics.' *American Economic Review*, 82(2), p. xxv.

Jackson, W.A. (2018) 'Strategic pluralism and monism in heterodox economics.' *Review of Radical Political Economics*, 50(2), pp. 237-251.

Jo, T-H, Chester, L. and D'Ippoliti, C. (eds). (2018a) *The Routledge Handbook of Heterodox Economics.* London: Routledge.

Jo, T-H, Chester, L. and D'Ippoliti, C. (2018b) 'The state of art and challenges for heterodox economics.' In Jo, T-H, Chester, L. and D'Ippoliti (eds), *The Routledge Handbook of Heterodox Economics,* London: Routledge, pp. 3-26.

Kitcher, P. (1993) *The Advancement of Science: Science without Legend, Objectivity without Illusions.* Oxford: Oxford University Press.

Lawson, T. (2013) 'What is this "school" called neoclassical economics?' *Cambridge Journal of Economics*, 37(5), pp. 947-983.

Lawson, T. (2009) 'On the nature and roles of formalism in economics.' In Fullbrook, E. (ed.), *Ontology and Economics: Tony Lawson and His Critics,* London: Routledge, pp. 189-231.

Lawson, T. (2006) 'The nature of heterodox economics.' *Cambridge Journal of Economics*, 30(4), pp. 483-505.

Lawson, T. (2004) '*Reorienting Economics*: On heterodox economics, themata and the use of mathematics in economics.' *Journal of Economic Methodology*, 11(3), pp. 329-340.

Lawson, T. (2003) *Reorienting Economics*. London: Routledge.

Lee. F.S. (2016) 'Critical realism, method of grounded theory, and theory construction.' In Lee, F.S. and Cronin, B.C. (eds.), *Handbook of Research Methods and Applications in Heterodox Economics,* Cheltenham, UK: Edward Elgar, pp. 35-53.

Lee, F.S. (2012), 'Heterodox economics and its critics.' *Review of Political Economy*, 24(2), pp. 337-351.

Lee, F.S. (2011/12) 'To be or not to be heterodox: Responding to heterodox critics and their arguments.' *Revue Française de Socio-Économie*, 8, pp. 123-144.

Lee, F.S. (2009) A History of Heterodox Economics: Challenging the Mainstream in the Twentieth Century. London: Routledge.

Lee, F.S. (2008), 'Heterodox economics.' In Durlauf, S.N. and Blume, L. E. (eds), *The New Palgrave Dictionary of Economics,* Second edition, London: Palgrave Macmillan, pp. 5790-5796. Online. Available at:

https://link-springer-com.ezproxy1.library.usyd.edu.au/referenceworkentry/10.1057/978-1-349-95189-5 2487#howtocite

Lee, F.S. (2002) 'Theory creation and the methodological foundation of Post Keynesian economics.' *Cambridge Journal of Economics*, 26(6), pp. 789-804.

Lee, F.S., Cronin, B., McConnell, S. and Dean, E. (2010) 'Research quality rankings of heterodox economic journals in a contested discipline.' *American Journal of Economics and*

Sociology, 69(5), pp. 1409-1452.

Lee, F.S. and Elsner, W. (2010) *Evaluating Economic Research in a Contested Discipline: Rankings, Pluralism and the Future of Economics.* Oxford: Wiley Blackwell.

Lee, F.S. and Lavoie, M. (eds) (2013) *In Defence of Post-Keynesian and Heterodox Economics: Responses to Their Critics.* London: Routledge.

Mearman, A. (2012) "Heterodox economics" and the problems of classification.' *Journal of Economic Methodology*, 19(4), pp. 407-424.

Mearman, A. (2011) 'Who do heterodox economists think they are?' American Journal of Economics and Sociology, 70(2), pp. 480-510.

Morgan, J. (ed) (2016) *What is Neoclassical Economics? Debating the Origins, Meaning and Significance.* London: Routledge.

Morgan, J. (2014) 'What's in a name? Tony Lawson on neoclassical economics and heterodox economics.' *Cambridge Journal of Economics*, 39(3), pp. 843-865.

Morgan, J. and Embrey, J. (2018) 'Heterodox economics as a living body of knowledge: Community, (in)commensurability, critical engagement and pluralism.' In Jo, T-H, Chester, L. and D'Ippoliti, C. (eds) (2018a) *The Routledge Handbook of Heterodox Economics,* London: Routledge, pp. 515-533.

Myrdal, G. (1965) *The Political Element in the Development of Economic Theory.* Cambridge, Mass.: Harvard University Press.

Reardon, J. (2008), 'Barriers to entry: Heterodox publishing in mainstream journals', *On the Horizon*, 16(4): 185-197.

O'Hara, P.A. (2007) 'Principles of institutional-evolutionary political economy – Converging themes from the schools of heterodoxy.' *Journal of Economic Issues*, XLI(1), pp. 1-42.

O'Hara, P.A. (ed) (1999) Encyclopaedia of Political Economy. London: Routledge.

Reardon, J. (2015) 'Roundtable dialogue of pluralism.' *International Journal on Pluralism and Economics Education*, 6(2), pp. 272-308.

Rosser, J.B., Colander, D. and Holt, R. (2013) 'How can something so right as heterodox economics have so little influence.' Unpublished Paper. Online. Available at: cob.jmu.edu/rosserjb

Salanti, A. and Screpanti, E. (eds) (1997) On Pluralism in Economics: New Perspectives in *History and Methodology.* Cheltenham, UK: Edward Elgar.

Samuels, W. (1979) 'Ideology in economics.' In Weintraub, S. (ed) *Modern Economic Thought,* Philadelphia: University of Pennsylvania Press, pp. 467-484.

Schumpeter, J. (1949) 'Science and ideology.' American Economic Review, 39(2), pp. 346-359.

Thornton, T. (2017) *From Economics to Political Economy: The Problems, Promises and Solutions of Pluralist Economics.* London: Routledge.

SUGGESTED CITATION:

Chester, Lynne (2019) 'Judging Heterodox Economics: A Response to Hodgson's Criticisms.' *Economic Thought*, 8.1, pp. 1-21. <u>http://www.worldeconomicsassociation.org/files/journals/economicthought/WEA-ET-8-1-Chester.pdf</u>

The Meaning and Future of Heterodox Economics: A Response to Lynne Chester

Geoffrey M. Hodgson, Loughborough University London <u>g.m.hodgson@herts.ac.uk</u>

1. Introduction: An Outline of the Argument

I have been writing and publishing in economics for 50 years and much of my work has been debated and criticised. But I think that this is the first time that someone has honoured me by a full-scale article criticising an unpublished working paper. I am very grateful to Lynne Chester for bringing the questions I raise to a wider audience.

The working paper that she criticizes went through several versions, of which the 12 July 2017 draft that Lynne downloaded from the World Interdisciplinary Network for Institutional Research (WINIR) website is not the final version. In addition, the working paper has now expanded into a book entitled *Is There a Future for Heterodox Economics?* (Hodgson, 2019). Lynne's criticisms help me to attempt to make the text clearer and deal with some misunderstandings that have arisen.

One reason which I turned the essay into the book was the necessity to expand and clarify on key points. Lynne gets several things wrong in her critique, so perhaps this need is confirmed. Lynne makes many points of importance that cannot be answered adequately here. They are addressed more completely in *Is There a Future for Heterodox Economics?*

For example, Lynne points out that in the working paper 'Hodgson assiduously avoids any self-identification as an economist, heterodox or otherwise'. This omission was not 'assiduous'. In earlier publications and the 2019 book I reveal myself as an unashamed heterodox economist, because I reject the utility-maximizing, equilibrium-oriented core of neoclassical economics.

Lynne chooses to 'view' my criticisms as 'criticisms of heterodoxy'. On the contrary, the book and the working paper make it clear that my project is not principally a critique of the content of heterodox economics as such. The few criticisms I make are secondary to the main objective, which is to investigate why heterodox economics is not as influential as it could be.

What has gone wrong? Despite the power of some of its theories, and despite major success in some areas – such as the methodology of economics and in monetary theory – heterodox economics has not made sufficient headway in terms of influence over the last 50 years. That is my starting point.

Of course, particularly since the financial crash in 2008, the community of heterodox economists has expanded, and its publications have proliferated. But its power in departments of economics has waned. This weakness does not necessarily stem from heterodox theories being wrong. Some are, and some are not: but that is not the point. It is more that heterodoxy has failed to gain overall influence that is commensurate with its best work. This paradox is the starting point of the narrative in my 2017 working paper and in my 2019 book.

In addition, heterodox research is insufficiently cumulative. It returns almost ritually to Keynes and Marx, while giving much less attention to many other important dissident economists. Many interesting insights by current authors are published and then forgotten. There are insufficient incentives to build upon them.

Heterodoxy also has widely-perceived problems of quality control. In my judgement, published heterodox research ranges in its quality from the excellent to the extremely poor. Too many heterodox journals and conferences contain too much low-quality material, reducing incentives for outsiders to engage with the heterodox community. Admittedly, these verdicts are largely matters of personal judgment. But heterodox economics is widely perceived to be of inadequate quality, including by some heterodox economists. In any case, heterodox economics has a problem that its quality is widely *perceived* as deficient. This problem should not be simply brushed aside.

To understand and address all these problems, we must analyse how scientific disciplines work as ensembles of social institutions, with their own internal culture. Scientific disciplines are organised systems of power. However imperfectly, these systems control quality and create incentives for enduring participation and engagement.

We have to apply social science to the analysis of economics as an institutionalised discipline. Publishing more and more heterodox books and articles is insufficient for heterodox progress. There needs to be an understanding of how academic disciplines work as organised systems of power. Strategies must be developed to install new, or capture old, centres of academic power.

Two major studies of sciences as systems of institutionalised power and authority were by Michael Polanyi (1962) and Philip Kitcher (1993). They both emphasise that pluralism (of opinion and approach) is vital for innovation and progress in science. But they also explain why an excess of pluralism would lead to chaos. If everything is ripped apart critically, and then rivalled by some new and short-lived alternative, over and over again, then sciences cannot build cumulatively. For cumulative advance, pluralism has to be adequate but restrained. While pluralism is essential, there is a trade-off between pluralism and consensus.

Sheila Dow (2007, p. 448) identified the problem of an 'unstructured pluralism or eclecticism, understood as an absence of selection criteria, or "anything goes", is antithetical to the building up of knowledge': it would lead to chaos or stagnation. I go further in my 2019 book, by also addressing the work of Polanyi and Kitcher. Both these authors emphasised that some degree of power and consensus is also important for quality control within a discipline.

Another major problem, raised in my 2017 working paper and at much greater length in my 2019 book, is that while heterodox economists form an identifiable community and network, they cannot agree on what heterodoxy means. The obverse of this is a similar failure to agree on the essential nature of orthodox economics – if there were a consensus on the nature of orthodoxy then agreement on the nature of heterodoxy could be achieved.

There is wide acknowledgement that this failure to agree on the nature of heterodoxy is a problem. Tony Lawson (2006, p. 484) wrote that 'very few ... have questioned the nature of heterodox economics' and when queried 'it is recognised ... as an umbrella term to cover ... *separate* heterodox projects or traditions'. Andrew Mearman (2011, p. 480) analysed a survey of members of the Association for Heterodox Economics and found 'little agreement on any core concepts or principles' and 'that there is little structure to heterodox economics beyond that provided by pre-existing (or constituent) schools of thought' (which are, most importantly, post-Keynesianism and Marxism). Clive Spash and Anthony Ryan (2012) conducted a different survey that confirmed a similar result.

Similarly, the editors of *The Routledge Handbook of Heterodox Economics*, of whom Lynne is one, concur that the question of the nature of heterodoxy 'has been the subject of a long-standing debate by heterodox economists although no consensus has been reached' (Jo et al., 2017, p. 8).

This leads to an obvious question, which I discuss at length in my 2019 book. If heterodox economists cannot agree on the nature of heterodoxy, then what keeps heterodox economists together as a community? What ties them together over a period of time? My answer is that it is largely to do with left-wing ideology. There are exceptions. Austrian economists and Nelson-Winter style evolutionary economists are regarded by some (but not all) in the heterodox community as being also heterodox. Many Austrian and evolutionary economists are not left-wing. But these cases are largely outliers. I demonstrate in my book that the history of heterodox economics, particularly the major stream that derived from the University of Cambridge in the 1960s and 1970s, were explicitly aligned with left-wing politics. This alignment is also clearly apparent in Fred Lee's (2009) *History of Heterodox Economics*.

Of course, there is nothing in principle wrong with being ideological. But when a particular ideology substitutes for failure to agree on a theoretical core, then something is awry. Cumulative scientific progress becomes more difficult. A healthy social science discipline relies not simply on a pluralism of *theories*, it is also helped by a diversity of *ideological* viewpoints. Viewpoint diversity helps scholars reach across ideological dividing lines and make their theories attractive for a wider audience.

This is a key test for heterodoxy. Some heterodox scholars have explicitly rejected conversations across ideological divides. Lee was one: he saw heterodox economics as part of the struggle against capitalism. But while social science is unavoidably entwined with ideology, it should not be reduced to ideology alone. Viewpoint diversity is invaluable. Heterodox economists who reject this line of argument are not being pluralist enough.

2. Some of the Errors in Lynne Chester's Account of my Project

I now identify some major errors in Lynne's account. I do not cite them all. I do not wish to over-burden the reader. But her errors come thick and fast.

First, the title of Lynne's essay is misleading. 'Judging heterodox economics ...' may be misconstrued by some readers about the content of heterodoxy, about the merits and demerits of particular theories. Neither my 2017 working paper nor my forthcoming book are on that topic. Instead, the aim is made clear in the abstract of my 2017 working paper: 'This paper addresses the possible meanings and potential of "heterodox economics" as an organizing label.' And in its opening paragraph: 'This essay explores the nature, boundaries and future of "heterodox economics". The aim is not to appraise particular heterodox theories, whatever they may be, but to examine the use of the "heterodox economics" label to organise opposition to mainstream economics and to develop alternatives to it.' The 2019 book has a similar aim.

Another error appears in the abstract, where Lynne claims that I allege that heterodox economics 'does not take pluralism seriously'. I do not say that. Instead, the 2017 working paper has the following claim, which appears towards the end of the draft: 'heterodox economists also need to take pluralism itself more seriously'. A plea to take something 'more seriously' does not imply that it is not already taken seriously. On the contrary, it is already taken seriously, but not seriously enough.

In the abstract Lynne wrote: 'To address these alleged problems, Hodgson proposes four alternative strategies'. It is true that I do consider four strategies for heterodoxy in the 2017 paper. But I do not 'propose' them in sense of advancing them as equally feasible or desirable. The strategies are put forward for discussion. They are neither fixed nor final. In my 2019 book I consider eight strategies, and suggest that four of them are more plausible than the others. But I also make it clear in the book that strategy in a complex social system must

rely on experiment and not merely design. We must all humbly admit that we do not know in advance, and for sure, what works best.

One of the errors appearing in the introduction is this: 'Hodgson treats the issue of pluralism as some sort of "quality control" measure needed by the heterodox economics community'. I say no such thing. The 2017 draft makes clear that pluralism has a quite different role. I wrote: 'Some internal pluralism or diversity within any academic school or discipline is vital. Internal debate is necessary for theoretical advance: diversity and dissent provide the fuel for theoretical innovation.' Nowhere do I say that the role of pluralism is for quality control. On the contrary, I argue in the paper that quality control depends on some degree of *consensus*. This, to a degree, and in ways I explore, runs against pluralism. Both pluralism and consensus are necessary, but for different reasons.

Another error appearing in the introduction is: 'Hodgson summarily ascribes "leftist political leanings" to all heterodox economists'. The 'all' here is incorrect: nowhere do I say 'all'. For example, Austrian economics can legitimately be regarded as heterodox and many of them are not leftists, as conventionally defined.

This mistake leads to yet another error, where Lynne says that Hodgson 'infers all heterodox economists are ideologically-driven and all mainstream economists are politically neutral.' Both parts of this statement are false. Nowhere do I say that all heterodox economists are ideologically driven. Some are. But that is not necessarily a flaw. And nowhere do I say that mainstream economists are politically neutral. That would be absolute nonsense. Many mainstream economists are well-known for their ideological positions.

Lynne also wrote: 'Hodgson criticises Lee for not including as heterodox the schools of Austrian, institutional, evolutionary or Sraffian economics.' I admit an error here and I thank Lynne for her correction. But in our many personal conversations in the 30-plus years that I knew him, Fred Lee argued with me more than once that Austrian school economists were mainstream or neoclassical. I contested this with him. I now accept that, sometime before 2009, Lee changed his position and admitted the Austrians into heterodoxy. This should have been recognised in my 2017 paper and I was in error. This error is already fully corrected in my forthcoming book. In addition, in the 2017 paper, I should have made it clearer that Lee excluded Sraffian economics in some, but not all, of his stated listings of what comprises heterodox economics. Again, this is already corrected in later drafts of my working paper and in my forthcoming book.

The remainder of Lynne's sentence is false. I do not accuse Lee of excluding 'institutional' or 'evolutionary' economics from his listings. Instead I note a possible exclusion of the *new* institutional economics and I wrote: 'The "evolutionary economics" of Richard Nelson and Sidney Winter (1982) was also excluded from Lee's (2008) list of "heterodox" approaches.' I stand by this claim. This is quite different from Lynne's inaccurate account of what I wrote.

I now turn to Lynne's discussion of Tony Lawson's arguments, where she writes: 'Lawson has repeatedly refuted the notion that his definition suggests blanket opposition by heterodox economists to the use of mathematical formalism.' She claims that I fail to recognise this aspect of Lawson's position. I do not. I stand by what I say on this in my 2017 paper, where I conclude: 'Consequently, for Lawson, mathematics is appropriate for economics in highly limited circumstances only.' I am fully aware that Lawson (2003, pp. xix, p. 27, pp.178–9; 2006, p. 49; 2009, p. 19) has repeatedly insisted that he was not 'anti-mathematics' in principle. Instead, he is against the 'abuse' of mathematics and against the dogmatic insistence by the mainstream that mathematics must always be used.

But these statements have to be read alongside Lawson's own explicit criteria for when it is appropriate to use mathematics. He argued that mathematics would be suitable for

economics only in the extraordinary circumstances of approximation to what he called a 'closed system'. To repeat what I quoted in my 2017 essay: Lawson (2003, pp. 21, 178) clearly regards the appropriate circumstances for the use of mathematics as 'seemingly rare' or 'rather rare'.

Lynne approvingly cites claims that the definitions of heterodoxy by Lee and Lawson are 'complementary'. I question this. Unlike Lee, Lawson has never claimed that Sraffian economics or Post Keynesian models are heterodox. Lawson excludes important streams that Lee includes as 'heterodox'.

In my 2017 paper, and at greater length in my 2019 book, I argue that Lawson's criteria would imply that non-mathematical economists such as Ronald Coase, Douglass North, Richard Posner and Oliver Williamson were heterodox, and that most of economics before 1950 was heterodox. In this respect, Lawson's argument is very different from Lee's (2009), who argues that heterodox economics is explicitly or incipiently anti-capitalist. Lawson's criteria lead implicitly to many inclusions as 'heterodox' that Lee vigilantly excludes.

3. The Question of Strategy for the Heterodox Community

I have already noted that Lynne misunderstands my purpose in laying out multiple strategic options for the heterodox community. My aim is to start a discussion about strategy that has so far been largely neglected. One of the few attempts to consider strategic options is the proposal of a 'transdisciplinary' strategy of unifying the social sciences, formulated by David Colander and others (Colander et al., 2010; Colander, 2014; Colander and Kupers, 2014). This is one of the eight strategies that I discuss in my 2019 book.

Heterodoxy will not gain influence and power simply by producing numerous books, articles and newsletters, or by holding more conferences. It is also unhelpful to ignore mainstream rankings, which Lynne and others are keen to dismiss. Strategies need to address the existing reality. Having more meetings and producing more output are not enough. The risk is that much of these will be ignored. Critics have to understand and engage with existing structures or power.

Lynne claims that my strategic views and suggestions are wrong. I would more humbly suggest that we need to experiment – to see what works. To that end, I confine the remarks here to only one of the eight possible strategies that I lay out in my 2019 book. I call this a 'strategy of separation' and it is exemplified by the long but eventually-successful struggle at the University of Sydney to form a Department of Political Economy separate from the Economics Department.

Lynne claims that the ultimate success of this strategy is 'proven', as evidenced by the high level of student recruitment. This is important, but it is hardly a sufficient criterion of success. Indeed, it succumbs to a view that student numbers are the main criterion of university health, which today has sadly become the one-sided opinion of many university managers throughout the world. 'Bums on seats' they say.

Lynne also warns us of the pecuniary and business considerations that dominate universities, while being unclear about alternative options. For instance, in response to my argument that few universities can afford the luxury of both mainstream and heterodox departments, Lynne responded: 'This assumes only one possible funding model'. On the contrary, no one funding model is implied or assumed. In her response, Lynne ignores the fact that even if a new department is formed by transferring existing staff, the creation of additional (smaller) departmental administrations is costly, with possible diseconomies of scale. Presumably, Lynne's remark that today 'universities are operated and managed like large corporations' is meant to remind of us the business orientation of modern universities. In fact, most universities have been corporations since their inception, including the ancient institutions at Cambridge and Oxford, founded in the 13th and 11th centuries respectively. At least in the UK, all universities and registered charities are corporations, and always have been. Perhaps Lynne meant that universities are run like 'business corporations' with a modern business culture. This would underline the actual and potential threats of a capitalist business mentality to the formation of a separate department of political economy, on ideological or cost-cutting grounds.

In the Department of Political Economy in Sydney, Marxism and left politics are dominant. In 2018, the Department of Political Economy boasted four Honorary Professors, namely Terrence McDonough, James Stanford, Yanis Varoufakis and (the late) Erik Olin Wright. All four have taken a strong left political stance, and three of them have declared Marxist allegiances. The department website also announces numerous critiques of an undefined ideology of *neoliberalism*. (On this see Boas and Gans-Morse, 2009, for example.) There are also quaint retro-1970s seminars on Marxian value theory and the transformation problem. By this evidence, pluralism in the Sydney department does not seem to stretch very far beyond Marxism and its fellow travellers. Pluralism is preached, but not taken seriously enough.

With the failure to establish a clear *theoretical* identity for heterodoxy, it is all too tempting to use leftist ideology as the alternative glue to bind people together. But to achieve this, a false theoretical narrative has to be manufactured, where *neoclassical economics* is deemed to be necessarily pro-market or right wing (Jones and Stilwell, 1986; Butler et al., 2009). On the contrary, neoclassical theory has been used to support socialism as well as capitalism (e.g. Lerner, 1934; Lange and Taylor, 1938). Furthermore, a number of 'analytical' Marxists have advocated the use of neoclassical approaches such a general equilibrium theory (e.g. Roemer, 1981; 1986). This is not to say that any approach is ideologically neutral, but neoclassical economics is ideologically far more adaptable than is often presumed.

While student interest and recruitment may be high, the Department of Political Economy at the University of Sydney is not yet highly visible in prominent research rankings. By contrast, the School of Economics, also within the Faculty of Arts and Social Sciences at the same university, has achieved relatively high international rankings (RePEc, 2019). Although some heterodox economists may dismiss these criteria as 'mainstream', it is not a very comfortable position for Political Economy in Sydney. It would be tempting for an administrator to fuse the two together under *de facto* mainstream control.

The Sydney experience illustrates some of the major potential problems with a strategy of separation. Without a sufficiently clear *raison d'être* for the breakaway department, and with a flawed understanding of the nature and ideological implications of neoclassical theory, a department of political economy can end up as a largely ideological grouping, with an excessively constrained pluralism in theory, and an even more limited pluralism of political viewpoints.

The Sydney experiment is useful, but I am far from convinced that it shows a way forward for heterodox economics. We need to experiment, with some understanding of how scientific disciplines function as institutionalised systems of power.

References

Boas, Taylor C. and Gans-Morse, Jordan (2009) 'Neoliberalism: From New Liberal Philosophy to Anti-Liberal Slogan.' Studies in Comparative International Development, 44(2), June, pp. 137-61.

Butler, Gavan, Jones, Evan and Stilwell, Frank (2009) Political Economy Now! The Struggle for Alternative Economics at the University of Sydney. Sydney: Darlington Press.

Colander, David C. (2014) 'The Wrong Type of Pluralism: Toward a Transdisciplinary Social Science.' Review of Political Economy, 26(4), October, pp. 516-25.

Colander, David C. and Kupers, Roland (2014) Complexity and the Art of Public Policy. Princeton: Princeton University Press.

Colander, David C., Kupers, Roland, Lux, Thomas and Rothschild, Casey (2010) 'Reintegrating the Social Sciences: The Dahlem Group', Middlebury College Economics Discussion Paper No. 10-33. Available at

http://sandcat.middlebury.edu/econ/repec/mdl/ancoec/1033.pdf (Retrieved 2 May 2019.)

Dow, Sheila C. (2007) 'Varieties of Methodological Approach in Economics.' Journal of Economic Surveys, 21(3), July, pp. 447-465.

Groenewegen, Peter D. (1985) 'Professor Arndt on Political Economy: A Comment.' Economic Record, 60(4), December, pp. 744-51.

Groenewegen, Peter D. (1987) "Political Economy" and "Economics." In John Eatwell, Murray Milgate and Peter Newman (eds) (1987) The New Palgrave Dictionary of Economics, vol. 3, pp. 904-7.

Hodgson, Geoffrey M. (1999) Evolution and Institutions: On Evolutionary Economics and the Evolution of Economics. Cheltenham: Edward Elgar.

Hodgson, Geoffrey M. (2019) Is There a Future for Heterodox Economics? Institutions, Ideology and a Scientific Community. Cheltenham UK and Northampton MA: Edward Elgar, forthcoming.

Jo, Tae-Hee, Chester, Lynne and D'Ippoliti, Carlo (eds) (2017) The Routledge Handbook of Heterodox Economics: Theorizing, Analyzing, and Transforming Capitalism. London and New York: Routledge.

Jones, Evan and Stilwell, Frank (1986) 'Political Economy at the University of Sydney.' In Martin, Brian, Baker, C. M. Ann, Manwell, Clyde and Pugh, Cedric (eds) (1986) Intellectual Suppression: Australian Case Histories, Analysis and Responses, Sydney: Angus & Robertson, pp. 24-38.

Kitcher, Philip (1993) The Advancement of Science: Science without Legend, Objectivity without Illusions. Oxford and New York: Oxford University Press.

Lange, Oskar R. and Taylor, Frederick M. (1938) On the Economic Theory of Socialism. Ed. Benjamin E. Lippincot. Minneapolis: University of Minnesota Press.

Lawson, Tony (2003) Reorienting Economics. London and New York: Routledge.

Lawson, Tony (2006) 'The Nature of Heterodox Economics.' Cambridge Journal of Economics, 30(4), July, pp. 483-505.

Lawson, Tony (2009) 'On the Nature and Roles of Formalism in Economics.' In Fullbrook, Edward (ed.) (2009) Ontology and Economics: Tony Lawson and His Critics, London and New York: Routledge, pp. 189-231.

Lee, Frederic S. (2009) A History of Heterodox Economics: Challenging the Mainstream in the Twentieth Century. London and New York: Routledge.

Lerner, Abba P. (1934) 'Economic Theory and Socialist Economy.' *Review of Economic Studies*, 2(1), pp. 157-75.

Mearman, Andrew (2011) 'Who do Heterodox Economists Think They Are?' *American Journal of Economics and Sociology*, 70(2), April, pp. 480–510.

Nelson, Richard R. and Winter, Sidney G. (1982) *An Evolutionary Theory of Economic Change*. Cambridge, MA: Harvard University Press.

Polanyi, Michael (1962) 'The Republic of Science: Its Political and Economic Theory.' *Minerva*, 1, pp. 54-73.

RePEc (2019) 'Top 25% Economics Departments, as of March 2019.' *Ideas.* <u>https://ideas.repec.org/top/top.econdept.html</u> (Retrieved 2 May 2019.)

Roemer, John E. (1981) *Analytical Foundations of Marxian Economic Theory.* Cambridge: Cambridge University Press.

Roemer, John E. (ed.) (1986) Analytical Marxism. Cambridge: Cambridge University Press.

Spash, Clive L. and Ryan, Anthony (2012) 'Economic schools of thought on the environment: investigating unity and division.' *Cambridge Journal of Economics*, 36, pp. 1091-1121.

SUGGESTED CITATION:

Hodgson, Geoffrey, M. (2019) 'The Meaning and Future of Heterodox Economics: A Response to Lynne Chester.' *Economic Thought*, 8.1, pp. 22-29. <u>http://www.worldeconomicsassociation.org/files/journals/economicthought/WEA-ET-8-1-Hodgson.pdf</u>

Was Smith A Moral Subjectivist?

Kevin Quinn, Bowling Green State University, Ohio, USA kquinn@bgsu.edu

Abstract

This paper challenges the commonly held view that Smith's moral theory is a subjectivist theory. Smith's test for goodness and rightness – for propriety – is not the approbation of an impartial spectator, but the *warranted* approbation of such a spectator. Something is right or good not because an impartial spectator would approve of it, but because such a spectator would be warranted in so approving.

Keywords: B12, B31

JEL codes: Adam Smith, impartial spectator

1. Introduction

This paper may appear quixotic in the extreme. Adam Smith's *Theory of The Moral Sentiments* (TMS) has generally been seen as a species of the genus of moral sentimentalism.¹ Moral sentimentalists agree in grounding our moral distinctions in our sentiments, as opposed to the world. They are in this respect the progenitors of various stripes of subjectivism in meta-ethics. I want to argue that Smith does not necessarily fit this picture. I think he can be easily read to do so, and that he was sometimes confused about what he was doing, but that we ought to, at a minimum, recognise an alternative, objectivist (and therefore, I think, correct) strain in Smith, in tension with his apparent subjectivism.²

Parfit (2011, pp. 378-80) identifies two variants of what he calls Moral Sentimentalism. An exponent might argue either that moral judgements make no claims at all - that they are disguised *expressions* of how we feel or what we approve of 3 - or that they are claims about what we either do feel or would feel under certain ideal conditions, not claims about the way things are. So 'This is good,' for the for the first type of subjectivist, may be

¹ In the Stanford Encyclopedia of Philosophy, e.g., the first thinkers cited in the article 'Moral Sentimentalism', are Smith and Hume.

² At the outset, I should say that I do not attempt to make a case-closed argument for Smith's objectivism; instead, I argue that the case for his subjectivism is *not* closed. That is, I take for granted the *prima facie* case for subjectivism that support the views of Griswold, Larmore (see the sequel) and many others, that this is the camp to which Smith belongs, and so play devil's advocate, trying to complicate the dominant picture. Further, one goal of the paper is to point out resources for modern antisubjectivism in a thinker who is at first sight not a very likely source. I know it is out of fashion to use the history of thought in this way- treating thinkers from the past as if they were co-participants in dialogue with contemporary thinkers concerning common, as it were perennial, concerns and questions. Instead we are to contextualise (that is Moses and all the prophets!) – so that the default position is that we share no questions in common with the inhabitants of that other country, the past. I think there is room for both approaches; the one which makes the apparently familiar strange, and the other, which makes the apparently strange familiar. It is true that the approach employed here makes the history of thought, in a sense, instrumental to (but also, I think, crucial to the advancement of) first-order ethical and meta-ethical thought.

³ This is Moral Expressivism, for which the canonical reference is C. L. Stevenson (1944).

analysed as something like 'Hurray for this!',⁴ while for the second type it would be read as 'I approve of this,' or 'We would approve of this,' or 'I would approve of this if I knew more'. It is either an expression of approval or a statement of the fact that we approve. In either case, the statement is not to be interpreted in a common sense way; that is a claim about how the world is or a claim that 'this' has the non-natural property of 'goodness'. Common sense takes in this respect an objectivist meta-ethical position. For common sense, the subjectivist has things backward: we approve of what is good, because it is good; our approval doesn't make it good. Goodness pre-exists and calls forth our approval.⁵

The philosophers Charles Larmore and Derek Parfit have both argued for objectivism about norms generally, with moral norms a special case. Norms, in this usage, give us reasons, whether they are moral norms, practical norms, or epistemic norms: they state what we *ought to do* or believe, where the fact that we ought to do x or believe y is a non-natural fact about the world, something we discover.⁶ Modern economics has given almost unquestioned allegiance to a subjectivism about practical norms, by identifying reasons with desires or preferences: that you 'ought to do x' means, in the subjectivist account, that you have some preferences which are best served by your x-ing. Subjectivist philosophers analyse 'you have a reason to x' as 'you either want x or would want x if fully informed' (Parfit, 2011, p. 269). For an objectivist about norms, on the other hand, what we have reason to do, what we ought to do is objective (as Larmore (1996) emphasises, we 'find' that we have a reason to do x) and this reason needn't necessarily coincide with what would fulfil either our actual or fully-informed desires.

On the evidence of the Theory of Moral Sentiments, I believe Smith was neither a moral sentimentalist, in either variant identified by Parfit, nor a subjectivist about reasons. Prominent philosophers on both sides of the subjective / objective divide disagree.

2. Smith's Moral Objectivism

Take first Charles Griswold (1999), whose *Adam Smith and the Virtues of Enlightenment* marked the beginning of a new interest in Smith among moral philosophers, for whom he had hitherto been not much more than a footnote to Hume, if noticed at all. Commenting on Smith's notion of the Impartial Spectator, perhaps the lynchpin of TMS, he writes:

'Since the agent's disapprobation of self or other must be reached from the standpoint of a spectator, moral judgements cannot be simply expressions of our own emotion. Smith's theory of moral sentiments is not emotivist in a narrow sense of the term ... it's a sophisticated emotivism according to which the emotions that the judgement of an informed and judicious spectator finds warranted (or appropriate, suitable, fitting) are moral' (Griswold, 1999, pp. 129-30).

⁴ Parfit calls Expressivism the 'Boo-Hurray Theory'!

⁵ The great musical philosophers Rodgers and Hammerstein ask, in a famous lyric, 'Do I love you because you're beautiful, or are you beautiful because I love you'. If the former, we have objectivism about beauty; if the latter we have subjectivism. ⁶ For Objectivists like Larmore and Parfit, it is a *non-natural* fact. There are objectivists who make norms

⁶ For Objectivists like Larmore and Parfit, it is a *non-natural* fact. There are objectivists who make norms natural facts. The work of Larmore, in *The Morals of Modernity* and *The Autonomy of Morality*, Parfit, in *On What Matters*, years in the making and his magnum opus, the late Jean Hampton, in *The Authority of Reason*, and *Charles Taylor*, in his *Sources of The Self* – all woke me from my dogmatic subjectivist slumber, as it were! The case they collectively make for: objectivity, the reality of reasons, irreducibly normative entities – however shocking it may be to a scientistic age – is, for me, overwhelming and profound.

And:

'Smith's account of morality in terms of emotions is subjectivist, in the sense that the meaning of moral terms is determined by what pleases or displeases the impartial spectator and not by some altogether external stimulus or state of affairs' (Griswold, 1999, p. 158).

Griswold sees Smith as a sophisticated emotivist, a nuanced subjectivist, and he is friendly to such a position. Reviewing Griswold's book, in The *New Republic*, Charles Larmore, who is a full-throated moral realist, and so opposed to subjectivism, nevertheless agrees with Larmore that this was indeed Smith's position:

'For Smith, morality is a point of view that we develop more as members of society than as separate individuals. In large part, conscience amounts to society within' (Larmore, 1999, p. 45).

And:

For Smith, the proper standards of moral judgement are simply the ones we imagine an impartial spectator using, not the ones an impartial spectator would be well-equipped to discover. In ordinary life people may believe that our moral judgements answer to the way things are, morally speaking. But in this belief, Smith only saw, as Hume had put it, 'the mind's great propensity to spread itself on external objects' (Larmore, 1999, p. 45).

And he contrasts an understanding of the impartial spectator as 'someone well-placed to discern the correct principles of morality' with Smith's view that he is 'the very author of their validity'.⁷

In this last distinction, we recognise the Rodgers-and Hammerstein question: does an impartial spectator approve of this because it is good, or is it good because an impartial spectator approves of it?⁸ I think Smith, on balance and *pace* Larmore and Griswold, gives the former, objectivist, answer to the question. He writes:

'Whatever judgement we can form {concerning our own sentiments and motives} must always bear some secret reference, either to what are, or to what, upon a certain condition would be, or to what, we imagine, *ought to be* the judgement of others. We endeavour to examine our own conduct as we imagine any other fair and impartial spectator would examine it (Smith, 1976, p. 110; emphasis added).⁹

⁷ More recently, see Rasmussen: 'Smith's fundamental claim in TMS is that the impartial spectator sets the ultimate standard for moral judgement: sentiments, qualities and actions that earn such a spectator's sympathy or approval are morally right, and those that earn his disapproval or resentment are morally wrong' (Rasmussen, 2014, p. 49).
⁸ It is also the question raised by voluntarist theologians, who claimed that 'God commands the good'

⁸ It is also the question raised by voluntarist theologians, who claimed that 'God commands the good' should be read not, as their opponents held, as constraining God to command what can be independently defined as good, but as *defining* the good as 'that which God commands'. Canonically, this is the problem first raised by Plato in his dialogue *Euthyphro*: 'the point which I first wish to understand is whether the pious or holy is beloved by the gods because it is holy, or holy because it is beloved of the gods' (Plato, 1937, p. 391). ⁹ David Andrews argues that I load the deck against subjectivism by ignoring the emphasis on the

⁹ David Andrews argues that I load the deck against subjectivism by ignoring the emphasis on the imagination in this passage. Weinstein similarly writes: 'Ultimately, the imaginary nature of the impartial spectator ends up limiting its detachment. Because it is imagined by an imperfect person, it is only as objective as its imaginer' (Weinstein, 2016, p. 352). In my view, we have to imagine, first, how we would evaluate our own action if we were, counter-factually, a spectator and not the actor, and secondly, we may have doubts about the correct standards to apply, so that we must imagine what an ethically impeccable spectator – which none of us are- would say about us. Weinstein goes on to contrast his

Any subjectivist reading of this passage is brought up short – very short! – by the 'ought to be' in this passage. For subjectivists, in this context, the idea is to analyse 'what we ought to do' as 'what others in fact judge, or would judge with full information, to be right'. If we make the test what others 'ought to' judge, we are arguing in a circle. Compare 'It is good because an impartial spectator approves' with 'It is good because an impartial spectator *correctly* approves'. The latter formulation quite obviously fails to reduce 'the good' to a natural fact about us – to our approving it – because the criteria of *correct* approval appeal to standards independent of our approval.

Look again at the quote from Griswold above. For Smith, he says, 'the emotions that the judgement of an informed and judicious spectator finds warranted (or appropriate, suitable, fitting) are moral'. If this is what Smith says, he certainly is a subjectivist. But in contexts like this, as in the quoted material above, Smith would add another 'warrant' here; so that it would be not what the spectator *finds* warranted, but what the spectator *would be warranted in finding* warranted, that marks the moral. And this would spoil the subjectivism.

Smith fleshes out his notion of the connection between morality and the Impartial Spectator in ways which support an objectivist reading, I think. In Part 3, Chapter 2, 'Of the Love of Praise, and of that of Praise-worthiness; and of the Dread of Blame, and of that of Blame-worthiness', we see how the impartial spectator is the vehicle that takes us from the first to the second of each of these pairs. Examining our own conduct, morality involves imagining what a spectator would be warranted in approving, not what he would in fact approve. Doing what is praise-worthy, what a spectator would be warranted in praising, is doing one's duty. To seek praise independent of praise- worthiness is vanity:

'Praise and blame express what actually are; praise-worthiness and blameworthiness what *naturally ought to be* the sentiments of other people with regard to our character and conduct. The love of praise is the desire of obtaining the favourable sentiments of our brethren. The love of praiseworthiness is the desire of rendering ourselves the *proper objects* of those sentiments' (Smith, 1976, p. 126, emphasis added).

There is nothing in the least subjectivist about this. A subjectivist would be unable to mark this distinction, would reduce praise-worthiness to praise, and virtuous behaviour to vanity. For Smith, this is what 'the licentious philosophy' of Mandeville does, and this is what makes it licentious.

Immediately preceding the chapter under discussion, Smith remarks:

'To be amiable and to be meritorious; that is, to deserve love and to deserve reward, are the great characters of virtue; and to be odious and punishable,

subjectivist reading with constructivist views such as Roderick Firth's 'ideal observer' theory of the impartial spectator. I agree that Smith is not a constructivist. I read Smith as a 'robust ethical realist' in the sense specified by Fitzpatrick (2008), for whom 'Ethical standards are independent of us in the sense that they are not constituted by the actual or hypothetical results of any *ethically-neutrally-specifiable* set of conditions or procedures applied to our beliefs, desires, attitudes, etc.' (Kindle LOC 2096, emphasis added). So constructivism is, in this view, a non- realist, and thus a *subjectivist* position. Fitzgerald goes on to specify the Aristotelian lineage of this robust realist view, since it implies that 'there is no way to characterise the route to ethical truth except from within a correct ethical perspective' (Kindle LOC 2123). Such a position, were it Smith's, would seem to satisfy Weinstein's concern to differentiate Smith's view from Ideal Observer theory and its ilk, but it would not make Smith's theory subjectivist, since, as Fitzgerald goes on to argue, 'there is no suggestion that what *makes* ethical claims true is that they would be endorsed by people deliberating from certain standpoints. In fact that couldn't be so on this view, since among the true ethical claims are claims about what those proper starting points are.'

of vice. But these characters have an immediate reference to the sentiments of others' (Smith, 1976, p. 113).

In light of what follows, it would be a mistake to give the last sentence a subjectivist import, to make the sentiment of others constitutive of amiability and merit. What the sequel supports, rather, is the idea that it is essential to goodness that the sentiments of *good* people (people able to deploy the correct criteria of goodness) would approve of it. The 'characters' have an immediate reference, that is, not to the sentiments, but to the *warranted* sentiments of others, or so it will turn out.¹⁰

Now, to say that warranted sentiments are not reducible, in Smith, to actual sentiments, and that moral sentiments are the former rather than the latter, is not to deny any empirical connection between the two. Albeit very tenuously, and in some cases even inversely, praise can track praise- worthiness. Your conviction that you are acting in praise-worthy ways may be and likely will be biased in your favour, and the absence of any actual praise, coupled with the presence of clamorous actual blame, may lead you to re-evaluate yourself, coming closer to the truth. Thus:

'The agreement or disagreement both of the sentiments and judgements of other people is of more or less importance to us, exactly in proportion as we are more or less uncertain about the propriety of our own sentiments, about the accuracy of our own judgement' (Smith, 1976, p. 122).

In addition, a big theme, arguably the biggest, in TMS is the pilgrim's progress, by dint of his essential sociality, from the natural to the normative. By taking account of what other people think of us, we may learn better who we really are and become better people.

Smith gives these ideas a theological cast in the closing pages of Chapter 2. He tells us that our concern for the actual sentiments of others has been implanted in us by 'the all-wise Author of Nature':

'He has made man, if I may say so, the immediate judge of mankind; and has, in this respect, as in many others created him after his own image, and appointed him his vice-regent upon earth, to superintend the behaviour of his brethren But though man has in this manner been rendered the immediate judge of mankind, he has been rendered so only in the first instance; and an appeal lies from his sentence to a higher tribunal, to the tribunal of their own consciences, to that of the supposed impartial and well-informed spectator, to that of the man within the breast, the great judge and arbiter of their conduct' (Smith, 1976, pp. 129-30).

Following Smith's metaphor, a subjectivist would make the decisions of the lower courts- the actual sentiments and judgements of mankind- constitutive of the law. Smith instead invokes the higher tribunal of the man within – our own conscience, and *identifies* the impartial spectator with that tribunal. Since the conscience is the locus of moral judgement, this supports my view that the impartial spectator approves what is good rather than constituting the good by his or her approval.

¹⁰ This reading would make Smith a precursor of Franz Brentano, who held, according to Elizabeth Anderson (1993, p.5), that an object is good if and only if it is correct to love it, and bad if and only if it is correct to hate it. She is herself in this tradition, 'in identifying what is good with the proper objects of positive valuation' (*Ibid*).

Note, as well, that this passage patently contradicts Larmore's claim, cited above, that for Smith, conscience is 'society within'. Just beyond the cited passage, glossing it, Smith identifies society, the immediate judge, with 'the man without', by contrast with conscience, the man within. They are clearly distinct.

For Smith then, though distinct, praise and praise-worthiness are not unrelated: the lower courts can faithfully apply the law. On the other hand, praise can sometimes track praise-worthiness, as I said, inversely. This is what Smith calls 'the corruption of our moral sentiments, which is occasioned by this disposition to admire the rich and the great, and to despise or neglect persons of poor and mean condition' (Smith, 1976, p. 61). This is the title of Part 1, Section 3, Chapter 3:

'That wealth and greatness are often regarded with the respect and admiration which are due only to wisdom and virtue; and that the contempt, of which vice and folly are the only proper objects, is often most unjustly bestowed on poverty and weakness, has been the complaint of moralists in all ages' (Smith, 1976, p. 61)

Notice that a moral sentimentalism in the traditional sense could find no meaning to the notion that our moral sentiments are 'corrupt'. If morality is essentially a matter of sentiments, however sophisticated, how can they fail to track the good!

This completes my case that Smith can be seen as a Moral Objectivist, appearances to the contrary notwithstanding. But those appearances haunt me. There is no doubt that Smith's project breathes a subjectivist air, as it were. And he always seems on the verge of subjectivism, with some normative qualification – 'warranted', 'proper' – pulling him back from the brink, while making him sound puzzlingly circular. Why?

This is my tentative suggestion. David Hume is seen by most people as an out-andout subjectivist, with respect to both moral and practical reasons generally. Hume was an intellectual giant. Hume was Smith's friend and they were mutual admirers of each other's work.¹¹ But I don't think Smith would have considered himself Hume's equal in moral philosophy, just as I don't think Hume would have considered himself to be Smith's equal in political economy.

Hume's subjectivism was new and it was radical. Smith, while to some extent deferring to Hume in matters moral, may have nevertheless profoundly disagreed with Hume on the issue of how thorough-going subjectivism could be. Might Smith not have taken the new Humean subjectivist bottles and filled them with old, objectivist, common-sense wine? Is this why the ghosts of subjectivism surrounding TMS are so hard to bust?

3. Utility, Propriety and Love of System

Smith, I have argued, was no expressivist, not even a collective expressivist. The criterion for what is right and good is neither what we (individually) approve, nor is it what an impartial spectator would approve, but rather is what an impartial spectator *ought to* approve. I am arguing that Smith's *meta-ethics* were not, notwithstanding appearances to the contrary, subjectivist.

¹¹ For a moving account of the deep friendship between the two, see Rasmussen (2017).

But among meta-ethical objectivists we may distinguish more or less *substantively* subjectivist¹² positions. On the subjectivist extreme, a utilitarian, for example, believes that the property that makes something objectively good is that it maximizes the utility or preference satisfaction of the greatest number. The ability to satisfy preferences or desires is the exclusive moral-reason-giving property. (Notice that at the level of the Collective, as opposed to the individual, this substantive subjectivism is very hard to distinguish from meta-ethical subjectivism - what is good is what We desire, albeit not necessarily what you or I desire. 'We desire what is good' becomes tautological – the hall-mark of meta-ethical subjectivism.)

In what follows I want to argue that Smith was not a substantive subjectivist, either. But I find his objectivism much less full-throated than it could be, due, I think, to the deference he pays to Hume. The best place to see this tension at work, I think, is the short part 4 of *TMS*, called *Of the Effect of Utility Upon the Sentiment of Approbation*, containing just two chapters, 'Of the beauty which the appearance of utility bestows upon all productions of art, and of the extensive influence of this species of beauty' and 'Of the beauty which the appearance of utility bestows upon the characters and actions of men, and how far the perception of this beauty may be regarded as one of the original principles of approbation.'

This short section contains several of the most widely-quoted passages in TMS (including the single use of the phrase 'the invisible hand' in the entire volume). And the whole of the two chapters is a running argument with Hume: the first chapter quarrels with Hume's contention, in Smith's words, that 'the utility of any object pleases the master by perpetually suggesting to him the pleasure or conveniency which it is fitted to promote' (Smith, 1976, p. 179) and the second with Hume's attempt, as Smith thinks, 'to resolve our whole approbation of virtue into a perception of this species of beauty which results from the appearance of utility' (Smith, 1976, p. 188).

Smith will argue, in the first chapter, that objects which provide utility are appreciated as much for the art and contrivance they display – or, out of what Smith calls our 'love of system' – as for the utility they provide. Similarly, in Chapter 2, concerning the evaluation of human character, he argues that 'the sentiment of approbation always involves in it a sense of propriety quite distinct from the perception of utility'.

Now the particular positions of Hume that Smith is criticising here are not a minor part of the Humean legacy. Indeed, they form the basis for the common view of Hume as a protoutilitarian. Why, then, is Smith often lumped with Hume in precisely this respect – as a forerunner of utilitarianism? I think it is because Smith's criticism takes place in the context of what may appear to be a fundamental agreement with Hume, that utility is the sole criterion of objective value. What he appears to argue is that while our moral sense is guided by our sense of propriety – which is of course tied for Smith to the (warranted) views of an impartial spectator, that either:

- i. Our sense of propriety in fact tracks what is useful (the Author of Nature has so seen to it); or, where it does not that
- ii. Our moral sentiments can be mistaken, just insofar as they fail to track utility'.

¹² I am using term 'substantively subjectivist' differently from Parfit in *On What Matters*. He uses it in the context of his examinations of normative reasons generally (not moral reasons alone) to refer to the view that we have objective reason to do whatever best satisfies our desires (the latter usually filtered in some way - e.g. what we would desire in 'ideal' conditions). This counts as substantive subjectivism as a view of moral reasons as I am using the term, too, but so does utiltarianism (which Parfit holds to be true in important respects and not substantively subjectivist, since it doesn't make exclusive pursuit of *the agent's* desires to be what we have most reason to do).

The underlying agreement that what is in fact good is what is useful would in that case make Smith's comments a fundamentally friendly emendation to Hume's proto-utilitarian position. But I don't think all of what Smith says in these chapters can be squared with either i. or ii. There is a residual that speaks to a reluctance on Smith's part to accept the reduction of all values to utility, I believe.

In Chapter 2, there is a clear statement of (i.). Concerning Hume's view that 'no qualities of the mind... are approved as virtuous, but such as are useful or agreeable to the person himself or to others', Smith says:

'Nature herself seems to have so happily adjusted our sentiments of approbation and disapprobation, to the conveniency of both the individual and of society that ... I believe this is universally the case' (Smith, 1976, p. 188).

But, he also says, 'the view of this utility or hurtfulness is not the first or principal source of our approbation and disapprobation', and that these sentiments are 'originally and essentially different from this perception'. Tellingly, he says:

'It seems impossible that the approbation of virtue should be a sentiment of the same kind with that by which we approve of a convenient and well-contrived building; or that we should have no other reason for praising a man than that for which commend a chest of drawers' (Smith, 1976, p. 188).

He does not say, *nota bene*, that we *think we have* other reasons for praising a man than the utility he creates for himself or others: he says we in fact have other reasons.

He goes on to consider qualities which are approved as virtuous because they are useful to ourselves, reason and understanding, on the one hand, and self-command, on the other. With respect to reason, he points out that 'superior reason and understanding are originally approved of as just and right and accurate, and not merely as useful or advantageous'. Again, Smith's language betrays him: is approval of the justness and rightness of reasoning merely the way in which the underlying exclusive objective value of reasoning – its utility – appears to us, or are these in fact independent grounds of value?

Smith's position is unstable. This is because his account of propriety, as I have argued above, is based not on the brute sentiment but the *warranted judgement* of an impartial spectator. If that is so, then to establish that propriety is not transparently utilitarian in its evaluation is thereby to establish that an exclusively utilitarian account of objective value is wrong.

Finally, let me turn to the love of system, of art and contrivance, that Smith analyses in Chapter 1. He argues that we come to value the means to the end of utility for their own sake. The poor man's son, famously in Smith's telling, who spends his life striving for, and finally achieves, wealth and greatness, is no happier and no more secure that he would have been had he remained poor. The trappings of wealth and greatness are merely elaborate contrivances.

Here we have something that seems to fit (ii): the value we place on art and contrivance, our love of system apart from any utility it may bring, is a mistake. Concerning the 'beauty of that accommodation which reigns in the palaces and economy of the great':

'If we consider the real satisfaction which these things are capable of according, by itself and separated from the beauty of that arrangement which is fitted to promote it, it will always appear in the highest degree contemptible and trifling. But we rarely view it in this abstract and philosophical light. We naturally confound it in our imagination with the order, the regular and harmonious movement of the system, the machine or economy by means of which it is produced. The pleasures of wealth and greatness, when considered in this complex view, strike imagination as something grand and beautiful and noble, of which the attainment is well worth the toil and anxiety we are so apt to bestow upon it (Smith, 1976, p. 183).

What does Smith conclude from this? Having diagnosed, like a modern behavioural economist, a serious failure in our ability to choose in utility-maximising ways, does he suggest a fix, a nudge? On the contrary:

It is well that nature imposes on us in this manner. It is this deception which rouses and keeps in continual motion the industry of mankind. It is this which first prompted them to cultivate the ground, to build houses, to found cities and commonwealths, and to invent and improve all the sciences and arts, which ennoble and embellish human life; which have entirely changed the whole face of the globe, have turned the rude forest of nature into agreeable and fertile plain, and made the trackless and barren ocean a new fund of subsistence, and the great high road of communication to the different nations of the earth (Smith, 1976, p. 183).

It is well! We have just been told that the strivers for wealth and greatness, at a great cost in anxiety and toil, find themselves no happier. Is there an argument that the unintended consequences Smith describes here so eloquently produce enough happiness to offset the cost? Not at all: what he lists here are all 'contrivances', means to happiness at most. I submit that they are valuable in themselves, apart from any happiness they might bring, and that this is the best way to make sense of Smith here. Science and Art *ennoble and embellish* human life – whatever they do for happiness. The founding of great cities and commonwealths, the creation of a civilisation is intrinsically valuable, whatever the consequences for happiness. Despite his deference to Hume, I think, Smith's fundamental differences with both Hume and the modern economist, for whom, like Hume, preference satisfaction is all there is, are undeniable.

Acknowledgements

The author wishes to thank Michel Zouboulakis, David Andrews and Thomas Scheiding, who all reviewed the paper on the Open Peer Discussion Forum. I hope that they will find the revised paper improved as a result.

References

Anderson, Elizabeth (1993) *Value in Ethics and Economics*. Cambridge, MA: Harvard University Press.

Fitzpatrick, William (2008) 'Robust Ethical Realism, Non-Naturalism and Normativity.' In *Oxford Studies in Metaethics, Vol.3*, Russ Shafer-Landau (Ed.) New York: Oxford University Press.

Griswold, Charles (1999) *Adam Smith and the Virtues of Enlightenment.* Cambridge: Cambridge University Press.

Hampton, Jean (1998) The Authority of Reason. Cambridge: Cambridge University Press.

Larmore, Charles (1999) 'The Visible Hand.' The New Republic, 18 October.

Larmore, Charles (1998) The Autonomy of Morality. Cambridge: Cambridge University Press.

Parfit, Derek (2011) On What Matters. Oxford: Oxford University Press.

Plato (1937) *Euthyphro*, in *The Dialogues of Plato, Volume 1*. B, Jowett, (Transl.). New York: Random House.

Rasmussen, Dennis (2014) *The Pragmatic Enlightenment*. Cambridge: Cambridge University Press.

Rasmussen, Dennis (2017) *The Infidel and The Professor*. New Jersey: Princeton University Press.

Smith, Adam (1976 [1759]) The Theory of Moral Sentiments. Oxford: Oxford University Press.

The Stanford Encyclopedia of Philosophy, https://plato.stanford.edu/

Stevenson, C.L. (1944) Ethics and Language. New Haven: Yale University Press.

Taylor, Charles (1992) Sources of the Self. Cambridge, MA: Harvard University Press.

Weinstein, Jack Russell (2016) 'My Understanding of Adam Smith's Impartial Spectator.' *Economic Journal Watch* 13(2), May 2016, pp. 351-358.

SUGGESTED CITATION:

Quinn, Kevin (2019) 'Was Smith A Moral Subjectivist?' *Economic Thought*, 8.1, pp. 30-39. http://www.worldeconomicsassociation.org/files/journals/economicthought/WEA-ET-8-1-Quinn.pdf

Addressing the Malaise in Neoclassical Economics: a Call for Partial Models

Ron Wallace, University of Central Florida, Orlando, USA Ronald.Wallace@ucf.edu

Abstract

Economics is currently experiencing a climate of uncertainty regarding the soundness of its theoretical framework and even its status as a science. Much of the criticism is within the discipline, and emphasises the alleged failure of the neoclassical viewpoint. This article proposes the deployment of partial modelling, utilising Boolean networks (BNs), as an inductive discovery procedure for the development of economic theory. The method is presented in detail and then linked to the Semantic View of Theories (SVT), closely identified with Bas van Fraassen and Patrick Suppes, in which models are construed as mediators creatively negotiating between theory and reality. It is suggested that this approach may be appropriate for economics and, by implication, for any science in which there is no consensus theory, and a wide range of viewpoints compete for acceptance.

Keywords: economic models, history of economic theory, complexity economics, computational modelling, Boolean networks, semantic view of theories

Abbreviations: BN: Boolean Network; CNA: CellNetAnalyzer; GET: General Equilibrium Theory; HFT: High-frequency trading; ODE: ordinary differential equation; SQUAD: Standard Qualitative Dynamical System; SVT: Semantic View of Theories

JEL Classification Codes: B40, B50, B59

1. Introduction

Joseph Stiglitz, recalling his chairmanship of the Council of Economic Advisers (1995-1997), noted that one of his major problems was hiring a macroeconomist. As he recalled it:

'The prevailing models taught in most graduate schools were based on neoclassical economics. I wondered how the president, who had been elected on a platform of "Jobs! Jobs! Jobs!" would respond to one of our brightest and best young economists as he or she explained that there was no such thing as unemployment' (Stiglitz, 2010, p. 350, note 14).

Like most satirical observations, this one contains (at least) a grain of truth. Of course neoclassical economists are aware that unemployment is real. But the target of Stiglitz' barb is the idealised neoclassical assumption of full employment of labour and other resources. This stipulation, together with other similarly unrealistic assumptions – e.g., perfect competition, fixed consumer income, perfect mobility of factors of production, as well as several others – comprise the foundation of Léon Walras' (1834-1910) General Equilibrium

Theory (GET): a critical component of the neoclassical framework and of mainstream economics (Turk, 2012). While any scientific theory is to some extent an abstraction, critics of GET maintain that the present form of this model is a purely mathematical achievement with remarkable internal consistency but total irrelevance to economic life (Ackerman, 2002). Criticism of GET, of its larger neoclassical context, and indeed of the entire science, has dramatically escalated - helped by a strong assist from the blogosphere (The Economist, December 28, 2011) - following the 2008 market collapse. Failure to predict the crisis, or to expeditiously cure it, has suggested that GET - and, in the bargain - all of economics, was hopelessly out of touch with reality. 'The economist has no clothes,' as one critic observed (Nadeau, 2008). (Yet, and importantly, it was probably never that simple. Historically, as The Economist (April 12, 2014) noted, economic slumps have generated emergency models cobbled together at the bottom of financial cliffs. Often what starts out as a post-crisis sticking plaster becomes a permanent feature of the system. If history is any guide, decisions taken now will reverberate for decades.') Perhaps most emblematic of the deepening self-critical mood was a February 9, 2015 New York Times colloquium of American economists which addressed 'the profession's poor track record in forecasting and planning, and the continued struggles of many Americans'."

This article is not the addition of one more voice – that of an anthropologist – to the growing heterodox chorus calling for an end to the neoclassical view. Nor is it a retrenched attempt to defend that orthodoxy in the face of its historical record. Instead, the article proposes an inductive, exploratory approach in which partial models of an economic system i.e., 'models that are at the same level of abstraction and represent different "views" of a phenomenon' (Amigoni and Schiaffonati, 2008) - are deployed in a computational strategy in which components of the models are combined in unpredictable ways. A theory is thus a synthesis of input models, and should be tested for its ability to predict an actual economy. Models, in this approach, are thus exploratory devices and clearly differ from theories. In accordance with the Semantic View of Theories (SVT) pioneered by Bas van Fraassen and Patrick Suppes, and subsequently developed by Margaret Morrison, Mary S. Morgan, Francesco Amigoni and Viola Schiaffonati, we would designate models as cognitive tools or, equivalently, as conceptual instruments, that 'mediate' between the referent (reality) and the synthetic interpretation, or theory. The strategy is illustrated through a Boolean Networks (BN) model originally utilised in cell biology. BNs are a class of computational models primarily distinguished by discretized variables (nodes) for which input-output relations are governed by Boolean functions (Helikar et al., 2011). (Alternative strategies, including hybridisation with agent-based models or ABMs, are certainly possible. New approaches are being developed all the time. The BN method was chosen because of its relative simplicity and demonstrated accuracy in partial modelling of complex systems).

In the following section, each major step of BN partial modelling is explained in (mostly) nonmathematical detail, and sample economic implications are embedded within the discussion. Emphasis is placed on two key properties: the use of modelling conventions or standards when partial models are combined; the ability of the method to incorporate externalities, such as cultural or religious variables, for which quantitative data are frequently inadequate or lacking. The BN method is then examined in the larger context of SVT. Here, the autonomy of the partial modelling procedure, in which the modeller cannot predict what the results will be, is construed as a computational variant of Morrison and Morgan's approach. As a programmatic example, partial modelling is proposed for recent theoretical controversies related to high-frequency trading (HFT). It is concluded that partial modelling is appropriate for economics – and by extension, for any science – in which the traditional

framework has failed, there is no consensus theory, and an array of alternative viewpoints compete for recognition.

2. Addressing the Climate of Uncertainty in Economic Theory: Partial Modelling with Boolean Networks

Boolean networks (BNs) began as the almost inevitable outgrowth of the Digital Revolution which swept the behavioural, social and biological sciences in the late 50s and early 60s. Computational analogies abounded, ultimately reaching their limit when Vladimir Brix announced that 'you are a computer' (Brix, 1970). The initial approaches were homeostatic: the new discipline of cybernetics, as described by mathematician Norbert Wiener (1894-1964) and psychiatrist W. Ross Ashby (1903-1972) comprised living and mechanical systems in which output was sensed, compared with a goal, and the discrepancy was reduced, generating a stable state. In a major theoretical shift, the approach was extended by Magoroh Maruyama (1963) who proclaimed a 'second cybernetics': deviations need not result in correction and continued stability, but may in fact 'amplify' and generate widespread systemic change. Influenced by these currents, economist Herbert Simon anticipated BNs in his 'satisficing' concept of the economic actor (Simon, 1947). Contrasting sharply with the optimising agents of neoclassical theory - firms which maximise profits from production and distribution, households which maximise utility, or satisfaction, from consumption - satisficing (satisfy and suffice) posited an 'aspiration level' or acceptability threshold as a tractable heuristic for decision-making behaviour.

Herbert Simon's two seminal concepts – binary variables and thresholds – were later incorporated into a fully realised BN model by theoretical biologist Stuart A. Kauffman (1969). Its basic properties were, and are, comparatively simple. Following Helikar et al. (2011), a BN is a discrete model comprised of a set of components or nodes $\{\sigma_1, \sigma_2, ..., \sigma_n\}$ which can typically assume only two values, ON (1) or OFF (0); these correspond, respectively, to the active or inactive state of the variable, or to its above- or below-threshold value. Nodes are linked by a 'wiring diagram' formulated by the investigator in a first approximation. The diagram may be – and often is – somewhat speculative, especially if the variables are not yet well-understood (Davidich and Bornholdt, 2008; Helikar et al., 2011). Finally, the binary output of each node is specified by logical operations utilising AND, OR, and NOT; the input-output relations, or Boolean functions $\{B_1, B_2, ..., B_n\}$, are represented in a 'truth table'. The model is thus algebraic (although its discrete values, 0 and 1, may be regarded as the limits of continuous functions, and in fact, hybrid variants utilising ordinary differential equations, or ODEs, continue to be developed). In Kauffman's summary:

'The dynamic behavior of each variable – that is, whether it will be on or off at the next moment – is governed by a logical switching rule called a Boolean function. The function specifies the activity of a variable in response to all the possible combinations of activities in the input variables. One such rule is the Boolean OR function, which says that a variable will be active if any of its input variables is active. Alternatively, the AND function declares that a variable will become active only if all its inputs are currently active' (Kauffman, 1991, p. 77).

Under the best of conditions – i.e., when educated guesswork is minimal – the BN approach has proven to be a valuable approximation technique. BNs, and their many variants, have

been used in a wide, and expanding, range of modelling applications, 'including gene regulatory systems, spin glasses, evolution, social sciences, the stock market, circuit theory and computer science' (Richardson, 2005, p. 365), frequently yielding results with high predictive power.

Partial modelling utilizing BNs has recently been applied to a sample problem in computational biology (Schlatter et al., 2012). Alternative BN models of liver-cell (hepatocyte) interaction were combined into a larger network representation. As a prerequisite for smooth model integration, the investigators proposed a set of standards or conventions, some of which were highly unrealistic: the ON (1) state of a network molecule may be discretised as multi-valued logic to represent varying concentrations, e.g. high, low, very low, but only if the variations have a functional effect; quantitative experimental data are to be utilised in configuring node interactions; the treatment of time is made somewhat artificial in that the value assigned to a node is based on the peak concentration of the referent molecule at any time point in the signalling process; artificial nodes which do not correspond to any molecular species sum up the network response to selected input nodes with regard to a cellular function of interest (e.g., the effect of the inputs on apoptosis, or cell death), thus constituting a form of early automated analysis; certain input nodes corresponding to molecules which are constitutively active (i.e., operative in the cell under all physiological conditions) are initialised at the ON (1) state; finally, and most importantly for uncertain modelling situations, artificial nodes are used to 'model unknown interrelations'. In the liver-cell example, cells switch between two different forms of apoptosis, but the underlying protein mechanism, which has not been identified, was modelled by an artificial node.

Using those conventions, the study demonstrated the coupling of two BN models in two different biological examples: in the first example, BN models of two different cell types were combined; the second example combined partial models of a single cell type. The accuracy of the first example was experimentally verified, and then used as a basis for evaluating the second (partial) modelling approach. In the first example, SQUAD (Standard Qualitative Dynamical Systems) was utilised: this is a hybrid modelling approach - i.e., one which synthesises discrete and continuous methods - which initially configures a target network as a discrete dynamical system (e.g., a BN), and then applies a binary decision algorithm to identify all its steady states (DiCara et al., 2007). SQUAD simulation essentially consists of three main stages. First, the network is described by a graph or wiring diagram which is then converted into a BN. Through the use of a BN algorithm, all the steady states of the system are identified. Second, through the application of a toolbox it is possible to convert a BN into a continuous dynamical system configured as ordinary differential equations (ODEs); this transform permits the modeller to identify the steady states of the newlydeveloped continuous model via reference to the preceding BN. Metaphorically, one might think of the steady states of the initial BN as mathematically 'visible beneath' the continuous model. Finally, dynamic simulation methods, especially perturbation techniques, reveal the overall behaviour of the network and the roles of specific nodes. (The perturbations can be sensitively configured; for example, singlepulse can modify a node at a single time point; rangepulse can sustain a perturbation for some specified time interval.) In this manner, SQUAD makes possible the simulation of large signalling or regulatory networks through identification and perturbation of multiple stable states. Importantly, SQUAD does not provide information regarding the states that can arrive at any given stable state; i.e., it is uninformative regarding basins of attraction.

For comparison with the experimentally-supported SQUAD results, partial BN models of the molecular network of a single cell type were combined using CellNetAnalyzer (CNA), a Matlab toolbox for BN analysis. A key property of this approach is the simplification of the partial models to avoid an intractable result when they are combined. Thus CNA, given the standards described above, computes node values that approach a unique steady state. This is done by excluding node values that will produce multiple steady states. In addition, feedback loops are excluded because they can frequently yield oscillations. Through the use of these, and additional, simplifying procedures, the partial BNs were then combined. Initially, the partial models were pooled in common model files, and modeller decisions were made regarding the interactions of common nodes; an automated 'quality assurance method' evaluated all possible input node values consistent with the modelling standards. Results of the two approaches were very similar, and the combination of partial models 'was achieved without fundamental adjustments and the complexity was only moderately increased' (Schlatter et al., 2012).

The study has possible direct implications for partial modelling in economics. Two aspects deserve closer attention: the use of simplifying assumptions, i.e. modelling conventions or standards, when combining partial models; the ability of BNs to include system components (e.g. cultural or religious variables) for which quantitative data are minimal or lacking, without significant loss of predictive power. As an example of the first property, in BN models of the global economy, initialisation of nodes in the ON (1) state could apply to 'antimonopoly' laws enforced by China against US firms doing business in that country which require 'merger reviews and investigations of alleged anti-competitive behaviour related to pricing and monopolistic conduct' (Ong and Huber, 2014). Because these regulations, for many modelling purposes, may be considered as 'always' present, they are systemically analogous to constitutive enzymes in the Schlatter et al. study which remain active without regard to physiological conditions. Similarly, the use of artificial nodes in the study to 'model unknown interrelations' would be directly applicable to unknown components of command economies such as that of North Korea, where economic statistics are regarded as state secrets (Noland, 2012). Often, the best that one can do is utilise 'mirror statistics' - e.g. 'adding up what other countries say they import from North Korea' to estimate its exports; the results, which are almost always highly questionable, would be configured as an output from an artificial node representing unknown variables.

With regard to the second property, everyone now realises - and some have been shocked by events into realising - that cultural, religious and ideological forces, especially in the developing world, can impact the world's economies (Chua, 2002; Kaplan, 2012). Accordingly, the institutional economists Greenwod and Holt (2008) vigorously defend the extension of their science, through an interdisciplinary framework, into the realm of 'technology and its relationship to cultural habits'. Global examples of these 'cultural habits' are not difficult to find. Chua (2002) has extensively documented the widespread destructive effects of the adoption by Third World countries of democracy and free-market economies without a supportive institutional context (i.e. an established tradition of nation-state governance, socioeconomic classes and economic upward mobility). The result has been the enrichment of already-dominant minority groups including, as a major example, Chinese minorities of the Philippines, Burma, Thailand and Indonesia. Ethnic-based income disparities have culminated in violent clashes in several of these countries generating, in some cases, social collapse (e.g., Rwanda). Similarly, my student Elaine Chamberlain demonstrated that the success or failure of microfinance organisations in the Middle East and North Africa (MENA) can be significantly shaped by local cultural conditions (Chamberlain, 2015). The examples could be easily multiplied. Yet, for many of these cultural agents, quantitative data are inadequate or lacking. This limitation could be addressed through educated guesswork, as it often is with molecular systems, provided that mirror data or, even better, on-the-ground reports (e.g. from NGOs such as Human Rights Watch, http://www.hrw.org) are available. In

those cases, an increase in the cultural activity – for example, the growth of an ethnicallybased nationalistic movement – would be represented as 1; decline would be assigned 0. If the available data are somewhat fine-grained – e.g. low, moderate, high – more precise, but still qualitative, models may be developed using multi-valued logic. In this variant, a node may assume more than one value – decimal expressions from 0 to 1 – and is typically governed by a threshold rule (Schlatter et al., 2009; Bornholdt, 2008). We should also note that BNs are remarkably flexible: in the event that detailed quantitative information becomes available, either for cultural variables or other features of the model, it is possible, following Bornholdt (2008), to convert selected nodes into ordinary differential equations (ODEs).

But we must end the overview on a cautionary note: it is the very inclusiveness of the BN approach - one of its most attractive properties to those of us who view economic processes as modulated by institutional forces - that can extract a nontrivial methodological price. The addition of nodes and linkages to incorporate cultural, ideological and religious data increases the complexity and instability of the BN network, making analysis difficult. Can stable states be found? The problem, as Veliz-Cuba (2014) has noted, is NP-complete. By this is meant that the problem is a member of a larger computational problem class for which no efficient solution algorithm has been found. (A famous example is the Travelling Salesman Problem, or TSP, important in circuit design: a traveller must follow an optimal route which connects all possible destinations, without visiting any destination more than once.) Motivated by this challenge, modellers are currently designing algorithms that can identify BN stable states within a realistic time frame. For example, Velez-Cuba (2014) has proposed a method to 'reduce the network to one that has less complexity while keeping the main features; the reduced network is easier to analyse and can not only help to answer questions, but also to give insight of why such answers were obtained'. Many strategies along these lines are being developed. Moreover, the PM approach includes the CAN toolbox for pruning destabilising features in the process of model synthesis.

A related, additional challenge to the expansion of BN models is the Aggregation Problem: the difficulty of developing an empirically-valid mathematical description of a heterogenous entity (e.g., consumer demand) in a macroeconomic system, as contrasted with the considerably simpler task of describing a homogenous unit (e.g., a household) in a microeconomic system. The problem has persisted for decades, finding its most recent expression in computational modelling and the development of network theory. Thus, if a BN node is used to represent an internally complex, heterogenous entity, what binary value should it be assigned? This difficulty is not unique to economic modelling; it is frequently encountered in computational cell biology where modellers frequently deal with the problem of combining small networks into a larger network. For example, Randhawa et al. (2009) propose a 'building-block' strategy for network aggregation (based on an application to the eukarvotic cell-division cvcle) that may be relevant to economic modelling. The method emphasises components that 'have been designed for the purpose of combining them'. This approach is to be distinguished from conventional methods in which 'models are typically built from existing sub-models, and therefore contain redundancies'. In contrast, the building-block method specifies inputs and outputs ('ports') that link units (modules), containing small networks, to one another, and link each unit's ports to internal molecular species. Perhaps a variant form of this strategy would abandon the attempt to combine pre-defined economic components in favour of designing components 'for the purpose of combining them'. Addressing the Aggregation Problem (in terms of BN modelling) this would entail the designing of modules which are internally homogenous (or highly homogenous), and thus easily binarised, with specified linkages to similarly designed units. Randhawa et al. (2009) further contrast the approach with the 'error-prone' tradition of a wiring diagram. But here they paint with too broad a brush. The knowledge and informed guesswork which, as Bornholdt (2008) reminds us, are involved in the early stages of network design (and re-design) are likely not impediments, but threads of creativity – indeed of 'art or craft' (Morrison and Morgan, 1999) – which are inextricably woven into computational modelling.

3. Partial Models, Theories and the Crisis in Economic Thought

The Digital Revolution, which as we have seen, exerted a significant influence on theoretical developments in the natural and social sciences some 60 years ago, is now extending that influence into the philosophy of science itself. What is a theory? A model? What do we mean when we speak of a model as having autonomy? How does autonomy affect the concept of scientific representation? Most importantly in the present context, how do these debates escape the confines of philosophy and affect the current state of economic theory? Francesco Amigoni and Viola Schiaffonai (2008) have evaluated these questions. As they note, the great strength of computational models, recognised in the early days of the Digital Revolution, resides in their ability to process quantities of data such as those routinely encountered in molecular cell biology (Amigoni and Schiaffonati, 2008). But the platform had a consequence to some extent unforeseen. The enormous challenge presented by manipulating the ordinary differential equations (ODEs) which describe the kinetic properties of molecular interactions led investigators to question the necessity of such descriptions for many types of problems (For a similar argument see Bornholdt, 2008). In effect, computational modellers were increasingly led to ask that most fundamental of epistemic questions: 'what counts as knowledge?' (Amigoni and Scihaffonati, 2008). More exactly:

'The adoption of computer programs, namely computational models, is firstly intended to process, manage, and classify huge quantities of data. Moreover, programs serve also to account for the meaning of these data: what counts as knowledge and what we consider as knowledge depends on the data we are able to acquire, on the ways in which these data are collected, and on the form in which they are represented' (Amigoni and Scihaffonati, 2008, p. 121).

The historical result, as noted earlier, was the discretisation of the continuous processes traditionally represented by ODEs, an innovation pioneered during the 1980s in Stephen Wolfram's cellular automata (CA), and Stuart A. Kauffman's Boolean networks (BNs) utilised here. However, a remarkable feature of these approaches was the inability of the modellers, when presented with simulations of highly complex biomolecular interactions, to predict what the results would be, even when the rules of the simulation were precisely specified. Discrete models thus assumed a new and unexpected identity: they became exploratory constructions, 'artificial universes evolving in accordance with local but uniform rules' (Amigoni and Scihaffonati, 2008, p.121).

These methodological developments were, fortuitously, consistent with paralleling transformations in the philosophy of science. From the 1920s to the 1960s, the dominant understanding of scientific investigation – usually designated the 'Syntactic View' and most strongly associated with Rudolf Carnap, Carl Hempel, and Herbert Feigl – had placed considerable emphasis on the role of 'theoretical sentences'. The latter did not deploy natural language but instead contained logical and mathematical symbols, and the symbols of the theory. The theoretical sentences were in turn connected to 'observational terms', which referred to the observable properties of a phenomenon, by means of 'correspondence rules'

(sentences which included both theoretical and observational terms). This 'Received View' (Putnam, 1962) prevailed until the 1960s, when it was vigorously challenged by Patrick Suppes (1960) and Bas Van Fraassen (1980), proponents of a 'semantic' strategy. One of the key defining features of their Semantic of Theories (SVT) was the replacement of the syntactic edifice linked by correspondence rules with set-theoretic relations based on structural isomorphism. Motivated by mathematics and the empirical sciences, Van Fraassen proposed that 'models occupy centre stage' (1980), or more exactly, that a scientific theory gives us a family of models to represent phenomena. This major conceptual shift resulted in a view of theory 'as determined by the class of its possible realizations' (Amigoni and Schiffonati, 2008). Thus, all possible models of a theory are reduced

'to a particular subclass that is more manageable and easier to study, being a subset of the set of all models. The goal, hence, is to consider just a subset, limited and manageable, of the whole set of the models of the theory and to work on it' (Amigoni and Schiffonati, 2008, p. 119).

Motivated by these foundational changes, Margaret Morrison and Mary Morgan (1999) claimed that models had now acquired an enriched epistemic role. They are not derived from theory; neither are they fully grounded in empirical observations. Instead, they are 'semi-autonomous', sharing components with the world and theory, while not being fully connected with either. Žitko compares the semi-autonomy of models to statistical correlation:

With perfect correlation there is little new knowledge to be acquired since the two sets of data will share the same variation, while with zero correlation there is even less to learn since the two sets of data have nothing in common. It is only in between the extreme values that something more can be argued about the two data sets, and a meaningful research can begin' (Žitko , 2013, pp. 95-96).

Because of this semi-autonomy, models are remarkably fluid, evolving into novel constructions that challenge traditional theories and (often) illuminate the actual world. Support for this view of science, Morgan and Morrison (1999) suggest, is not to be found through formal arguments in the manner of the Syntactical school, but by finding common properties in the actual work of scientists. Accordingly, they consider accounts of model-building in economics, chemistry, and physics, eliciting from their analyses a portrait of the scientist closely resembling that of the artist. (For a similar conclusion based on extensive interviews with scientists and artists see the engagingly-written *Notebooks of the Mind* (1997) by Vera John-Steiner.) In a key passage, they note:

'As we have pointed out, there are no rules for model building and so the very activity of construction creates an opportunity to learn: what will fit together and how? Perhaps this is why modeling is considered in many circles an art or craft; it does not necessarily involve the most sophisticated mathematics or require extensive knowledge of every aspect of the system' (Morgan and Morrison, 1999, p. 30-31).

(Morrison and Morgan's construal of model-building is, of course, to be distinguished from the 'cobbling together' of models under emergency conditions discussed in *The Economist* article referenced at the beginning). This perspective is evidenced in a study by Olav Bjerkholt

(2007), which documents the early development of business-cycle theory (1920s–1930s), revealing in the process how 'bits of the business-cycle theory and evidence could be integrated together into a model' (Morrison and Morgan, 1999). The studies depict in detail how the cognitive 'notebooks' (John-Steiner, 1997) of the econometrician Ragnar Frisch were a dynamic amalgam of economic and physical theories (the latter including the famous, and controversial, pendulum analogy), statistics, direct observations and, intriguingly, 'heroic guesses, transgressing the observational facts' (Frisch, 2010 [orig.1930]). The 'model world' which emerged from Frisch's mediating approach comprised 'those indefinable things in the real world which we might call "essentials"...with regard to our own ends' (Frisch, 2010 [orig. 1930). R.I.G. Hughes (1997) anticipating the views of Amigoni and Schiaffonati (2008), has shown how Frisch's mediating approach lends itself to simulation. Deploying cellular automata (CA), he discovered 'generic cycles which had empirical credibility, and provided a prediction of a new cycle which had not yet been observed in the data' (Morrison and Morgan, 1999).

But Frisch's ideas were developed nearly a century ago. Can partial modelling address today's economic issues and, in particular, the current crisis in economic theory? We would argue that this is indeed the case, and would propose as a sample study that the several competing models of high-frequency trading (HFT) could be simultaneously subjected to a mediating, computational approach. HFT is a relatively recent computer platform, currently expanding throughout much of the developed world, and into the BRICS countries, in which firms use complex, high-speed algorithms to detect supply-and-demand opportunities, and to execute trades. These transactions, fully automated, are typically conducted in milliseconds (thousandths of a second); Johnson et al. (2013) report that a new chip, the iX-eCute, can 'prepare trades in 740 nanoseconds' (a nanosecond is a billionth of a second). Although a single HFT trade will often net less than a penny in profit per share, the ultrafast transaction speed permits thousands of transactions a day (Bell, 2013). The practice is spreading rapidly, transforming market culture into 'geographies' of competing algorithms (Grindsted, 2016). According to a 2016 estimate by the Congressional Research Service, HFT 'accounts for 55% of trading volume in US equity markets and about 40% in European markets' (Miller and Shorter, 2016). High-speed trading is intensely controversial - and hence the object of much model-building - especially since the May 6, 2010 "flash crash", and the later appearance of Flash Boys, a critical popular account of HFT (Lewis, 2015). Many recent studies assert that the practice may strongly contribute to national and global market volatility, and should therefore be subjected to stronger government regulation (Adrian, 2016). To explore HFT volatility, Johnson et al. (2013), utilising NANEX NxCore software, analysed the millisecond-resolution price stream 'across multiple stocks and exchanges' from January 3, 2006 to February 3, 2011. They detected 18, 520 sub-second 'extreme events' which, in turn were coupled to 'slower global instabilities'. A possible key factor underlying this instability. according to Austin Gerig (2012) is price synchronisation: if two securities are closely related, a price change in the first will generate, almost instantly, a similar price change in the second. This process, a 'gargantuan task' in the traditional stock market, given the more than 1000 transactions per second in US equities alone, can become highly destabilising in an ultrafast trading environment. Gerig's bio-inspired model proposes that HFT 'efficiency' - here, the rapid information transfer between related individual equities - may yield coordinated collective behaviour analogous to that of animal groups (herds of ungulates; schools of fish). So, following Levine (2014), we might ask: is HFT too efficient? Holly Bell (2013) suggests it is not, proffering a defence of HFT as the ultrafast realisation of Eugene Fama's 'efficient market'. Fama (1970) had famously argued that, at any given time, prices were an expression of all the available information on a particular stock market. This property was largely due to the preponderance of rational investors - the neoclassical Homo economicus; but a measure of irrational behaviour was also consistent with the view (Szyszka, 2007). The latter behaviour is generally uncorrelated, and so the investment decisions would likely cancel each other out. Alternatively, a rare (but in principle, possible) coordinated movement would result in a stabilising counter-movement by rational arbitrageurs. In Bell's model, HFT is a novel micro-world, differing profoundly from the traditional market, where algorithms, as agents, are almost instantly aware of price movements of other agents (Bell, 2013), and adjust their investment behaviour (bid-and-ask decisions) accordingly. Volatility does not result, therefore, from irrationality and swarming in the HFT micro-world, but is primarily due to the extraneous over-corrections of individual investors to dramatic economic events (e.g., the subprime mortgage crisis). These HFT models, and many others not considered here, would be appropriate starting-points for a partial-modelling strategy. Thus, the HFT swarming behaviour described by Austin Gerig systemically resembles that examined by Caetano and Yoneyama (2015) in a macroeconomic BN model of contagion in BRICS countries. Similarly, the putative efficiency of HFT claimed by Holly Bell would be amenable to BN approaches which model hubs and feedback loops, evaluate their connectivity, and their stabilising effects (Kwon and Cho, 2007).

4. Conclusion

Neoclassical economics, the traditional framework of the science, is widely viewed as an obsolete relic of early 20th century thought (Ackerman, 2002; Colander, 2007; Nadeau, 2008). Its obsolescence, it is held, is tellingly reflected in its axiomatised structure, its demonstrated inability to predict financial crises, and in its potential to generate ineffective and dangerous policies. This assessment may be correct. Yet it is also arguable that the neoclassical view will – and should – persist, at least in the short run, in the form of input models that contribute to a synthetic theory. What is required for theoretical advance, as well as for informed policy, is the deployment of today's powerful computational platforms to initiate the interactions of semi-autonomous partial models. As an intriguing digital mimicry of the human creative process - with demonstrated successes in medicine and cell biology - partial models are cognitive tools which can generate new theories in a manner no one can anticipate. This property is of signal importance because it impedes the Procrustean habit: the rote imposition of outmoded, but dominant views on non-conforming, recalcitrant data. Economics' selfcritical mood may thus have a salutary effect: The emergence of a changed science in which models are not formally derived from a set of governing axioms, but are cognitive instruments in a regime of exploration.

Acknowledgements

I am grateful to Arturo Hermann, Steve Keen and David Orrell for valuable comments. Responsibility for any errors of fact or interpretation is entirely my own.

References

Ackerman, F. (2002) 'Still dead after all these years: interpreting the failure of general equilibrium theory.' *Journal of Economic Methodology*, 9, pp. 119-139.

Adrian, J. (2016) 'Information inequality: How high-frequency traders use premier access to information to prey on institutional investors.' *Duke Law and Teaching Review*, 14, pp. 256-279.

Albert, I., Thakar, J., Li, S., Zhang, R., Albert, R. (2008) 'Boolean network simulation for life scientists.' *Source Code for Biology and Medicine*, 3.

Amigoni, F. and Schiaffonati, V. (2008) 'A multi-agent approach to modelling complex phenomena.' *Foundations of Science*, 13, pp. 113-125.

Bell, H. (2013) 'High-frequency trading: Do regulators need to control this tool of informationally-efficient markets?' *CATO Institute Policy Analysis*, 731, pp. 1-11.

Bjerkholdt, O., Ragnar (2007) 'Frisch's contribution to business-cycle analysis.' Memorandum 8, Department of Economics, University of Oslo.

Bock, M., Scharp, T., Tainikar, C., Klipp, E. (2014) 'BooleSim: an interactive Boolean network simulator.' *Bioinformatics*, 30, pp. 131-132.

Bornholdt, S. (2008) 'Boolean network models of cellular regulation: prospects and limitations.' *Journal of The Royal Society Interface*, 5, S85-S94.

Brix, V. (1970) You Are a Computer: Cybernetics in Everyday Life. White Plains, New York: Emerson Books Incorporated.

Caetano, M. and Yoneyama, T. (2015) 'Boolean network representation of contagion dynamics during a financial crisis'. *Physica A: Statistical Mechanics and its Applications*, 417, pp. 1-6.

Chamberlain, E. (2015) 'Microfinance in Algeria, Tunisia, and Lebanon.' University of Central Florida Honors in the Major Thesis (HIM), Paper 1698, http://stars.library.ucf.edu/honorstheses1990-2015/1698.

Chua, A. (2002) World on Fire: How Exporting Free Market Democracy Breeds Ethnic Hatred and Global Instability. New York: Doubleday.

Colander, D. (2007) 'Pluralism and heterodox economics: suggestions for an "inside the mainstream" heterodoxy.' *Middlebury College Economics Discussion Paper, 07-24*. <u>http://www.middlebury.edu/~econ</u>

Davidich, M. and Bornholdt, S. (2008) 'The transition from differential equations to Boolean networks: A case study in simplifying a regulatory network model.' *Journal of Theoretical Biology*, 255, pp. 269-277.

DiCara, A., Garg, A., De Micheli, G., Xenarios, I., Mendoza, L. (2007) 'Dynamic simulation of regulatory networks using SQUAD.' *BMC Bioinformatics*, 8, p. 462, DOI: 10.1186/1471-2105-8-462.

Fama, E. (1970) 'Efficient capital markets: A review of theory and empirical work.' *Journal of Finance*, 25, pp. 383-417.

Famelis, M., Salay, R., Chechik, M. (2012) 'Partial models: towards modeling and reasoning with uncertainty.' Proceedings: International Conference on Software Engineering. May.

Frisch, R. (2010) [orig. 1930] A Dynamic Approach to Economic Theory: Lectures at Yale University Beginning September, 1930. New York: Routledge.

Gerig, A. (2012) 'High-frequency trading synchronizes prices in financial markets.' SSRN *Electronic Journal*, November.

Greenwood, D. and Holt, R. (2008) Institutional and ecological economics: The role of technology and institutions in economic development.' *Journal of Economic Issues*, 42, pp. 445-452.

Grindsted T. (2016) 'Geograhies of high-frequency trading – algorithmic capitalism and its contradictory elements.' *Geoforum*, 68, pp. 25-28.

Helikar, T., Kochi, N., Konvalina, J., Rogers, J. (2011) 'Boolean modeling of biochemical networks.' *The Open Bioinformatics Journal*, 5, pp. 16-25.

Hughes, R.I.G. (1997) 'Models and representation.' Philosophy of Science 64, pp. 5325-5336.

Johnson, N., Zhao, G., Hunsader, E., Qi, H., Johnson, N., Meng, J., Tynan, B. (2013) 'Abrupt rise of new machine ecology beyond human response time.' *Scientific Reports*, 3, p. 2627.

Kaplan, R. (2012) *The Revenge of Geography: What the Map Tells us about Coming Conflicts and Battle against Fate*. New York: Random House.

Kauffman, S. (1969) 'Metabolic stability and epigenesist in randomly constructed genetic nets.' *Journal of Theoretical Biology*, 22, pp. 437-467.

Kauffman, S. (1991) 'Anti-chaos and adaptation.' Scientific American, 265, pp. 78-84.

Kwon, Y-K and Chao, K-H (2007) 'Analysis of feedback loops and robustness in network evolution based on Boolean models.' *BMC Bioinformatics*, BioMed Central.

Levine, M. (2014) 'High-frequency trading may be too efficient.' Bloomberg View, April 2.

Lewis, M. (2015) Flash Boys: A Wall Street Revolt. New York: W.W. Norton.

Maruyama, M. (1963) 'The second cybernetics: deviation-amplifying mutual causal systems.' *American Scientist*, 5, pp. 164-179.

Miller, R. and Shorter, G. (2016) 'High-frequency trading: overview of recent developments.' Congressional Research Service, April 4, 2016.

Morrison, M. and Morgan, M. (1999) 'Models as mediating instruments.' In: *Models as Mediators: Perspectives on Natural and Social Science.* Cambridge: Cambridge University Press, pp. 10-37.

Nadeau, R. (2008) 'The economist has no clothes.' Scientific America, 298, p. 42.

Noland, M. (2012) 'The black hole of North Korea: What economists can't tell you about the most isolated country on Earth.' *Foreign Policy*, March 7.

Ong, R. and Huber, E. (2014) 'American firms troubled by China's antimonopoly actions.' *China Business Review*. September 5.

Putnam, H. (1962) 'What theories are not.' In. *Logic, Methodology, and the Philosophy of Science.* Stanford: Stanford University Press pp.240-252.

Randhawa, R., Shaffer, C., and Tyson, J. (2009) 'Model aggregation: a building-block approach to creating large macromolecular regulatory networks.' *Bioinformatics* 25, pp. 3289-3295.

Richardson, K. (2005) 'Simplifying Boolean networks.' *Advances in Complex Systems,* 8, pp. 365-381.

Schlatter, R., Philippi, N., Wangorsch, G. (2012) 'Integration of Boolean models exemplified on hepatocyte signal transduction.' *Brief Bioinformation*, 13, pp. 365-376.

Simon, H. (1947) Administrative Behavior: A Study of Decision-Making Processes in Administrative Organizations. New York: Macmillan.

Stiglitz, J., (2010) *Freefall: America, free markets, and the sinking of the world economy*. New York: W.W. Norton.

Suppes, P. (1960) 'A comparison of the meaning and uses of models in mathematics and the empirical sciences.' *Synthese*, 12, pp. 287-301.

Szyszka, A. (2007) 'From the efficient market hypothesis to behavioral finance: How investors' psychology changes the vision of financial markets.' http://ssm.com/abstract=1266862

Turk, M. (2012) 'The mathematical approach in economics: Walras, the French mathematicians, and the road not taken.' *Journal of the History of Economic Thought,* 34, pp. 149-167.

van Fraassen, B. (1980) The Scientific Image. Oxford: The Clarendon Press.

Veliz-Cuba, A. (2014). 'Reduction of Boolean networks.' arXiv:0907.0285 [q-bio.QM]. Full text available on ResearchGate.

Žitko, M. 2013. 'Models, fictions, and explanations.' *Filizofia I Društvo,* XXIV, 4, pp. 84-101.

SUGGESTED CITATION:

Wallace, Ron (2019) 'Addressing the Malaise in Neoclassical Economics: a Call for Partial Models.' *Economic Thought*, 8.1, pp. 40-52.

http://www.worldeconomicsassociation.org/files/journals/economicthought/WEA-ET-8-1-Wallace.pdf

Commentary on 'Addressing the Malaise in Neoclassical Economics: A Call for Partial Models'

David Orrell, Systems Forecasting, Toronto, Canada dorrell@systemsforecasting.com

The article by Ron Wallace 'proposes the deployment of partial modelling, utilising Boolean networks (BNs), as an inductive discovery procedure for the development of economic theory'. The central argument in favour of partial models is well-made, and while I agree with this aspect of the paper, and the conclusion that models should serve as 'cognitive instruments in a regime of exploration,' I have a number of comments about the proposed strategy and the example of BNs.

The paper states that a theory 'should be tested for its ability to predict an actual economy,' and notes that BNs have been applied to areas including systems biology, 'frequently yielding results with high predictive power'. The implication is that a technique which is predictive in systems biology may also be useful for predicting the economy. However (speaking as someone who works in the area), in systems biology the word 'predict' tends to be used rather loosely. It often just means that a result which is already known and/or non-surprising can be reproduced, which is not the same as the usual meaning (e.g. predicting a financial crisis). A typical usage for example is the title of the paper 'Boolean Network Model Predicts Cell Cycle Sequence of Fission Yeast'. And when researchers refer to 'testing against empirical data to assess predictive power' (as in the response to a comment from Steve Keen on the Economic Thought Open Peer Discussion forum), this usually means calibration, unless it is done in a blind-tested fashion, which is extremely rare. This is an important distinction for this paper, because a sufficiently complicated model is very flexible and can be made to match known data, but may be poor at making non-trivial predictions (examples are given below). The article notes that 'BNs are remarkably flexible', which is not necessarily a good thing.

The article describes a method whereby BNs can be combined together to form a larger model; however this relies on 'simplification of the partial models to avoid an intractable result when they are combined', such as 'excluding node values that will produce multiple steady states. In addition, feedback loops are excluded because they can frequently yield oscillations.' An advantage of the strategy is 'the ability of BNs to include system components (e.g. cultural or religious variables) for which quantitative data are minimal or lacking, without significant loss of predictive power'. Furthermore 'it is possible to convert a BN into a continuous dynamical system configured as ordinary differential equations (ODEs)'.

The idea is therefore to patch together simple models to create a larger and more sophisticated model, while pruning features which create problems, and adding new nodes for missing information. However it should be noted that, if models are viewed as patches, it need not follow that combining the patches will give a better result, or that simply adding a node to a network is enough to account for missing information or dynamics.

To give a few examples from different contexts: in a model of gene regulation in yeast (Ramsey et al., 2006), it turned out that the main (experimentally verified) prediction concerned stochastic effects that were invisible to *any* ODE model, no matter how many equations or parameters were added. Predictive (and blind-tested) models used by drug companies to optimise cancer treatments rely on the careful modelling of dynamical cell

population effects of the sort that cannot be captured by either BNs or ODEs, so instead a combined ODE/agent-based approach is used (<u>https://www.physiomics-plc.com/technology/</u>), which nonetheless limits parameters to things that can be measured or estimated experimentally. A simple model, based on just a few parameters, outperformed large-scale biophysical 'gold standard' cardiac models, containing hundreds of parameters, at predicting the cardiac toxicity of drug compounds (Mistry, 2018). In cognitive science, quantum decision theory shows that decisions are inconsistent with classical probability: interference effects need not more detail, but a different kind of probability (Yukalov and Sornette, 2015). In economics, as Bezemer (2012) notes, the money system 'is alien to the (DS)GE models structure and trying to introduce it undermines key model properties'. It isn't therefore enough to add a 'finance node' to a model (general equilibrium or other). In all these cases, the proposed strategy would fail, because extending the model doesn't address the problem, which is not model size or complexity (in fact small is often better), but model structure (which is never perfect because a complex system doesn't reduce to equations) and the difficulty of identifying parameter values.

More generally, as these examples also illustrate, the strategy does not address the main practical limitations of modelling complex living systems, from a cell to an economy. The first is that as further detail is added to a model (e.g. extra nodes or equations), the number of unknown parameters increases, as does uncertainty about model structure, resulting in 'sloppy parameters' which cannot be determined from data (Gutenkunst et al., 2007). Second, such systems are also characterised by opposing positive and negative feedback loops (which the paper notes are sometimes omitted during model integration because they are destabilising). These are extremely hard to tease out from data because they are usually hidden (being often in a state of tension, they seem to cancel out), and also lead to complex unstable behaviour. Instability is of course not a problem in itself, since natural systems also exploit dynamic instability in order to produce rapid change, but in a model it means that a small change in parameters can give a very different result (Orrell, 2007, p. 266), and attempts to dampen the instability may just make the model less realistic.

What the cited paper (Schlatter et al., 2012) calls 'the final goal of a comprehensive dynamic model' may therefore remain elusive in systems biology as in other fields, despite its enduring popularity. (General equilibrium models were born out of neoclassical economists' intention to build such a 'comprehensive dynamic model' of the economy, though the dynamics were of the equilibrium sort.) However the problem is not the size or scope of the system being modelled, but its complexity – modelling a single cell is as hard as modelling the climate system.

One result of these limitations is that, paradoxically, simple models usually give the best predictive results (Makridakis and Hibon, 2000), but at the same time never give a complete picture. I therefore agree completely that models should be viewed as partial approximations and I found the discussion of this very interesting. It is also certainly the case that economics can learn much from systems biology, which uses a variety of models including ODEs, stochastic models, agent-based models, machine learning, BNs, and so on. However the argument of the paper points towards the goal of a comprehensive model (even if it is only for a segment of the economy such as HFT), patched together using semi-automatic techniques; and it isn't clear how this strategy (joining and extending models while pruning features) can address the main problems with economics models, which is that: (a) all the difficult features – such as feedback loops, human behaviour, money, and so on – have already been pruned out to avoid conflicts or an 'intractable result'; and (b) there are already too many 'sloppy parameters' that can't be determined from data (Romer, 2016).

In light of the structural and predictive limitations of models, I have argued for a different dashboard approach, which uses a range of model types (as in systems biology), but keeps them mostly separate, on the understanding that none are correct but each captures some complementary aspect of the underlying reality (see Orrell, 2018, p. 254; Orrell, 2017, p. 21-22). Model size should be limited in order to avoid the above-mentioned problems associated with dimensionality. So instead of seeing a BN (or other) as a partial model on which to add another model in order to form a larger (and less partial?) model, one treats any model as a partial model to be *complemented* by different models or approaches.

Finally, one should consider the role of incentives. In an area such as systems biology, which is relatively unconstrained by the need to make blind-tested predictions, there is a tendency to build ever-larger models of the sort critiqued in Mistry (2018). For something like 'theoretical controversies related to high-frequency trading' on the other hand, where, unlike in systems biology the debate over regulation is a matter of considerable interest to the financial sector, there are extremely powerful incentives at work which shape both underlying assumptions and the kinds of questions which are asked (Wilmott and Orrell, 2017, p. 194). I think a first step before combining models would be to investigate influences such as the source of funds for the various modelling approaches.

References

Bezemer D.F. (2012) 'Finance and Growth: When Credit Helps, and When it Hinders.' <u>https://www.ineteconomics.org/uploads/papers/bezemer-dirk-berlin-paper.pdf</u>

Davidich M.I., Bornholdt S. (2008) 'Boolean Network Model Predicts Cell Cycle Sequence of Fission Yeast.' *PLoS ONE* 3(2), e1672.

Gutenkunst R.N., Waterfall J.J., Casey F.P., Brown K.S., Myers C.R., Sethna J.P. (2007) 'Universally Sloppy Parameter Sensitivities in Systems Biology Models.' *PLoS Computational Biology* 3(10), e189.

Makridakis S. and Hibon M. (2000) 'The M3-Competition: results, conclusions and implications.' *International Journal of Forecasting*, 16, pp. 451–76.

Mistry H.B. (2018) 'Complex versus simple models: ion-channel cardiac toxicity prediction.' *PeerJ*, 6, e4352.

Orrell D. (2007) The Future of Everything: The Science of Prediction. New York: Basic Books.

Orrell D. (2017) 'A Quantum Theory of Money and Value, Part 2: The Uncertainty Principle.' *Economic Thought*, 6 (2), pp. 14-26.

Orrell D. (2018) Quantum Economics: The New Science of Money. London: Icon.

Ramsey S.A., Smith J.J., Orrell D., Marelli M., Petersen T.W., de Atauri P., Bolouri H., Aitchison J.D. (2006) 'Dual feedback loops in the GAL regulon suppress cellular heterogeneity in yeast.' *Nature Genetics*, 38 (9), p. 1082.

Romer P. (2016) 'The Trouble with Macroeconomics.' Available at <u>https://paulromer.net/wp-content/uploads/2016/09/WP-Trouble.pdf</u>.

Wilmott P., Orrell D. (2017) The Money Formula: Dodgy Finance, Pseudo Science, and How Mathematicians Took Over the Markets. Chichester: Wiley.

Yukalov V.I., Sornette D. (2015) 'Preference reversal in quantum decision theory.' *Frontiers in Psychology*, 6, pp. 1–7.

SUGGESTED CITATION:

Orrell, D. (2019) 'Commentary on "Addressing the Malaise in Neoclassical Economics: A Call for Partial Models".' *Economic Thought*, 8.1, pp. 53-55. <u>http://www.worldeconomicsassociation.org/files/journals/economicthought/WEA-ET-8-1-Orrell.pdf</u>