

# Reviews of Rethinking the theoretical foundations of economics

## Round 1

### Reviewer 1

#### OVERALL SUMMARY

This is a highly ambitious and potentially very important article. The idea of applying evolutionary ideas to economics is welcome and pertinent. There are some important core ideas in the paper. In particular, the idea of deploying multilevel selection theory to enhance insights in economics is particularly valuable.

However, much of the paper is marred by loose formulations and mischaracterizations of key terms and arguments in orthodox economics. These errors will alienate potential readers and distract attention from the positive contribution in the essay. The paper needs a major rewrite to remove the errors and to make its positive contribution sharper, clearer and more effective.

Along with reading the paper itself, these comments have taken three full days to draft. This exceptional (for me) effort has been devoted to this one article because of the accepted importance of pushing evolutionary ideas in the social sciences. But this report is very critical of this draft. It is hoped that these criticisms are seen as ultimately constructive, as helping the development of a paper (or papers) that will eventually be powerful and persuasive.

#### THE RHETORICAL STRATEGY OF THE PAPER

Not enough thought has been given to the rhetorical strategy of the paper. As it stands it is flawed. In part, rhetoric is about the choice of an audience. That readership could be any combination of (a) orthodox economists, (b) heterodox economists, or (c) other social scientists.

In each case the rhetorical strategy is weak. Orthodox economists (a) will be quickly alienated by the inaccurate or unnuanced depiction of orthodox views right from the start. Heterodox economists (b) will be repelled by the fact that the paper makes critical points (seemingly as if they are original) that have been said by many heterodox economists before (but with hardly any reference in the text under review to heterodox scholarship in these areas). Similarly, there is very little engagement with other social scientists (c). The overall failure to recognise much important past work in the areas touched on in the paper will alienate scholars in groups (a), (b) and (c).

Not only is this rhetorically impaired, but also it is flawed scientific practice. For science to work it must be cumulative. Contributions must overturn and/or build on previous published work. Of course, it is impossible to cite more than a tiny fraction of previous scholarship. But making some connections with preceding work is vital. Any paper in this area will be judged in part by evidence of previous knowledge on the relevant topics.

Take one key issue. The paper insists (rightly in my view, but with excessive repetition and insufficient clarification) that social relations are important as well as individuals. There is a massive literature on this, in economics, social theory and other social sciences. None of the important contributions in this area are referenced. The insight (that social relations are important as well as individuals) is presented in this paper as if it is original. Little further nuance or clarification is added. Consequently, other social scientists (c), especially those familiar with social theory, could also be alienated by this paper.

The lack of precision in making this important point is also relevant. Most sociologists and social theorists will agree to the basic claim (that social relations are vital, as well as individuals) but regret the failure in this paper to make this sharper and more meaningful, and the failure to build on the work of others that attempts to bring such necessary clarifications.

An effective rhetorical strategy is also undermined by the structure of the paper, and also in part by its contents, particularly the material in Part I. At the outset the paper announces a “new paradigm”. This immediately raises questions for the reader: (1) Why should we ditch the current paradigm? (2) What is the new paradigm? And (3) why is the new paradigm better than the old one?

There is a high probability that orthodox and other economists will lose patience with the arguments when they come across some of the flawed characterizations of the orthodox position in the opening pages. Details of these serious flaws will be raised in several places below.

## THE CHARACTERIZATION OF THE “FIVE BASIC ASSUMPTIONS” OF ORTHODOX ECONOMICS

### Basic Assumption 1

Orthodox economics is accused of “individualism”, defined here by the assumption that individuals are “the basic units of decision making” (page 4). This is an unusual definition of individualism. But that is not the only problem. Furthermore, the accusation that orthodox economists always take the individual as “the basic units of decision making” is demonstrably false. Orthodox microeconomics has often (but not always) treated firms or households as if they were single decision makers. Nobel Laureate Douglass North and his followers often treated organizations as single actors or decision makers. Orthodox macroeconomics has sometimes assumed that a whole economy can be characterized as if it were a single decision maker. The assumption that “government” is a decision maker is commonplace in orthodox and heterodox economics.

Mainstream economists have often discussed collective decision making. I put “collective decision making” into the JSTOR list of journals, in economics only, and I got 1,698 hits, including a significant number in “top” journals. Hence it is wrong to describe orthodox economics as “individualist”, if “individualist” means the denial of collective or group decision making.

Also it is not new to say that “human decisions are made both individually and collectively” (as on page 17). There is a substantial literature (albeit outside economics) on the meaningfulness of collective or group decision making (e.g. Douglas, 1986; List and Pettit, 2011; Searle, 2002; Tuomela, 1995). It discusses the possibility of “we intentions”. This literature is overlooked in this paper. Strikingly, although “individualism” is defined up-front in terms of taking “individuals as the basic units of decision making”, there is little further discussion of this “decision making” issue in the whole paper. To be more specific, there is no explanation of how entities other than individuals could make decisions. There is much important discussion of the social context of decision making in the paper, but that in itself does not necessarily point to any kind of decision maker other than the individual.

Elsewhere and throughout this paper, the term “individualism” is used far too loosely. Furthermore, defining individualism in terms of the denial of decision making by groups or collectives is rather unusual and would require much more explanation and justification. But as a basic accusation against orthodox economics it is false. It is best removed.

We might also raise the question of who or what would be the basic decision makers other than individuals? What does making a decision “collectively” mean? If it means individuals making decisions through some framework of procedures, voting etc., then again there is nothing new. And individuals are still making decisions, as well as collectives.

Making a decision “collectively” does not mean that individuals do not decide also. We might want to explain how an organization or collective reached its decision. It is really picking a difficult (and here unnecessary) fight to suggest rather bluntly that individuals are NOT the basic “units of decision making”.

The very loose use of the term “individualism” to rebut orthodox economics is combined with a failure to acknowledge the several different types of individualism and the large literature discussing them. See Lukes (1973), O’Neill (1973) and Bunge (2000) for perceptive critical introductions to different types of individualism, including ontological individualism, methodological individualism, ethical individualism, political individualism, and so on. Not all individualisms are the same, and one type of individualism does not necessarily imply another.

The quote from page 4 noted above seems to suggest a version of ontological individualism – about the loci of decision-making. It is not about explanation, so here it does not amount to methodological individualism. If this ontological line of argument is to be pursued, there should be some reference to the literature on ontological individualism, ranging from Archer (1995) through Bunge (2000) to Epstein (2015) and many others.

## Basic Assumption 2

It is alleged in this paper that orthodox economics has a second “basic assumption” of “socially disengaged behavior (i.e., behavior generated by the pursuit of individual objectives subject to constraints, rather than by social interactions)” (page 4).

This characterization will be quickly rebutted by orthodox economists, who will point to abundant examples of social interactions in their models. All these interactions involve individuals and are (socially) structured in some way. Even in perfect competition models there are limited interactions. Individuals make contracts with each other. These interactions become more prominent and consequential in orthodox models of imperfect competition, oligopoly, monopoly, etc. Standard neoclassical theories, including partial and general equilibrium models, involve relational interactions between individuals.

These interactions may be portrayed inadequately in orthodox economics, but that is not the claim by the authors of this paper – they wrongly claim that they are absent. Some leading neoclassical economists have made these social interactions explicit in top journals (Nobel Laureate Kenneth Arrow, 1994).

Nobel Laureate Vernon Smith has recently argued that experimental economics points to the importance of social context and social interactions (Smith and Wilson, 2019). Game theory is infused with examples of strategic social interaction, where agent behaviours are dependent on the structures and rules of the games involved.

I fear that the second “basic assumption” will be quickly rebutted by most orthodox economists. They will stop reading this important paper at this point. But while the criticism is inaccurately formulated, there is more than a hint of possible truth. This hint is made later in the paper too, but it is underdeveloped. A more valid criticism would be that (many but not all) orthodox economists assume preference functions that are fixed for life. For orthodoxy, social interactions with others may affect the decisions made with these preference functions, but not the functions themselves. Preference functions are fixed in individuals, and they are ontologically inert. This is the same point that is raised in this review in regard to the first basic assumption above. Basic assumptions 1 and 2 converge on one issue – the assumption of fixed preference functions.

As noted above, the important criticism that many orthodox economists assume ontologically inert preference functions that are fixed in individuals, can be linked to an evolutionary perspective that stresses individual development and transformation in response to circumstances. But this point is not new. See Veblen among many others. It has been widely discussed before.

## Basic Assumption 3

The third “basic assumption” of orthodox economics to be criticized in this paper is “ignorance as risk (i.e., as lack of information about realized values of variables with well-defined probability distributions)” (page 4).

It is unclear what is meant here. Is the accusation that orthodox economists treat ignorance of a possibility (somewhat perversely) as knowledge of that possibility, where that possibility is specified in terms of a probability? Or, do orthodox economists treat ignorance as imperfect information, rather than a lack of information? Much more clarification is needed.

After the criticism is made clear (so far it is not) I would need evidence that prominent orthodox economists make such mistakes. No evidence is provided. This unclear criticism is raised again, several times in the paper. For example, on page 26: "Economists refer to gaps in our knowledge as 'imperfect information.'" That is true. But it does not mean that they refer to ignorance as risk, or that they treat unknown events as probabilities.

A JSTOR search in economics journals showed that "imperfect information" got 7,809 hits. But that is not the only term they use. "Incomplete information" got 6,744 hits, "missing information" 1,341 hits, "imperfect knowledge" 1,235 hits.

This unclear criticism is further confused by an odd use of terminology. On page 7 there is: "Ignorance as uncertainty" (subheading iii). This is perplexing. There is no reference in the paper to the distinction between risk and uncertainty, made by Knight (1921) and Keynes (1921, 1936, 1937). Following them, it is important to distinguish between

(a) risk (where, by definition, it is possible to calculate a probability by some method),

(b) uncertainty (in the Knight/Keynes sense, where it is impossible to calculate a probability), and

(c) ignorance.

Typically, orthodox economists ignore (b), and they sometimes confusingly describe risk (a) as uncertainty. They ignore (b) because it is difficult to model (but see Brady and Arthmar, 2012). But it is not true that they always ignore (c), or generally reduce it to (a) or (b). There are many orthodox models where agents suffer from a degree of ignorance (c). Game theory has plenty of such models.

The authors seem to introduce non-standard meanings here, and this should be avoided. This is also unclear later in the paper, where these terms are used in confused ways (see below). Hence, instead of "ignorance as risk", which is unclear and confusing, it would be better (if at all) to address more broadly an orthodox "failure to address chronic information problems, such as ignorance and uncertainty". The paper under review does not address the relevant literature on this topic, of which Knight (1921), Keynes (1921, 1936, 1937), Shackle (1972) and Loasby (1976) are but a small part.

Overall, I see little value in this unclear criticism of orthodoxy, and I would advise that it is dropped. A focus on uncertainty (contra risk) and on ignorance could be an important topic for another essay.

#### Basic Assumption 4

The fourth “basic assumption” of orthodox economics to be criticized in this paper is “wellbeing understood in terms of utility (i.e., a single metric of wellbeing, based on consumption preferences, which can be revealed through economic decisions)”.

This is an important criticism. On a small point, it should be made clear that this is about “welfare”, as well as “wellbeing”. Orthodox economists more often use the term welfare. Most orthodox economists do focus on utility as the sole measure of welfare.

The detailed criticism of this assumption in the paper is not very powerful. It is sometimes unclear. I could not find anything that was original. Earlier critics of this assumption are overlooked. The criticism here does not build on the work of others.

In addition, the introduction of the criticism is flawed. It is not true that utility is based solely on “consumption”. It is now widely assumed in orthodox economics that we can gain utility from observation of, and interaction with, others, as well as by the consumption of goods and services. Utility can also change as a result of other stimuli, leading for example to a change in mood (and utility). Most importantly, see the literature on “social preferences” for example (Fehr and Fischbacher, 2002; Bowles, 2016). Jeremy Bentham, the founder of utilitarianism, also argued that pain/pleasure or utility could change as a result of various factors, and not just by consumption.

It is also wrong to suggest that individual utility maximization implies a “single metric of wellbeing” or overall welfare. Although a single metric may be assumed for the individual, that does not apply to the assessment of welfare in the economy as a whole. The Pareto welfare criterion is one possibility among many. Even within a Paretian framework we have modifications, such as Kaldor-Hicks compensation (Kaldor 1939; Hicks 1939).

There is no mention in the paper of one of the most important critics of utility-based welfare assessments, that is the economic Nobel Laureate Amartya Sen (e.g. 1977, 1979, 1991). He is one of the most important critics of mainstream economics in this area. Veblen is an important earlier critic. See also the works of Kenneth Boulding and Nicholas Georgescu-Roegen.

The proposed alternative to utility-based assessments of welfare appears on page 7: “Multi-faceted, context-dependent wellbeing” (subheading iv). This could be an important way forward. But there is no recognition that this kind of thing has been said, used (and criticized) before. If you put “multidimensional welfare indices in economics” in Google Scholar, then there are several books and articles, going back 30 years or so.

The development by the United Nations and others of a “human development index” (HDI) is an important issue here. The HDI serves as an important alternative measure to GDP per capita. But there is no mention of this index in the paper.

The development of alternative, non-utilitarian measures of welfare is very

important. But to have an impact it needs to be done carefully, with reference to previous attempts and to assessments of their value. I do not think that this can be tackled adequately in the present article. It would require a full and separate study.

#### Basic Assumption 5

The fifth “basic assumption” of orthodox economics to be criticized in this paper is “progress conceived primarily in terms of economic growth”.

This is clearly very relevant, but it is not a universal assumption of all orthodox economists. Some neoclassical economists advocate lower and sustainable economic growth (Stern 2007; Stiglitz 2016). A JSTOR search of journals in economics for “sustainable growth” or “sustainable economic growth” found 2,878 hits, including several in top journals such as the American Economic Review.

Furthermore, many prominent economists criticize the focus on economic growth. Happiness economics is a case in point. Many economists have been involved in developing other indices, such as the HDI.

All this is very important. But it needs to be done properly, building on the existing literature and moving forward, step by step. Despite what is said, there is no “new paradigm” announced on this in the current paper. What has been done before in this specific area of development or welfare measures is largely ignored.

#### A SUGGESTED STRATEGY FOR (RADICALLY) REVISING THIS PAPER

Currently this paper addresses far too many issues, and the treatment of each one is deficient, for reasons given above and below.

Consequently, the ambitions of this paper should be dramatically reduced. It may be useful to outline issues that might be tackled in subsequent work, but it is impossible to deal properly with more than a fraction of the topics that are addressed in the current version, even if its current massive (and excessive) length is retained.

The second crucial strategic suggestion is to avoid blaming “orthodox economics” as a whole for the various sins that are listed. Many of these blames are inaccurate, and in any case, it is better to coax rather than bludgeon the orthodox reader. More carrot, less stick. More modesty, less hype.

Hence, the claim that the paper presents a “new paradigm” for economics should be dropped. With the exception of the application of multilevel selection, there is little that is both persuasive and particularly new in the paper. In addition, it is not in a sufficiently robust and complete shape to pass muster as a new paradigm.

Pointing to the need for a new paradigm would be OK. But implying that you have got one wrapped up and ready to go is a different matter. Write about changing the “perspective” rather than the “paradigm”.

In several places the current paper falls below scholarly standards. Much of it is unclear. Crucial key terms are often undefined. There is a general failure to cite and build on previous work. At least a few references are required to back up and lend some authority to each argument. Without such references, alongside perceptions of overblown claims of novelty, the paper will not be taken seriously.

Much of the paper could be cut. Almost all of Part I could go, for reasons raised above and below. I would further suggest that if the paper is to exceed 40 pages, then it should be split into two or more parts, so that no part is above 40 pages. It is implausible to ask referees to referee any more than that. There is a need to consider what an unpaid referee can reasonably be asked to do.

Something like Part 2 could be the first paper/part and something like Part 3 could be the second paper/part. Accordingly, the new first paper/part would address the importance of evolutionary ideas for economics and the second paper/part would address some of the applications and policy implications. The new paper part 1 would start along the lines of “this paper explains how key ideas taken from evolutionary thinking can help to improve economics and move it forward”. This would interest many orthodox and heterodox readers. It may all lead eventually to a “new paradigm” but I strongly suggest that this formulation is not pushed at this stage. This change in tone and rhetorical strategy is important.

The first part would then go on to discuss multilevel selection theory and (as suggested above) and the non-evolutionary status of the assumption of fixed preference functions. But note that the latter will bring you into conflict with economists (such as Sam Bowles and Herb Gintis) who strongly promulgate evolutionary ideas but generally assume that preference functions are fixed and that individuals are utility maximizers. Evolutionary-based alternatives to utility maximization might be indicated, for example the work of W James, Veblen and Dewey on instincts and habits. On all that, Hodgson (2004) might be useful as a source of references etc.

The paper needs to explain more clearly and robustly what the authors mean by “evolutionary economics”. There are several different versions, and much vagueness over what is meant. Hodgson (2019a) provides an overview, pointing to communalities and divergences.

Veblen like others insisted that assumptions in economics must be consistent with our knowledge and understanding of human evolution, including its biological and psychology components. This issue is touched on in the paper, but it could be made a stronger feature. See Hodgson’s (2004) “principle of evolutionary explanation”. But again, it needs to engage with likely rebuttals, like the claim of Robson (2001a, 2001b, 2002) and others that human preference functions are consistent with evolution. (The problem here is that preference functions are non-falsifiable and hence consistent with any empirical evidence. But they have little explanatory power.)



## PARTICULAR COMMENTS

Note that detailed comments have been made to the first 26 pages only, with a couple more added at the end.

Page 6. The authors write: “the approach proposed here ... As an economic paradigm, it aims to be a coherent structure for organizing thought on economic activity and making sense of economic interactions, with significant explanatory power. As such, it differs from the “heterodox” and “pluralist” economics movements (for example, Davis, 2006; Foldvary, 1996; Lawson, 2005) that do not adhere to any particular paradigm and thus cannot offer such a coherent structure.”

The final sentence is problematic. First, a distinction should be made between the advocacy of pluralism and the advocacy of heterodoxy. Pluralism is simply the view that economics as a discipline in academia should accommodate multiple (perhaps fundamentally different) viewpoints. We can advocate pluralism in the academy while promoting one approach and criticising others. Criticising another approach does not necessarily amount to a call for its exclusion from a discipline.

Several particular approaches might be described as heterodox. The various “heterodox” economists each propose a root-and-branch reform of economic theory. The trouble is they cannot agree what those changes should be (Hodgson, 2019b). But in any case, it is very wrong, as the authors of this paper suggest, to claim that heterodox economists do not want to offer a coherent and fundamental alternative to orthodoxy.

After alienating many mainstream economists, page 6 (as currently formulated) is likely to alienate the heterodox economists as well.

Page 6. The use of “evolutionary science” is introduced here. The authors should at this point show their awareness of “evolutionary economics”, particularly of the Nelson and Winter (1982) kind. This kind of “evolutionary economics” is raised on page 33. But it should be mentioned up front in the paper as well.

Page 7. “The primacy of social relations” (subheading ii). What does this mean? Does “primacy” mean temporal primacy – i.e. social relations came before individuals? This is clearly false: they emerged together. The following sentences do not establish “primacy”: instead they establish the interdependence of individuals and social relations. That is a more tenable position. So the subheading should be changed to reflect this. “Primacy” is misleading.

Incidentally, there is a huge theory in social theory on the interdependence or mutual constitution of individual and social relations (or structures). Archer (1995) is a good place to start.

Page 7. “Ignorance as uncertainty” (subheading iii). This is confusing. It is important to distinguish between (a) risk (where it is possible to calculate a probability), (b) uncertainty (in the Knight/Keynes sense, where it is impossible to calculate a probability), and (c) ignorance. Typically, orthodox economists ignore (b) and they sometimes confusingly

describe risk (a) as uncertainty. It is not true that they always ignore (c), or generally reduce it to (a) or (b). There are many orthodox models where agents suffer from a degree of ignorance (c).

Page 7. “Multi-faceted, context-dependent wellbeing” (subheading iv). My main concern is that there is no recognition that this kind of thing has been said, used (and criticized) before. If you put “multidimensional welfare indices in economics” in Google Scholar, then there are several books and articles, going back 30 years or so. Also there is no mention here (or anywhere in the entire article) of the important work of Amartya Sen (e.g. 1977, 1979, 1991). Sen is one of the most important critics of mainstream economics in this area.

Pages 12-13. Contra Robbins, it is important to distinguish between local (or immediate) and global scarcity. Some things are globally scarce – water, coal, etc.. Some things are not globally scarce – love, trust, honour – because more of them can be generated. Local scarcity refers to the time and resources needed to satisfy wants, even if the satisfiers are globally abundant. Local scarcity accounts for Darwin’s ‘struggle for existence’. At least for organisms, local scarcity is universal.

Pages 13. “The objectives of individuals are not “given” (predetermined).” It would be useful here to challenge the idea of preference functions that are assumed to be fixed from birth.

Page 14. “Furthermore, people’s non-positional wants are not necessarily unlimited. There is only so much food that we can eat and only so much clothing that we can wear. What is potentially unlimited are our positional desires.” This confuses availability (2nd sentence) with wants/desires (1st and 3rd sentence). They are different concepts. Desires can be unlimited even if availability is limited. This is a very basic point. The error here will cause most trained economists to dismiss the entire essay. (The concept of a positional good is also introduced without explanation. There is a literature on this too.)

Page 14. The notion that “people’s objectives are all reducible to the individualistic enjoyment of the consumption of goods and services” is criticized here. But many orthodox economists would not formulate it this way. For them, what matters is individual utility. And it is widely accepted by orthodox economists that “the consumption of goods and services” are not the only source of utility. It is widely accepted by orthodox economists that some individuals can gain utility by observing the satisfaction of others. Hence the (now large) literature on social preferences (e.g. Fehr and Fischbacher, 2002; Bowles, 2016; but see Wilson, 2010). Among other things, social preferences have been used to “explain” (a version of) altruism. Altruism has long been a topic within orthodox economics (Becker, 1976; Collard, 1978; Samuelson, 1993).

Page 15. The notion that “Only when individuals use resources efficiently can these resources be used efficiently in the economy as a whole” is criticised. But this is not an accurate depiction of the orthodox position. Orthodox economists assume that every individual maximizes utility (under constraint).

so every individual is always efficient, in these terms. In orthodox economics there are several notions of “efficiency of the economy as a whole”. And some orthodox economists reject the very notion of “efficiency of the economy as a whole”.

Page 15. The above-quoted sentence is followed by this: “Thereby the definition of economics presupposes methodological individualism.” In response: (a) methodology is about explanation. The preceding sentence is primarily about efficiency rather than explanation. Hence it is difficult to draw conclusions about methodological individualism, at least without further explanation. (b) There is no consensus definition of the meaning of “methodological individualism” among either its exponents or its critics (Hodgson, 2007). Yet it is simply used here without explanation. I strongly recommend that the term is dropped (as with the loose talk of “individualism” elsewhere in the paper).

Page 18. “people can also pursue collective objectives, such as they do when they follow social norms, share common moral values, or vote for their elected representatives”. This claim is put out as novel. Yet I cannot think of an orthodox economist who would deny it. But some might prefer “shared” to “collective”.

Page 18. “Even when individuals have social preferences, their welfare depends on other people’s welfare outcomes, but not on the nature of the social relations within societies. Economies are viewed as independent of societies. This is the sense in which orthodox economics is socially disengaged.”

It is good to see that social preference are mentioned. But in principle orthodox economics could easily make individual “welfare” (it would be better to say “utility”) dependent on social relations. If I enjoy watching soccer, part of the pleasure may be to see how the players interact on the field – teamwork etc.. In which case social relations would affect my utility. If I dislike politicians that lie, it may not be simply the lie that offends me, it may also be the pollution and corruption of social interactions and of the social fabric. Furthermore, the (vague) claim that economies are seen by orthodox economists as separate from societies has little support or substantiation in modern orthodox economics.

Page 18. “In the orthodox paradigm, economic behavior is socially disengaged. This means that an individual’s economic behavior is generated by how they individually value the goods and services per se, not by the social relations among individuals.” Neither sentence is strictly true. Orthodox economics does consider social relations or structures (see Arrow 1994). It may downplay them. But it does not (and arguably cannot) ignore them. The second sentence is also inaccurate. Consider the example of the soccer game, as raised above.

Page 19. “This social disengagement is apparent in the conventional conception of the structure of the economy” – the circular flow. But the circular flow is not the only structural framework raised in orthodox economics. There are several other presentations of economic structure in

mainstream economics, including those that focus on different types of input and output, input-output analysis, classifications of sectors by types of ownership etc.. The circular flow is one among several. It is used by mainstream economists to present just one aspect of the whole.

Furthermore, it is not clear how the circular flow diagram supports the “social disengagement” accusation. It all depends on what is meant by “social”, which is not defined by the authors. The “economy” also requires definition. In the circular flow diagram, households involve families, and “social” (?) relations such as marriage. Orthodox economists such as Nobel Laureate Gary Becker (1981) have famously investigated these. “Firms” also include charities, as long as they produce goods and services. Are these social? There is a large literature on the economics of religion. Is religion “social” too?

Hence there are two major problems with this “social disengagement” accusation. First, the meanings of “social” and “economy” are unclear. Second, a Robbinsian definition of economics applies to any “social” or any other situation where there is scarcity and choice (as with Becker’s work). Hence some might argue that a Robbinsian definition of economics can make the discipline more “socially engaged” than the alternatives – even if we dislike the way that engagement is made. The Robbins-Becker trajectory is sometimes described as “economic(s) imperialism” because it spreads their mode of analysis over social, political and even extra-human domains.

Page. 20. “Mainstream economics does little to explore how economic relations are embedded within political and social relations taking place within planetary boundaries.” Once again, it should be noted that the intended meaning of “social” is not clearly defined in the paper. Mainstream economists will respond that from Becker (1981) onwards, numerous economists have been exploring gender relations, the family, the economics of religion, and so on. There are even “feminist economists”, with their own journal, several of which use mainstream tools, including utility maximization and game theory. Again, we may not always like the way the analysis is performed, but to say that these “social” issues are ignored is wrong.

Turning to the “planetary boundaries”, there is a legion of mainstream economists looking at the environment, global warming, limited planetary resources, etc.. This is (kind of) admitted further down the page, mentioning Nordhaus among others. Is being suggested by the authors that Nordhaus is not a mainstream economist? He employs utility analysis and he commits several other sins that the authors identify in this paper.

Page 21. “Since countries differ markedly in terms of their institutions and legal practices, the rules whereby goods and services are bought and sold and the rules whereby these transactions are financed must differ accordingly. These differences are largely overlooked in macroeconomics.” Again, orthodox economists will loudly protest. Overall there is now a huge literature in mainstream economics on the impact of different kinds of institutions at the macro and micro levels. Again, we may not like the way the analysis is

performed, but to say that these issues are ignored is wrong.

Page 21. “the interaction of the economy with society is missing ... Several other factors contribute to the oversight. Economic analysis focuses almost exclusively on utilitarian consequentialist values; that is, people are assumed to pursue the activities that yield most “utility,” depending primarily on consumption of goods and services. There is no explicit description of the possibility that goods and services may derive their value from the social relations in which their exchange is embedded.”

First, utilitarian consequentialism can be applied to “the interaction of the economy with society” (whatever “society” means). Bentham applied it to a host of things. Many factors can cause pleasure, pain, utility or disutility. There is nothing in utilitarian consequentialism that implies a focus on goods and services alone.

Second, it is mistaken to suggest that orthodox economics does (or is impelled) to focus on goods and services alone. Anything that affects utility can be put in an orthodox framework.

Page 21. Methodological individualism (undefined) again. See above.

Page 22. I cannot think of any orthodox economist, or any social scientist, who would object to Figure 2. Who, for example, has said that the economy does not include social relations, or that politics does not matter, or that the economy or society are not embedded in the natural environment? This is not a new paradigm. It is commonplace. For example, the study of the interaction between the polity/legislature and the economy is a common theme. See the works of Daron Acemoglu, James Buchanan, Glaeser, Shleifer, Niskanen and many others.

Page 24. Gifts are introduced here. There is a huge literature in mainstream and heterodox economics on gifts. None of it is mentioned.

Page 25. The moral limits to markets debate is introduced here, citing Sandel and Satz only. Sandel’s arguments have been strongly criticized. Again the literature is otherwise ignored. This debate has moved on considerably since the contributions of Sandel and Satz, including on issues such as the sale of kidney transplants.

Page 25. What is meant by “economic markets”? What markets are not “economic”?

Page 25. A “distinction between economic and social exchanges” is introduced but it is not clear what is meant by this. Are gifts “economic” or “social” exchanges, or both? What makes them one or the other?

Page 25. “Most economic exchanges involve some degree of trust”. This has been said many times before, but again this large literature is ignored.

Page 25. I would suggest that the problem is not that the “porousness of economic and social relations is ignored in mainstream economic analysis” but that mainstream economics does not have a clear definition of “market” or “contract” and consequently it is unable to distinguish between market and non-market phenomena (Hodgson, 2020). The problem here is that much of

orthodox economics has insufficient institutional scaffolding, largely because it attempts to be ahistorical and universal.

Page 26. "If a proposition is either true or false, then propositional knowledge must be person-and perception-independent." These seems to be an attack on philosophical realism, although it is far from clear. If it is meant to be an attack on philosophical realism, then it raises the ambitions of this essay far too high. This material should be cut.

Page 26. "Economists refer to gaps in our knowledge as "imperfect information." Yes, sometimes, but not always. A JSTOR search in economics journals got "incomplete information" 6,744 hits, "missing information" 1,341 hits, "imperfect knowledge" 1,235 hits, compared with "imperfect information" 7,809 hits.

Page 36. "We propose that economics be defined as the discipline that explores how resources, goods and services can be mobilized in the pursuit of wellbeing in thriving societies, now and in the future." This conception of economics is posited as if it were new. But it is as old as Adam Smith, and it is still used by some mainstream and many heterodox economists. The authors argue reasonably that such a definition of the subject should replace the Robbins definition. But many economists have argued this before. It is not new. It does not constitute a new paradigm.

P.136. Reference. It should be Hodgson, G. M. and Knudsen, T. (2010)

## REFERENCES

- Akerlof, George A. and Kranton, Rachel E. (2005) 'Identity and the Economics of Organizations', *Journal of Economic Perspectives*, 19(1), Winter, pp. 9-32.
- Archer, Margaret S. (1995) *Realist Social Theory: The Morphogenetic Approach* (Cambridge: Cambridge University Press).
- Arrow, Kenneth J. (1994) 'Methodological Individualism and Social Knowledge', *American Economic Review (Papers and Proceedings)*, 84(2), May, pp. 1-9.
- Becker, Gary S. (1976) 'Altruism, Egoism, and Genetic Fitness: Economics and Sociobiology', *Journal of Economic Literature*, 14(2), December, pp. 817-26.
- Becker, Gary S. (1981) *A Treatise on the Family* (Cambridge, MA: Harvard University Press).
- Bowles, Samuel (1998) 'Endogenous Preferences: The Cultural Consequences of Markets and Other Economic Institutions', *Journal of Economic Literature*, 36(1), March, pp. 75-111.
- Bowles, Samuel (2016) *The Moral Economy: Why Good Incentives are No Substitute for Good Citizens* (New Haven: Yale University Press).
- Collard, David (1978) *Altruism and Economy: A Study in Non-Selfish Economics* (Oxford: Martin Robertson).
- Brady, Michael Emmett and Arthmar, Rogério (2012) 'Keynes, Boole and the Interval Approach to Probability', *History of Economic Ideas*, 20(3), pp. 65-84.
- Bunge, Mario A. (2000) 'Ten Modes of Individualism - None of Which Works - And Their Alternatives', *Philosophy of the Social Sciences*, 30(3), September,

pp. 384-406.

Davis, John B. (2003) *The Theory of the Individual in Economics: Identity and Value* (London and New York: Routledge).

Douglas, Mary T. (1986) *How Institutions Think* (London and Syracuse: Routledge and Kegan Paul and Syracuse University Press).

Epstein, Brian (2015) *The Ant Trap: Rebuilding the Foundations of the Social Sciences* (Oxford and New York: Oxford University Press).

Fehr, Ernst and Fischbacher, Urs (2002) 'Why Do Social Preferences Matter – The Impact of Non-Selfish Motives on Competition, Cooperation and Incentives', *Economic Journal*, 112, March, pp. C1-C33.

Hicks, John (1939) 'The Foundations of Welfare Economics', *Economic Journal*, 49(4), December, pp. 696–712.

Hodgson, Geoffrey M. (2003) 'The Hidden Persuaders: Institutions and Individuals in Economic Theory', *Cambridge Journal of Economics*, 27(2), March, pp. 159-75.

Hodgson, Geoffrey M. (2004) *The Evolution of Institutional Economics: Agency, Structure and Darwinism in American Institutionalism* (London and New York: Routledge).

Hodgson, Geoffrey M. (2007) 'Meanings of Methodological Individualism', *Journal of Economic Methodology* 14(2), June, pp. 211-26.

Hodgson, Geoffrey M. (2019a) *Evolutionary Economics: Its Nature and Future*, in a series edited by John Foster and Jason Potts: *Cambridge Elements in Evolutionary Economics* (Cambridge and New York: Cambridge University Press).

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

Hodgson, Geoffrey M. (2020) 'How Mythical Markets Mislead Analysis: An Institutional Critique of Market Universalism', *Socio-Economic Review*, 18(4), October, pp. 1153-1174.

Kaldor, Nicholas (1939) 'Welfare Propositions in Economics and Interpersonal Comparisons of Utility', *Economic Journal*, 49(3), September, pp. 549–552.

Keynes, John Maynard (1921) *A Treatise on Probability* (London: Macmillan).

Keynes, John Maynard (1936) *The General Theory of Employment, Interest and Money* (London: Macmillan).

Keynes, John Maynard (1937) 'The General Theory of Employment', *Quarterly Journal of Economics*, 51(1), February, pp. 209-23.

Knight, Frank H. (1921) *Risk, Uncertainty and Profit* (New York: Houghton Mifflin).

List, Christian and Pettit, Philip (2011) *Group Agency: The Possibility, Design, and Status of Corporate Agents* (Oxford and New York: Oxford University Press).

Little, Ian M. D. (1950) *A Critique of Welfare Economics* (Oxford: Oxford University Press).

Loasby, Brian J. (1976) *Choice, Complexity and Ignorance: An Enquiry into Economic Theory and the Practice of Decision Making* (Cambridge: Cambridge University Press).

Lukes, Steven (1973) *Individualism* (Oxford: Basil Blackwell).

O'Neill, John (ed.) (1973) *Modes of Individualism and Collectivism* (London: Heinemann).

Robson, Arthur J. (2001a) 'The Biological Basis of Human Behavior', *Journal of Economic Literature*, 39(1), March, pp. 11-33.

Robson, Arthur J. (2001b) 'Why Would Nature Give Individuals Utility Functions?', *Journal of Political Economy*, 109(4), August, pp. 900-14.

Robson, Arthur J. (2002) 'Evolution and Human Nature', *Journal of Economic Perspectives*, 16(2), Spring, pp. 89-106.

Samuelson, Paul A. (1993) 'Altruism as a Problem Involving Group versus Individual Selection in Economics and Biology', *American Economic Review (Papers and Proceedings)*, 83(2), May, pp. 143-8.

Searle, John R. (2002) 'Collective Intentions and Actions', reprinted in Searle, John R. (2002) *Consciousness and Language* (Cambridge: Cambridge University Press), pp. 80-105.

Sen, Amartya K. (1977) 'Rational Fools: A Critique of the Behavioral Foundations of Economic Theory', *Philosophy and Public Affairs*, 6(4), pp. 317-44.

Sen, Amartya K. (1979) 'Personal Utilities and Public Judgements: Or What's Wrong with Welfare Economics', *Economic Journal*, 89(3), September, pp. 537-58.

Sen, Amartya K. (1991) 'Welfare, preference and freedom', *Journal of Econometrics*, 50(1-2), October, pp. 15-29.

Shackle, George L. S. (1972) *Epistemics and Economics: A Critique of Economic Doctrines* (Cambridge: Cambridge University Press).

Smith, Vernon L. and Wilson, Bart J. (2019) *Humanomics: Moral Sentiments and the Wealth of Nations for the Twenty-First Century* (Cambridge and New York: Cambridge University Press).

Stern, Nicholas (2007) *The Economics of Climate Change: The Stern Review* (Cambridge and New York: Cambridge University Press).

Stigler, George J. and Becker, Gary S. (1977) 'De Gustibus Non Est Disputandum', *American Economic Review*, 76(1), March, pp. 76-90.

Stiglitz, Joseph E. (2016) 'How to Restore Equitable and Sustainable Economic Growth in the United States', *American Economic Review: Papers and Proceedings*, 106(5), May, pp. 43-47.

Tuomela, Raimo (1995) *The Importance of Us: A Philosophical Study of Basic Social Notions* (Stanford, CA: Stanford University Press).

Wilson, Bart J. (2010) 'Social Preferences aren't Preferences', *Journal of Economic Behavior and Organization*, 73(1), pp. 77-82.



## Reviewer 2

Orthodox economics ends up being a vanishing set in this paper, as one has to remove any behavioural, cross-country, institutional, environmental, political economy, network.

behavioural norms literature. They need to address where norms come from, which I think is the concern of a lot of economics.

behaviour that is pro-social, for example charitable giving literature. Again, the issue is why behave in a pro-social way and diagnosing whether it really is pro-social or self-gratification of some kind.

This is, in fact, a lot of the law and economics area, as well as institutional economics.

This eliminates the innovation literature, it seems, too. I'm not sure what the difference is here, and elsewhere, between their proposal and what has already been discussed. In other words, is this review of quite well established literature or are they trying to say that they are the first ones to propose this?

adjustment to equilibrium is a good point, but this is what the modelling with paths of adjustment do - it is fine to model the path more, but if we eliminate any view of tendency of a system, regardless of how dynamic the equilibrium is, what is the notion of prediction?

overstatement. It seems that they should define the sort of problem they want to look at and then explain the techniques that they want to focus on. In other words, what are the problems for our time, what are the assumptions that we need to make and techniques we need to apply, and what are the challenges?

## Round 2

### Reviewer 1

This is one of the more difficult reviews that I have written. The reason for this is that I am torn between two sentiments. On the one hand, I fully support the objective of the paper (to rethink economics from an evolutionary perspective), I agree with more than eighty percent of what is said, and I applaud the ambition to propose an evolutionary approach to economics as an entirely new paradigm. On the other hand, I found much of the paper a frustrating read, and in places I was downright disappointed. I will start with detailing the main reasons for my frustration and disappointment. While I realize that this part of my review runs the risk of sounding overly negative, I hope that my comments will be taken in the spirit in which they are intended, which is simply to articulate the frank opinion of a sympathetic reviewer. I will finish on a more positive note by highlighting the parts of the manuscript that I did find convincing. I will also offer suggestions for improvement throughout.

My four main concerns cluster into two issues. The first is how the paper relates to economic theorizing (points 1 and 2 below). This issue is the main reason for my frustration while reading the paper. The second issue is the exposition of the paradigm itself (points 3 and 4 below). This issue is the main reason for my disappointment with the paper.

#### 1. Framing, positioning and tone

It never became very clear to me why I was reading what I was reading. In particular, what is the problem that the paper proposes to solve (and how, exactly, does it solve that problem)? The abstract and introduction did not give me much to go on. Apparently, the main objective of the paper is to propose an evolutionary paradigm that can replace the current neoclassical paradigm. That is fine as far as it goes, but why is this needed? Which problem(s) does it solve? And, in particular, what can theories grounded in this paradigm explain that economic theories grounded in the neoclassical paradigm cannot?

None of these questions are given a clear answer, neither in the introduction nor later in the text. In fact, in terms of tone the text more often than not reads like a popular treatise exposing a worldview as opposed to an academic paper written in the service of theory development, and in terms of organization the text comes across as haphazard (I was reminded of a reviewer comment that I once got myself for an early version of a similar type of paper, which was that my text read like a walk through a disorganized museum). These issues are exacerbated by the fact that the introduction apparently is meant to serve the function of not just introducing this paper, but two other (complementary) papers as well.

What I took away from the introduction was that the objective of the paper was to articulate an evolutionary paradigm for economic theorizing, and one that can – in Kuhnian fashion – replace the old neoclassical paradigm. That made me expect a line of argumentation in line with Kuhn's exposition, meaning that I was expecting that the paper would first expose the anomalies that have been

building up in the current paradigm, that it would then show how these anomalies are resolved by the new paradigm (including making clear what that paradigm entails in terms of exemplary problems and methods), and that the paper would end on showing how the old paradigm is merely a special case within the new. There are hints of the first and last point in the paper, but none of the storyline as just suggested. In fact, as already intimated above (the walk-through- the-museum comment) I did not see a storyline at all. Moreover, and this is crucial: there is little, if anything, of the second point in the text. It never becomes clear what the new paradigm solves, what it allows us to explain that the old paradigm does not, and (especially) how it does that explaining.

If, as is claimed on p. 7, the paper is written to talk to neoclassical economists, then the current set up, organization, and tone are guaranteed to make the paper ineffective. There is nothing in the introduction that will 'hook' a mainstream economist in reading onwards, the text is much too discursive, it is difficult to see where the text is going in terms of contributing to (mainstream) economic theorizing, and there is a continuous jumping back and forth between positive and normative text that mainstream economists are particularly allergic to. The overall message that mainstream economists are likely to get from the paper is that it is a 'bashing' of the neoclassical paradigm that purports to present an alternative but fails to make it clear how that alternative can help them (i.e. mainstream economists) to better explain things that they care about. Suggestions:

- Write a separate introduction for the collection of the three papers to 'unburden' the introduction of the current paper
- Rethink and narrow the objective of the paper
- Rethink the abstract. In its current form it is uninformative: it should at a minimum make clear what the paper contributes to economic theorizing
- If the objective of the paper is to articulate an alternative paradigm, then single-mindedly focus on that and leave out all unnecessary text. It should be possible to reduce the text to 40 pages when doing this
- If the focus is on articulating an alternative paradigm, then at the very minimum have a table that contrasts the new paradigm with the old
- Clearly and explicitly separate positive and normative arguments. Either have the normative arguments in a separate section of this paper, or save them for one of the two other papers.

## 2.Relation to previous literature

While my previous comment was an attempt to articulate my frustration with the paper as a general reader, my second point is meant to articulate my frustration with the paper as someone who has made a point of studying some of the previous work in economics informed by evolutionary theory. This second point is simple: the paper almost completely ignores this work.

Whether it is Veblen, Hayek, Schumpeter, Nelson & Winter, or the more recent work in evolutionary economics and institutional economics, an uninformed reader would probably come away from reading the paper with the idea that

this is the first serious work on rethinking economics from an evolutionary perspective. It is not.

I say this realizing that it is probably next to impossible to talk convincingly to both mainstream and 'heterodox' economists in one and the same paper.

Hence, I can certainly see an argument not to bother with embedding the paper's arguments in the more heterodox literature(s). But that is not what the current paper pretends to do. Instead, on p. 7 the paper claims both mainstream and heterodox economists as its audience. My strong impression is that the paper in its current form is likely to upset both audiences. Mainstream economists will get the message that their paradigm does not make any sense. Heterodox economists will be fine with this 'bashing'(that is what they have been saying all along) but will be upset that their own literature is simply ignored.

Suggestions

- Make a choice between talking to mainstream or heterodox economists
- Based on the current text, my suggestion would be not to primarily target heterodox economists, simply because the paper ignores just about everything they have already said and done
- Instead, simply acknowledge this (that the paper will not try to explicitly relate to what evolutionary economists of various stripes, orientations and convictions have already said). Then go on to articulate the evolutionary paradigm by contrasting it with the mainstream neoclassical paradigm

### 3.The Darwinian triad

I now come to the two issues that are not merely about making the paper more effective in what it purports to do, but that relate to where I fundamentally disagree with its content. These issues are fundamental and the main reason for my disappointment with the paper. The issues are fundamental because they go at the heart of what I would expect an evolutionary paradigm to do.

The way I see the argument in the current paper is that the concept of multi-level selection (MLS) extends traditional evolutionary theorizing on the basis of the Darwinian triad to multiple interconnected levels of analysis, which both allows and requires us to think in terms of a layered ontology. I wholeheartedly agree with this (and have argued as much myself in work going back some 15 years). But this also means, first, that the Darwinian triad continues to be the basic building block of any evolutionary explanation, and second, that an important part of putting the notion of MLS to work in analyzing socio-economic evolution is to specify a meaningful layered ontology. I strongly disagree with the current paper on how it treats both of these points. I start with the Darwinian triad. (I will come to the ontology in my next point.)

I was, frankly, a bit shocked by how the Darwinian v-s-r triad was presented (on pages 19-20). First of all, the text on these pages is vague and replete with questionable statements. But more importantly, there is no definition of variation, the definition of selection is simply wrong, and the definition of replication is at best questionable.

There is previous literature on what Darwin's explanatory logic is all about, for instance the work of Campbell, Lewontin, Dennett, Plotkin, and Geoffrey Smith in the wider literature, but also work in evolutionary economics (Hodgson, Knudsen, and Stoelhorst). What is said in the current paper goes against that literature. This is most clearly the case in how (economic) selection is defined: as 'the process by which economic agents adapt to changing economic circumstances'(p.20). This definition equates one of the three Darwinian mechanisms (selection) with the outcome of the entire algorithm. That is, selection is defined in terms of the outcome (adaptation) that will result from the interplay of variation, selection, and retention. In other words, the paper confuses the thing that is to be explained (adaptation) with one of the three things that helps explain it (selection).

The essence of Darwin's algorithmic explanatory logic is that it explains adaptive fit as (given enough time) the necessary outcome of the interplay of variation, selection, and retention. The text on pages 19 and 20 does not capture this at all and therefore misrepresents the basic Darwinian logic. For a paper that wants to articulate an evolutionary paradigm, that is more than worrying.

Unfortunately, this issue does not stop at the definition of selection. The statement that economic selection is artificial selection is problematic: most economic selection is the result of bottom-up competition, not top-down selection. The definition of replication as the transmission of phenotypes is equally problematic: replication is typically understood as the transmission of the information underlying a phenotype (in the case of traditional biological evolution, the genes that code for the phenotype).

Note that at least part of the issue here is similar to what I already commented on in my previous points: that the paper seems to have problems in being clear about what it explains (or what an evolutionary paradigm can help economists explain), and how that explanation works. Instead it mixes (and thus confuses) what is to be explained with what needs to do the explaining.

Suggestions:

- In order to have a viable paper, explanandum and explanans should be much more clearly and convincingly stated.

- For a reference that may be helpful in thinking this through, also because it refers to and incorporates much of the other work mentioned above, see Stoelhorst 2008, Journal of Economic Methodology.

#### 4. The multilayer ontology

The second and closely related reason for my disappointment with the paper is the layered ontology that it presents. This is the second of the core features of the new paradigm that is presented – as visualized in Figure 2 on page 21. As with the exposition of the Darwinian triad, I was unconvinced.

The 'levels and domain of selection' that are identified in Figure 2 strike me as very problematic. What does 'levels and domains' mean? And what, exactly, are the levels pictures in Figure 2 supposed to be? Are they units of selection? Or .

are they selection environments? I would suppose the latter, because it is difficult to see how, for instance, the environment could be a unit of selection. But if they are that (i.e. selection environments), then what is the idea of defining 'society' as 'the aggregate of all groups of people' (p. 22) and conceptualizing this at a higher level of analysis than 'polity', defined as 'all organized forms of institutionalized social relations' (p. 22). Surely, groups of people (like households, which the paper mentions as part of society) compete in the context of polities, with their institutionalized rules? If so, then they should be at a lower level of analysis. More generally, the text on page 21-23 seems to jump back and forth between units of selection and selection environments without clearly separating them out. The result is very confusing. For a paper that wants to make the most of MLS as a paradigm that can help economists build better theories, I would expect a clear ontology. The essence of such an ontology is a clear definition of the relevant units of selection. On an MLS logic, such units are individuals and groups (with groups, perhaps, further clustering into higher level groups). I would therefore at a minimum expect a clear description of the groups that are relevant for economic evolution, and how they stand in relation to each other.

Only after the relevant units of selection have been defined in relation to each other would it make sense to consider the selection environments that each of them faces. On an MLS logic, a key part of detailing the nature of selection pressures would be to separate out within-group competition from between-group competition. I was very surprised to find very little about within-group versus between-group competition in the paper (it is a theme on pages 7-19, but it hardly features in the rest of the paper), and more generally very little about competition as such.

Summarizing across my points 3 and 4, the essence of applying evolutionary theorizing to economics is getting (1) the application of the v-s-r logic to socio-economic systems right, and (2) being able to do so in the context of a layered ontology that offers a meaningful conceptualization of the relevant socio-economic units of selection and their multi-layered interrelationships. I was very disappointed with what the paper offers on these two counts.

Suggestions:

- The section that is currently titled 'Core features of the multilevel paradigm' struck me as the weakest part of the paper. A much more convincing statement of the paradigm itself is needed to carry a paper like this.
- One way of achieving this may be to think of the entire paper as a statement of the paradigm. For instance, surely some of the elements discussed on pages 7-14 are part of the paradigm? If so, then a table along the lines of my earlier suggestion (contrasting the current paradigm with the new one) may help to impose more structure on the arguments.

5. Other points

- The abstract defines/describes 'selection' in a different way than p. 20.
- The paper seems to focus mostly on cooperation, with some additional

attention to innovation. But what about competition? Isn't competition ultimately what drives all of evolution? (It certainly is what economists think of when they think of economics.)

-Firms get very short shrift. Yet, they play a central role in economic processes (especially in realizing economic growth) and are central to how economists think about economic systems.

-What is said about theories of the firm on p. 32 makes little sense: it is certainly NOT the case that managers are seen as dictating the behavior of firms in agency theory (where they are the agents of shareholders) and the behavioral theory of the firm (which describes firms as political constellations).

-P. 26 states that 'The multilevel paradigm requires us to rethink the purpose of economics ...'. It wasn't clear to me why this is the case (it certainly did not follow as an inescapable conclusion from the previous text). More fundamentally, I am not at all convinced that the paper really states a different purpose for economics. Much, if not all, of neoclassical economics is premised on the idea that economists study how resources can be mobilized to enhance collective welfare. How, exactly, is that different from what the current paper claims as its 'new' purpose? Moreover, it may be a tactical mistake to claim an entirely new purpose, because this gives economists a reason to neglect the paper on the argument that it is not really about their field.

-On p. 24, four 'prominent features' of the multi-level paradigm suddenly pop up (the text suggests that these features logically follow from the text about the Darwinian triad, but I failed to see how this was the case). These features confused me. How are they features of the paradigm itself? What, then, is that paradigm, exactly?? It would seem that the four things mentioned here are more a mix of normative statements about the world (with the possible exception of uncertainty). In fact, much (but not all) of the text that follows under the headings of these four features seems to be more about presenting a (normative) worldview (the exceptions are parts of the text that make more direct links to economic theorizing).

-There are large chunks of text throughout the paper that have very little or (all too often) no referencing. This is mostly text about things on which there is literature, sometimes even economic literature. One example of this is the text on uncertainty. How can you discuss this without (at the very minimum) referring to Knight?

## 6. Positive features of the paper

-As noted at the start of my review, I am all with the authors when it comes to the central importance of multi-level selection. I agree that the quote from Wilson & Wilson at the top of p. 14 could and probably should be pivotal in rethinking economics. If anything, much more can be made of systematically working out its implications for economic theorizing.

-More generally, I also like the attention that is given to the psychological foundations of the human ability to achieve cooperation (and innovation). That is something that has been given relatively little attention in evolutionary and

institutional economics. (But is a central theme in game theory and behavioral economics).

-I found pages 7-18 fairly convincing (although the text could be tightened). It seems to me that there is more here that qualifies as building blocks of the actual paradigm than on pages 19-26.

-The conceptualization of and distinction between CAS1 and CAS2 systems is especially useful. More can probably be done with systematically applying that conceptualization to socio-economic systems.

In conclusion, while I see the potential of a paper like this, I was quite disappointed by the current manuscript. There is some very serious work to be done to come to a paper that really does justice to the potential of evolutionary theorizing in general, and multi-level selection in particular, for economics. The current version of the paper is almost sure to fall flat.

Part of the necessary work is 'tactical' and simply a matter of putting more time and craftsmanship in (1) clearly positioning the argument and its value for economists and (2) better organizing and substantially pruning the text. It would also help to be more humble towards economists in general and evolutionary and institutional economists in particular to really stand a chance of convincing them.

However, another part of the necessary work is to articulate a much clearer and more precise statement of (1) what, exactly, the proposed paradigm is, (2) what it can explain that the current neoclassical paradigm cannot, (2) and how, exactly, it explains this.

The end result could (and probably should) be a much more traditional paper of around 40 pages instead of the current 77.

## Reviewer 2

I have considerable sympathy for Snower and Wilson's attempt to inject some biological reality into economics, which has for too long struggled under the illusion of a rather impoverished view of the human world. I also have sympathy for their attempt to introduce a multilevel selection environment into the discussion. However, the prospect of having to review (never mind read) three 80-page manuscripts makes me wonder why these papers aren't being bound together as a book – since, stylistically and lengthwise if nothing else, that is clearly what they are. A journal just seems the wrong place.

Broadly speaking, I don't have any great difficulty with the material in the text as it stands, providing I take a relaxed view over the definitions hidden beneath some of the concepts. I do, however, have some questions about detail in the interests of bringing evolutionary biologists on board – many of whom will otherwise baulk at the content.



1)The style seems rather casual and informal, and somewhat repetitive, resulting in a paper that is probably twice as long as it need be. For a book, that's fine. For a paper, not. If it is to stay a paper, I would recommend heavy editing to reduce its length by about a half.

2)P. 4: although the basins of attraction metaphor is technically correct, I really don't see how it helps the argument. The point is made well enough without detouring into this metaphor for the sake of a diagram.

3)P. 6, last paragraph: I am aghast at the prospect of two further articles of equal length to wade through. People don't read long papers. They are too busy.

4)P. 12, para 2: "MLS theory was widely rejected..." - no, it wasn't. It didn't exist then in any form, and had never existed as such in the preceeding century since the publication of the Origin of Species. This is completely misleading.

5)P. 12, para 2, last sentence: this is also misleading. It has nothing to do with individualism, American, economic or otherwise. It simply has to do with the logical implications of the genetic mechanisms of inheritance. It still stands correct.

6)P. 12, para 3, sentence 1: this simply isnt true. First, multilevel selection is not yet widely accepted within evolutionary (never mind any other form of) biology. Second, it misconstrues what IS moderately widely accepted as multilevel (or group-level or group augmentation) selection. Multilevel selection, as widely accepted, is the recognition that fitness contributions (selection) can accrue at different levels of the biological system, but that the cost-accounting is still done at the level of the individual/gene. We can specify the fitness of (or selection pressure on) a gene or trait, and that represents a summation of the costs and benefits of that trait, as Hamilton reminded us. What is different is the recognition that the costs derive from a variety of sources, some of which represent classic constraints on altering parts of a system (i.e. both the upfront energetic or other costs of producing a new trait plus the destabilising effects this causes elsewhere in the organism as a system) and others represent either novel costs that arise because of where the new trait puts you AND Hamiltonian opportunity costs (or regrets, as economists classically labelled them) from not pursuing alternatives.

7)P. 14, para 2: what does "functional organization" mean? MLS is explicit about the level at which cost-accounting occurs (it remains very firmly the gene); it is also explicit about the sources that contribute to that (costs incurred within the organism, costs and benefits at the individual, social group, and wider community levels, etc). The latter are not "levels of selection". Selection can only occur at the level of the gene [or meme], if that is relevant]. This seems to conflate the level at which benefits accrue with the level at which selection (the cost accounting) occurs. That confusion has been endemic in DSW's longstanding views on group selection, but it is not an accurate description of the contemporary multilevel (or group level or group augmentation) selection approach within evolutionary biology.

8)P. 15, para 3, l.1: “group-selected”: playing fast and loose with definitions is always problematic for naïve readers. The terminology needs to be clarified here. We are not group-selected by any stretch of the imagination [according to the classic, and therefore correct, definition of group selection]. We do operate within a group-level selection environment – all primates and other social mammals and birds do.

9)P. 19, l. 1-2: a new breed of historians? Who? The gentlemen listed in l.2 are by no conceivable definition historians, and Ober is a classicist and political scientist rather than a historian as such.

10)P. 39, para 2, l.1-2: I don’t think MLS theory in particular recognises the relevance of moral values as relevant, does it? General evolutionary theory does do so, because it recognises (and has always recognised) that selection does not work directly on traits (or the genes for traits) but rather acts indirectly on them via the motivational and physiological mechanisms that underpin the trait. There are very few behavioural traits that are directly genetically determined – anatomy being the only relevant examples. In advanced organisms, there are very few neural traits that act in so direct a way that they are selected for like eye colour.

11)P. 50, para 1: I hate to defend economists, but there is an issue of scale here. In both biology and economics, the details of individual differences become simple error variance when we abstract far enough up the hierarchy. Seen as a population of communities, the individual is mere froth on the surface in population ecology and species evolution. By the same token, individual differences disappear when we look at pricing decisions, manufacturing processes and national economics. This is simply reflective of the structure of science. Yes, we can describe everything we see in the world in terms of quantum physics, but as many philosophers of science have pointed out over the decades, it is pointless to do so. We don’t have computers big enough to do the calculations, and our minds are too limited to comprehend the models anyway! Moreover, there is a sense in which the emergent properties at different levels work well enough as approximations on their own. Hence we have chemistry, biochemistry, biology and ecology, etc as viable disciplines. It doesn’t make sense to try and convert the whole of chemistry to quantum physics, even though in theory it is possible. NASA could have done all the calculations to get Apollo 13 back using the moon as a slingshot using quantum physics; they didn’t, instead they used Newtonian physics, despite Newtonian physics only being an approximation – because it’s an approximation that works well enough at the scale of the earth-moon planetary system (you won’t be more than a few centimeters out).

12)P. 50, para 2, sentence 2: this may well be true of labour relations, but of pricing or production line input/output? Surely not?!

13)P.67, para 2, sentence 1: I’m worried about definitional slippage here. Is this multilevel selection [the between grouping levels in which the organism is embedded] or the multidimensionality of fitness? They are two different things.

14) P. 68, para 3, l.1-2: there has been an emphasis throughout on cooperation in human societies. That's an economists way of looking at human sociality – everything is trading. Cooperation is an outcome of living in groups, and has nothing to do with why groups form [in primates or humans]. Group-living evolves for coordination, which is an entirely different issue and one not subject to the public good dilemma problem that has bedevilled attempts to provide both economic and evolutionary explanations for the evolution of cooperation. Once you live in groups, the group determines who you cooperate with and who you don't cooperate with. Cooperation is not an all or nothing trait.

15) P. 72, para 2, sentence 1: I suspect there is a confusion here between the default position [where we would be if the species hadn't evolved some solutions to the problem we are interested in or where we would revert to if we lost the selection pressure favouring those solutions] and the situation as we have it now [after the species has evolved these solutions]. The first is a common tactic in reverse engineering modelling [which most evolutionary models are really a form of].

### Reviewer 3

“Our exercise of agency involves seeking solutions to ongoing challenges and choosing a course of action in the light of envisaged outcomes. In this exercise of agency, we are driven by our bodily and psychic needs and constrained by our physical and social environment” – A bit trivial as a sentence, no?

“This exercise of agency can occur at various levels of functional organization. Like individuals, social groups face ongoing collective-action challenges calling for collective responses. Participation in the collective involves participating in the social process of finding solutions to these challenges and choosing a course of action from these solutions. The resulting collective decisions constrain the individuals belonging to the collective.” – Again, many words to say something obvious. More importantly, so what? Except for the reference to a broader set of thinking/perceptual channels and allowing for more complex “constructions of the world” than traditional approaches, what does it concretely imply conducting analysis very differently than what a more traditional economist would do? You ABSOLUTELY NEED a running example or two.

“A theoretical framework for understanding this epistemological framework has been articulated by Denis and Raymond Noble and George Ellis (see, for example, Noble, 2012; Noble & Noble, 2020; Ellis, 2016; Ellis & Noble, 2021. They represent organisms as nested within higher levels of functional organization, just as organs are nested within organisms, tissues within organs, cells within tissues, and so on, down to the subatomic particles. Within these nested systems, higher levels constrain the dynamics of lower levels (downward

causation), while each level “harnesses stochasticity” in terms of influencing its dynamics through the conscious or unconscious choice among alternatives. At the level of the individual, this involves the application of perception, attention, memory and values to the choice among feasible courses of action...” - you could take this paragraph and apply it without any problems to traditional models of hierarchies or statistical models of organisations a la Sah-Stiglitz or to neural network models, all very mainstream approaches. So, again, what is so very different? Examples and implications of these examples, please.

“At the level of the immune system, it involves sensing an antigen invasion, triggering hyper-mutation in a fraction of the genome, sensing the correctness or incorrectness of the outcome, and reproducing an effective antibody to the antigen.” - I have simply no idea of the relevance of this last sentence for social analysis. I am not trying to be dense but, frankly, you are doing a terrible job of communicating what is new and attractive in the approach that you propose. Of course, one element that is needed is the discussion not just of how your approach differs but what results it leads to and how useful these are compared to the results obtained from a more traditional approach.

“This approach permits an appreciation of the two capacities that have arguably been most important in the evolutionary success of the human species: the capacity for social cooperation (permitting cooperative social interactions among people as well as the accumulation of cultural knowledge through time) and the capacity for innovation (resting on human creativity in finding new solutions to new problems, particularly through sharing one’s knowledge with others).” - This is much clearer...and I would suggest STARTING with this difference.

“An example of chance uncertainty is the outcome of an election, after the candidates are known. The outcome of a U.S. election, just after the Democratic and Republican parties have chosen their candidates, is uncertain. We know the domain of possible outcomes, but we can’t attach a probability to these outcomes, because an election of this sort has not been run repeatedly in the past.” - This seems too simple to me. An election with these specific candidates and in this specific social and political environment has not been run in the past but this does not mean that individuals cannot get some ideas of these probabilities by evaluating the differences and similarities with the past and relying on the representation of the world that their previous experiences have created. In this sense the “do not know the probability” case does not look like a useful benchmark to me.

“Ontological uncertainty occurs when new experiences don’t fit into our existing mental models. The degree of ontological uncertainty is always relative to our current experience.” - Yes. That seems to be a crucial form of uncertainty

n your approach and it does underline the importance of understanding how these mental models get updated

“Whereas chance and domain uncertainties can be classified as “puzzles,” – things to be figured out within our existing paradigms of thought – ontological uncertainties are “mysteries,” since we are unable to resolve them with the existing paradigms. New paradigms are called for.” - What about “strategic” or, if you prefer “social” uncertainty: if the outcome depends also about the actions of others, then uncertainty about others’ mental model should also be important no?

“The new paradigm recognizes the existence of chance, domain and ontological uncertainties. This has important implications for our understanding of decision making. The concept of efficiency has limited applicability, since the presence of uncertainty make it impossible to assess whether an objective can be reached without waste. Waste can be eliminated only when the objective and the best means of achieving this objective have been identified. Instead, adaptability (the capacity to adjust to new situations), resilience (the capacity to recover readily from an array of shocks), and robustness (the ability to maintain operations and accommodate a variety of uncertain future events) become intrinsically important properties of economic decisions as well as economic policies” - This might be understood as follows. One COULD, conceptually, define a concept of efficiency conditional on informative and cognitive limitations and taking into account the important learning and social dynamics implied by your view....but it would be so complex as to be useless. It therefore make sense to move away from this “decision by decision” view of efficiency and to move to a more systemic view of efficiency, i.e to directly measure efficiency in terms of adaptability, resilience and so on.

“Acknowledging uncertainty also means recognizing that economic models are always simplified abstractions of the real world”-does anybody deny this?

“Since economic behaviors are the outcome of decisions formulated in human brains, which are continually restructuring themselves in response to lived experiences and continually responding to an ever-changing physical and social environment, we cannot assume that there are timeless “laws of nature” governing human interactions, analogous to laws of physics and chemistry. Under these uncertain conditions, we are not justified in applying a theory that works well in one sort of environment (e.g. stable times when such uncertainties are relatively small) to other environments (e.g. unstable times, such as in the aftermath of a novel pandemic).” -This is clear and interesting.

“It is impossible to escape from the small-world models that underlies our economic decisions. The small-world models are not the result of mental laziness, a fast-thinking short-cut to proper slow-thinking deliberations. In the presence of uncertainty, small-world models are all that we have access to. Consequently, it is inconceivable that our perceptions, beliefs and objectives could ever be free of social influences – just as it is inconceivable that the social forces for norms, values, identities and narratives could ever be free of our perceptions, beliefs and objectives. Our economic decisions – along with our underlying understanding of the world – are inevitably embedded within our societies, both in terms of persistent identities and cultures and of our ever-varying social relationships.” -Again, a concrete example of how we would apply this approach would be helpful.

“To address the uncertainties of the “large world” in which we live, it is wise to entertain theory-pluralism, which is meant to engender “cogni-diversity,” namely, the recognition that multiple, mutually incompatible mental schemata may be relevant guides for our decisions in an unpredictably changing world. Such cogni-diversity plays an analogous role to that of biodiversity in ecological ecosystems. The greater the diversity of mutually incompatible theories” -Why do they have to be mutually incompatible? Actually, how would we even know whether or not they are incompatible? Isn't it enough that they are substantially different?

“In the presence of uncertainty, the fundamental economic concept of “equilibrium” – a state in which there is no tendency for changed behavior – becomes irrelevant as a description of the “large world.” Even if we fully understood the full physical environment in which we live – which is inherently unknowable – we could still not be confident that people have no tendency to change their behavior, since people are continually learning and innovating and continually interacting with one another in new ways.” Good point.

## Reviewer 4

### OVERALL SUMMARY

This remains a highly ambitious and potentially very important article. It is also a significant improvement on the previous version, with a better structure and some of the problematic arguments removed. In my previous report I wrote:

“In several places the current paper falls below scholarly standards. Much of it is unclear. Crucial key terms are often undefined. There is a general failure to cite and build on previous work. At least a few references are required to back up and lend some authority to each argument. Without such references, alongside perceptions of overblown claims of novelty, the paper will not be taken seriously.”

Despite significant improvements in the current text, parts of the paper still fall below the scholarly academic standards for a reputable journal. Several of my previous recommendations have not been followed.

The current version of the paper has been (partly) refereed, with reports from three credible reviewers. These reports raise further issues that require major attention.

This report is structured on a topic-by-topic basis. I shall cite some selected points from the three solicited reviews. I shall also refer, where appropriate, to my December 2021 report ("Hodgson 2021"). It is acknowledged that several of the issues raised in this 2021 report have been dealt with. But some important ones have not.

This document is not intended to be a complete summary of all the points made by the aforementioned referees. The authors should take account of other points in their reports that are not highlighted here. A purpose of highlighting a selection of important issues is to ask the authors how they plan to respond to them in their next draft. The relevant points are grouped together in four sections.

There are a number of other points that are mentioned elsewhere (in current or previous reports) that need attention. For example, the idea that a paradigm can be understood as a basin of attraction is remote from the idea Thomas Kuhn. What he meant by paradigm cannot be summed up or extended in this way, using a simple mechanical metaphor. Paradigm shifts are not gravitational. They require revolutionary restructuring and reconstruction. They involve push much more than pull. The section (including a diagram) from the final paragraph on page 3 up to and including the sentence "What's needed is a new basin of attraction, a new configuration of ideas with its own gravitational pull" on page 4 should simply be cut. The words and diagram add nothing much of value and they unnecessarily invite controversy and rebuttal, thus issuing an unintended invitation to readers to simply stop reading on page 4.

This is just one case where cuts should be made. More than one referee points to the excessive length and over-wordiness of this paper. More effort is required by the authors on this front.

## 1. QUESTIONS CONCERNING AUDIENCE AND AIMS

This issue was raised in Hodgson 2021, and there is a response in the current draft (p. 7) where the audiences of (a) neoclassical economists, (b) heterodox economists and (c) a transdisciplinary community of scholars contributing to the multilevel paradigm, are identified. But simply naming the audience(s) is not enough. It should be followed by an attempt to understand, among other things, (i) where each audience segment of audience is coming from, (ii) what each segment has achieved that is useful for progress in this area, and (iii) what problems exist in each approach that can be resolved by the 'new paradigm'. There are only minimal indications in the paper of such understandings and routes to engagement.

Referee 1 acknowledges the mention of a), (b) and (c), but writes that the paper's current

“organization[,] and tone are guaranteed to make the paper ineffective. There is nothing in the introduction that will ‘hook’ a mainstream economist in reading onwards, the text is much too discursive, it is difficult to see where the text is going in terms of contributing to (mainstream) economic theorizing, and there is a continuous jumping back and forth between positive and normative text that mainstream economists are particularly allergic to.”

Concerning (b) and (c), Referee 1 notes his own “frustration with the paper as someone who has made a point of studying some of the previous work in economics informed by evolutionary theory. This second point is simple: the paper almost completely ignores this work.” The implication is that many in groups (b) and (c) will also be dissatisfied with paper.

Referee 1 continues:

“it is probably next to impossible to talk convincingly to both mainstream and ‘heterodox’ economists in one and the same paper. Hence, I can certainly see an argument not to bother with embedding the paper's arguments in the more heterodox literature(s). But that is not what the current paper pretends to do. ... My strong impression is that the paper in its current form is likely to upset both audiences. Mainstream economists will get the message that their paradigm does not make any sense. Heterodox economists will be ... upset that their own literature is simply ignored.”

Referee 1's recommendation is to “make a choice between talking to mainstream or heterodox economists”. He adds: “Based on the current text, my suggestion would be not to primarily target heterodox economists, simply because the paper ignores just about everything they have already said and done”.

Referee 1 makes another, closely related point:

“what is the problem that the paper proposes to solve (and how, exactly, does it solve that problem)? The abstract and introduction did not give me much to go on. Apparently, the main objective of the paper is to propose an evolutionary paradigm that can replace the current neoclassical paradigm. That is fine as far as it goes, but why is this needed? Which problem(s) does it solve? And, in particular, what can theories grounded in this paradigm explain that economic theories grounded in the neoclassical paradigm cannot? None of these questions are given a clear answer, neither in the introduction nor later in the text. In fact, in terms of tone the text more often than not reads like a popular treatise exposing a worldview as opposed to an academic paper written in the service of theory development, and in terms of organization the text comes across as haphazard.”



This issue relates to the previous question concerning the choice of audience. If a scholarly audience is identified and its current state and developments are appraised, then connections may emerge between the arguments in the paper and the problems that are still pertinent in that scholarly literature.

For example, as Referee 1 suggests, “first expose the anomalies that have been building up in the current paradigm, ... then show how these anomalies are resolved by the new paradigm (including making clear what that paradigm entails in terms of exemplary problems and methods), and that the paper would end on showing how the old paradigm is merely a special case within the new.”

I ask Dennis and David how they plan to address these defects, which have been raised independently in Hodgson 2021 and in Referee 1's report. What changes do they plan to make in the light of these comments?

## 1. THE DARWINIAN TRIAD

Referee 1 makes the major claim that the elements of the Darwinian (V-S-R) triad are poorly defined. I quote the entire passage from Referee 1's report, as it is very important:

“I was, frankly, a bit shocked by how the Darwinian v-s-r triad was presented (on pages 19-20). First of all, the text on these pages is vague and replete with questionable statements. But more importantly, there is no definition of variation, the definition of selection is simply wrong, and the definition of replication is at best questionable.

There is previous literature on what Darwin's explanatory logic is all about, for instance the work of Campbell, Lewontin, Dennett, Plotkin, and [Godfrey]-Smith in the wider literature, but also work in evolutionary economics (Hodgson, Knudsen, and Referee 1). What is said in the current paper goes against that literature. This is most clearly the case in how (economic) selection is defined: as ‘the process by which economic agents adapt to changing economic circumstances’ (p.20). This definition equates one of the three Darwinian mechanisms (selection) with the outcome of the entire algorithm. That is, selection is defined in terms of the outcome (adaptation) that will result from the interplay of variation, selection, and retention. In other words, the paper confuses the thing that is to be explained (adaptation) with one of the three things that helps explain it (selection).

The essence of Darwin's algorithmic explanatory logic is that it explains adaptive fit as (given enough time) the necessary outcome of the interplay of variation, selection, and retention. The text on pages 19 and 20 does not capture this at all and therefore misrepresents the basic Darwinian logic. For a paper that wants to articulate an evolutionary paradigm, that is more than worrying.

Unfortunately, this issue does not stop at the definition of selection. The statement that economic selection is artificial selection is problematic: most economic selection is the result of bottom-up competition, not top-down selection. The definition of replication as the transmission of phenotypes is

equally problematic: replication is typically understood as the transmission of the information underlying a phenotype (in the case of traditional biological evolution, the genes that code for the phenotype).”

This criticism goes to the core issues of Darwinian evolutionary theory. Referee 1’s claim is that the account in the paper under review is unclear, misleading, flawed and ungrounded on the most recent literature on the topic. In my view, Referee 1’s criticisms here carry much weight and need to be addressed. Dennis and David, how do you respond? How would you plan to fix these major problems?

## 1. THE (VAGUE AND UNDEFINED) BOGEY OF “INDIVIDUALISM”

The treatment of “individualism” (as an alleged defect of neoclassical economics) in the paper was raised as a major problem in my previous report. As this issue is still not fixed, I must raise it again.

The previous draft of the paper defined “individualism” as the view that individuals alone, not collectives, make decisions. Part of my 2021 response to this was to show that mainstream economists, on many occasions, have assumed that collectives make decisions. Consequently, and fortunately, this definition of “individualism” has been dropped from the current draft.

But there is nothing of stature, or adequate clarity, to replace it. Yet “individualism” (whatever it means) remains a major bogey throughout the paper. “Methodological individualism” is cited up-front as a key word. But “individualism” (on its own) is used more frequently. Neither are defined clearly, and there is hardly any reference to the relevant literature.

On page 5 the authors write: “Individualism comes in many varieties (Hodgson 2007) but all of them privilege the individual organism as a fundamental unit of analysis and attempt to reduce all things social to the thoughts and actions of individuals.” This is inaccurate and misleading.

First, Hodgson (2007) does not show that “individualism comes in many varieties”. He shows that methodological individualism comes in many varieties. Hodgson (2007) does not substantiate the broader “individualism” accusation made in the current essay, because Hodgson (2007) says very little about political, ethical, epistemological or other types of individualism. As Hodgson 2021 pointed out, far better references on varieties of individualism (more broadly) would be Lukes’s book on Individualism (1973), plus O’Neill (1973) and Bunge (2000), but these suggestions have been ignored.

Could this problem be fixed by using the fuller term “methodological individualism” throughout? The first problem here would be that (as argued in Hodgson 2007) there is no consensus of opinion, among advocates or critics, on what the term means. Pushing “methodological individualism” as a major defect when there is little agreement on its meaning, is not a good strategy. The second problem is that the authors of the paper under review confuse and conflate methodological with ontological individualism. These are quite

different, as several other authors have shown. Methodology is about explanation. Ontology is about being (i.e. what does or may exist). Consider the following statements from the text under review:

(p. 5) "Individualism comes in many varieties ... but all of them privilege the individual organism as a fundamental unit of analysis and attempt to reduce all things social to the thoughts and actions of individuals."

This statement might be construed as methodological individualism (rather than just "individualism" – because it seems to be about explanation), but as such it is sorely inaccurate, and wrong.

For example, while advocating (a version of) "methodological individualism" Karl Popper (1945) wrote of "resulting from the decisions, actions, attitudes, etc., of human individuals". He did not write "reduced to". "Resulting from individuals" does not exclude social relations as additional, possible explanantia (= things that form part of the explanation). Friedrich Hayek is also widely cited as an advocate of "methodological individualism". But Hayek (1967) insisted that "relations which are essential for the existence of the whole cannot be accounted for wholly by the interaction of the [individual] parts but only by their interaction with an outside world both of the individual parts and the whole."

These examples (see fuller quotes in Hodgson 2007) show that two of the supposedly foremost advocates of the doctrine did not reduce explanations sole to individuals.

The authors also write:

(p. 5) methodological individualism, in "evolutionary biology, it [is?] manifested as the theory of individuals maximizing the fitness of their selfish genes."

As an alleged claim in biology, this is ontological not methodological. It is a statement about being, not about explanation. If it were altered to: "in evolutionary biology, it is manifested as explanations of outcomes (solely?) in terms of individuals maximizing the fitness of their selfish genes", then it would be methodological. But it would be an odd form of individualism. Within it the genes are alleged to be the ultimate explanantia, not the individuals.

(pp. 5-6) Further examples of "individualism" here include Gekko on "greed is good". This is not a methodological statement, and it is not necessarily individualistic. It could be expanded to "corporate greed is good", and would not thus apply foremost to human individuals. Statements about individuals being greedy are ontological, not methodological. Margaret Thatcher's "no such thing as society" is ontological not methodological. The full statement is not 100% individualistic either, as she added "there are individual men and women, and there are families."

Could the focus instead be on ontological individualism? This might have some traction, but the problem here, as Kenneth Arrow (1994, AER) pointed out, is that all mainstream economists assume some kinds of social relations, even if they do not put enough stress on them. If ontological individualism means that socio-economic reality consists of individuals only, then mainstream economists

are not strictly ontological individualists. Albeit too often poorly articulated, mainstream economics includes social relations, e.g. how agents relate to one another, how they exchange information, how they form expectations on the basis of the behaviour of others, and so on.

There is not clear definition of either individualism or methodological individualism in the paper. It ignores over 99% of the literature on the topic. This issue must be fixed, in a scholarly manner, or dropped.

In Hodgson 2021 I suggested that instead of “individualism” the targeted assumption might be fixed preference functions. (But NB: not all mainstream economists assume these.) The whole idea of basing economics on utility maximization and preferences can be criticized. I advised that use could be made of the work of Amartya Sen. All this advice has been overlooked. What we have instead does not pass muster.

Dennis and David, what is your view on what needs to be done here? It is a very major issue, and it needs to be sorted.

## 1. UNCERTAINTY

In Hodgson 2021 I referred to the “Ignorance as uncertainty” subheading, and what followed. (This subheading now appears on page 51.) I wrote:

“There is no reference in the paper to the distinction between risk and uncertainty, made by Knight (1921) and Keynes (1921, 1936, 1937). Following them, it is important to distinguish between

(a) risk (where, by definition, it is possible to calculate a probability by some method),

(b) uncertainty (in the Knight/Keynes sense, where it is impossible to calculate a probability), and

(c) ignorance.”

“Typically, orthodox economists ignore (b), and they sometimes confusingly describe risk (a) as uncertainty. They ignore (b) because it is difficult to model (but see Brady and Arthmar, 2012). But it is not true that they always ignore (c), or generally reduce it to (a) or (b). There are many orthodox models where agents suffer from a degree of ignorance (c). Game theory has plenty of such models.”

“The authors seem to introduce non-standard meanings here, and this should be avoided. This is also unclear later in the paper, where these terms are used in confused ways (see below). Hence, instead of “ignorance as risk”, which is unclear and confusing, it would be better (if at all) to address more broadly an orthodox “failure to address chronic information problems, such as ignorance and uncertainty”. The paper under review does not address the relevant literature on this topic, of which Knight (1921), Keynes (1921, 1936, 1937), Shackle (1972) and Loasby (1976) are but a small part.”

Ignoring the advice to build on (or at least contrast with) previous extensive work in this area, the present paper invents a seemingly novel taxonomy of “types of uncertainty” (p. 54). (The text says “four types of uncertainty”: only

three are enumerated.) Non-standard meanings are again introduced. There is no attempt to ground these on the previous literature. For a scholarly academic journal, this is unacceptable.

There are further problems: “Ontological uncertainty occurs when new experiences don’t fit into our existing mental models.” That is not an ontological issue. It is more a question of knowledge and understanding, which would be epistemological rather than ontological. This illustrates the kind of difficulty encountered when taxonomies are invented without reference to previous work.

Pages 51-8 of the current draft contain some important points, for which referee 3 has provided some detailed comments, but the most valuable can be made without inventing a new taxonomy of uncertainty.

The criticism of much of economic orthodoxy here is largely that it rules out uncertainty in the Knight-Keynes sense, i.e. it wrongly assumes that probabilities are always calculable. This point has been made many times before. It could simply be cited as a criticism, to then move on.

Other points could be included, using standard terms, such as,

(i) that much of social evolution involves uncertainty in the Knight-Keynes sense (i.e. probabilities cannot be calculated by agents),

(ii) that much of social evolution involves ignorance concerning much relevant information, and

(iii) much of social evolution presents agents with severe epistemic and cognitive difficulties in acquiring, processing and utilising information,

All these points can be made by using standard terms, without inventing new meanings for old words. All this has been said before. The current text (pp. 51-8) could then be cut by 60% even with the insertion of much-needed references. As they stand, these pages are over-wordy and they do not meet scholarly standards.

Dennis and David, how do you respond? How would you plan to fix this?

## Round 4

### Reviewer 1

tPaper I.

This reads much better and more succinctly than I remember the original doing. It’s a manageable length and does a decent both of summarising evolutionary theory and contextualising it into economics.

A few observations:

P. 2, Abstract: the last sentence reads oddly and rather out of the blue. I pick up an interesting sounding article to read only to be told don’t bother to read it because the really important part of the story will be given somewhere else. I would insert a mention on the first line that this is the first of a set of three:

“This article is the first of a set of three that seeks to establish a new paradigm.... Our aim in this first paper is to ....[set up the foundations]. We contrast multilevel selection.....”

And delete the last sentence. The Abstract should be about THIS paper and stand alone.

p. 3, para 2: Worthy though Hodgson is of being cited on all occasions, I wonder about the credit-worthiness of citing a podcast.

P. 7, last para: surely the point is that this is both a physical system that can be analysed as a physical system [how its bits fit together] AND a functional system (what it is designed to achieve)? It's not either/or but [A] vs [A+B].

p. 10, l.2-3: in the context of evolution the last phrase [“how well the regime functions for the common good”] does nothing except strike fear and trembling into the heart of any evolutionary biologist. Nothing can do this in biology. Strike it out. It is exactly this kind of sloppy wording that gets you a bad name!

p. 13, para 1: MLS was never rejected in the 1960s. It did not exist. What sociobiology was a reaction against was the sloppy evolutionary thinking of group selection, which is something completely different and which remains, to this day, an impossible evolutionary mechanism. You confuse MLS with group selection at your peril.

p. 13, para 2, l.1-2: it is a gross exaggeration to suggest that “Today, there is widespread acknowledgement that MLS theory's nested series of fitness

“This article is the first of a set of three that seeks to establish a new paradigm.... Our aim in this first paper is to ....[set up the foundations]. We contrast multilevel selection.....”

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p. 13, para 2, l.1-2: it is a gross exaggeration to suggest that “Today, there is widespread acknowledgement that MLS theory’s nested series of fitness comparisons is a fully legitimate accounting method for evolutionary change”. This manifestly isn’t true! Most evolutionary biologists operate in a world where MLS doesn’t exist, and so don’t need it [and are deeply suspicious of it]. It is certainly widely accepted by those who work on very social species, but then it always has been. After all, it is the core to Hamilton’s concept of neighbour modulated fitness [aka inclusive fitness], but very few people use that meaningful concept.

p. 13, para 2, l.3-4: this is hopelessly confused and confusing. Inclusive fitness, evolutionary game theory and selfish gene theory are one and the same thing, NOT alternative theories for social evolution.

p. 16, l.5: there are no “group-selected species”, not even humans. There is group-level or group-augmentation selection [aka MLS], but not ‘group selection’ which means something entirely different (and no one believes in for good reason).

p. 17, l.4-5: “In a food shortage, for example, there were other people to share their food in addition to one’s own fat stores”: this reads awkwardly. It sounds like the other people are sharing my food, when what you mean is that they are allowing me (starving) to share their [surplus] food.

p. 21: the discussion of social Darwinism is rather weak. The issue was that the term and concept were invented by a political scientist [Herbert Spencer] not a biologist and has only a very vague connections with anything in evolutionary theory [it is more closely related to Eugenics, which has very little evolutionary theory in it]. Second, Darwin very firmly repudiated it as nonsense.

p. 25: I don’t often defend economists, but I might answer on their behalf by saying that economic theory is NOT a theory about individual behaviour, but a theory about the average behaviour of communities. It’s what emerges when individuals pursue their various different objectives – rather in the way that ‘collective intelligence’ emerges out of crowd behaviour in animals. It is certainly not Just-So storytelling, not least because it actually kind of works. In this sense it is description of what happens in a particular corner of the world, not a theory that predicts the behaviour of individuals.

## Part 2

188 pages of text for an article isn't reasonable. It is just too much to expect anyone to read for a paper, never mind part of a series of papers. I decline to read it. Just reading the Abstract, I suspect nothing will be lost by omitting this whole paper and just having two parts. If you want to write this much, then make it a book, not journal articles.

## Part 3

pp. 5-14 [section 2.0]: it is not obvious to me that this has anything to do with an evolutionary framework for economics. I completely agree with the content, but it should be in a management psychology book, not here. And it is too long. Just because it is about what makes groups work better doesn't mean it is about multilevel selection.

pp. 5-19 [sections 2.0 and 3.0]: could be summarised adequately in just a couple of pages. Personally, I think much of this just misses the point as to why evolutionary theory and MLS are relevant to economics. I might point out that macroeconomists would argue that MLS is exactly what they try to do when they build models of national economies – these precisely focus on the complex, layered web of inter-relationships that create natural systems.

I don't see any mention anywhere of this branch of economics [probably >50% of economics given that they manage national economies, as opposed to figuring out what price you can get away with selling soap bars!]. They might legitimately ask why evolutionary theory would change the way they think/act.

I'm beginning to feel that the approach here misses the key point, that what is at stake is the nature of the utility function that drives economic activities. Economics has always insisted this is profit or wealth [hence the title of Adam Smith's book]; evolutionary biologists would say "No, that's just a proxy that works as an approximation in one small corner of the world; the real utility function is fitness, and fitness in complex systems like human societies involves both what is good for you AND what added benefits you gain by being in a group [at a cost, of course – noting that group-living is not a matter of cooperation, but of coordination, and hence not susceptible to the public good dilemma....].

p. 27, last two paras: I don't understand the point being made here. What does phoning 311 [or not] really show us?

p. 28, para 2: why is this an example of 'convergent cultural evolution'? What does that even mean in this context?



pp. 25-41: I agree completely with all these, but my guess is that the response of most economists is going to be: And so? Why is this relevant to economists? They know this happens, but why should they care enough to change their understanding of the economic performance of these groupings? I think they would justifiably want to know why this has anything to do with an evolutionary approach.

p. 31, para 2, l. 4-5: why is it “important to acknowledge that women and slaves were excluded from democratic governance”? It’s irrelevant to the point and looks like moral cowardice in the face of wokeism. Trying to curry favour with grumpy minorities should have no place in a supposedly scientific journal.....

p. 36, l.6 from end: ....and the UN is proving to be just as much a failure as the League of Nations, isn’t it? And largely for the same reasons.

p. 36, last paragraph: what’s the point of this paragraph? Delete it.

p. 37, end: isn’t the issue here the difference between short and long term benefits, which is precisely the issue with neighbourhood modulated fitness and MLS. No one is going to be persuaded to worry about anything except self-interest unless they can see a longer term benefit [in terms of self-interest]. This is the root of the Poacher’s Dilemma Problem and the Tragedy of the Commons. Taking an MLS perspective does not in and of itself solve that dilemma because there is no meaningful utility function. In any case, in reality even the long term perspective is not that long scale: humans are just not very good at worrying about the consequences beyond their grandchildren – they are meso-term satisficers, not long term optimisers.

p. 39, l. 4: “would be evaluated with the global system in mind”: Here’s the problem. It is NOT the benefit of the global system that is the utility function!! That IS pure 1950s group selection. It will never work. And you certainly wont persuade economists, never mind evolutionary biologists!

p. 39, last line: “internal mechanisms of cooperation”: these are not ‘internal’, they are imposed by the community [ie they are external]. I think you radically misunderstand why and how cooperation occurs in primate and human societies [though you are in good company on this]. What follows is on the right lines, but doesn’t really reflect what is going on underneath.

pp. 42-49 [section 6]: I am not sure I see why this is relevant or how an evolutionary MLS perspective helps us here. This whole paper seems like pitching at currently popular themes rather than trying to persuade

microeconomists why they should take evolution seriously. I wouldn't be persuaded.

In sum, I found this third paper disappointing. If I was an economist, it wouldn't persuade me to take evolution seriously because there is nothing about evolution in it. It simply points out that there are higher organisational (systems) issues that should be taken into account [but why is not clear, and I might, as a nationalist economist, stick with conventional wisdom in order to try and protect the survival of my nation state since globalisation has quite clearly been a disaster]. And it won't persuade the macroeconomists because they already know all about systems-level effects. I think it misses the real issue – that the fundamental question is a difference of opinion between economics and biology as to what the right utility function is. That is something that economists will understand and could be persuaded to think about. But this paper completely skirts round that issue.

## Reviewer 2

I have now had time to read through all three papers.

Despite my annoyance regarding the dismissal of the literature on evolutionary economics and the associated omission of relevant references, I now feel that this is a worthwhile enterprise. David Sloan Wilson has written much, usually with co-authors, about multi-level selection and ultrasociality. But most of this has been addressed to a heterodox economic audience or social scientists more broadly defined. If publishing in your journal gets this out to a more conventional economic audience that would be a significant achievement. Even generating a debate would be good since these kinds of ideas are generally ignored, rather than contested, particularly when neoclassical economics is being criticized.

I could quibble quite a lot about the way that the first paper is written but, in the kind of journal that you have, such discussion can be left for the commentaries. There is nothing seriously incorrect there, just omissions. The second paper is the one with the substantial content. It lays out the multi-level hypothesis and its implications for economics. I am familiar with most of this, but I am certain that most mainstream economists will not. And they certainly should be. The third paper is not likely to go down well with mainstream economists. It appeals to what I call a 'communitarian spirit' of the kind that Elinor Ostrom wrote about in small economic communities. There is a belief here that such a spirit can exist at a higher scale and can be mobilized to formulate and implement policies. I like this, but mainstream economists will blow many holes in it. They will protest that a 'CAS2 system' need not be Hobbesian, as depicted, but rather Smithian, and thus, will argue that markets

will emerge in which self-interested agents will generate positive social outcomes. But, perhaps, if they do make these objections, rather than none, that is what we need. And the sharper of them might well reference John Gowdy's *Ultrasocial* which covers a lot of the same ground but is much more pessimistic about the possibility of the kind of moral and ethical spread that is discussed here. However, that is no reason to argue that this should not be published. Ernest debates and arguments are urgently needed at this point in history. Something has to give.

So, in conclusion, I can support the publication of all three papers. But it also occurred to me that it might be useful to ask Ulrich Witt for his opinion. He is a past co-author with David Sloan Wilson but that would not stop him providing a frank assessment.

### Reviewer 3

I have reviewed the revised version of the three essays on rethinking the theoretical foundations of economics from an evolutionary (multi-level selection) perspective. I was (to say it mildly) surprised that there was no reply to my comments in the previous round included with the revision.

That I nevertheless reviewed the revisions is mostly because, as communicated in my previous review, I applaud the project as such.

My overall impression of the three essays is that the first and the third essay are largely convincing and effective. However, I am not convinced by the second essay. While there is relatively little that I disagree with as such (some exceptions are noted below), I (still) find this essay quite vague and very unorganized. After eighty-plus pages I was still wondering what, if anything, the text had really offered me as an actual new foundation for economic theory.

Given my differing evaluations of the three essays, the logical advice to the editor would be to publish the first and third essay – and to either reject or ask for a further revision of the second. However, the problem with this advice would be that the second essay should logically cement the overall argument and project: this essay should presumably be the pivot on which everything turns.

All in all, I gladly leave the ultimate publication decision in the hands of the editor, where it belongs.

#### Comments on Essay 1

- I found this essay generally effective
- Some textual editing is still needed
- The contrast with the 'physical' conception of neoclassical economics could be sharpened (it now pops up multiple times, but without really becoming a clear and central theme)
- If you want to use 'generalized Darwinism' as a label, then provide references

to the literature that has developed this term (especially when it is first mentioned)

-Why refer to a 'podcast'?

-Why the (exclusive) use of the term 'replication'? The common term in evolutionary economics since Nelson & Winter is 'retention'.

-Why define replication as 'transmission across generations'? That would leave out horizontal transmission of adaptive information within generations, which is included as being part of 'replication' elsewhere in the text.

## Comments in Essay 2

-How, exactly, does this essay link to the first essay?. The transition from the first to the second essay is very unconvincing.

-More generally, the intro should probably be rewritten quite substantially in order for this essay to work. In its current form, it simply does not communicate. For instance, at a minimum, formulate a clear question. The implicit question seems to be how humans are able to flexibly maintain functional organization at different levels, which links to MLS, but does not necessarily link directly to economic questions (i.e. that latter link would have to be explicitly made). Also, the organization of the essay into sections seems very haphazard.

-I found the table that compares the new paradigm with the old paradigm very, very vague ... For instance, the 'features' are not features in the sense of characteristics of the paradigm. Some are antecedents or building blocks, others are characteristics of the paradigm, and yet others consequences.

-More generally (and this is my overall problem with this essay), it never really became clear to me what the essential features of the old paradigm are for which the new paradigm offers alternatives. What do you keep from economics and what do you replace? Are you really redefining economics, or rather beginning to articulate a new type of social science? And even if what you are really doing is the latter, then what is the continuity, and what the discontinuity, with extant economics? For instance, which research questions in economics would the new paradigm answer better than the old? As I said in my earlier set of comments: if you claim a paradigm shift, then I would expect the argument to proceed along the lines of 'anomalies' and 'exemplars'.

-Along similar lines: I generally just did not have any clear sense of why I was reading what I was reading. What are the underlying questions? What, exactly, are you trying to achieve?

-What is the point of the (long) section on motives? To propose a general theory of human motivation? If so, why? It is not that what is said here is uninteresting, but much of it seems unrelated to explaining how flexible human functional organization works ... (if that is indeed the underlying question of the essay, as I suspect it should be). On page 14, I was very much wondering where all of this was going ... More generally, much of what it is discussed throughout the essay is about human nature and behavior in general, but without making

any explicit links to (1) the MLS framework, or (2) economic questions as such ...

-Another example where I was desperately asking myself where the text was going is the discussion of uncertainty and the related importance of adaptability. What, exactly, is the point here? It simply seems to be to establish that there is uncertainty and that this makes adaptability important. But do we need to spend all these pages to establish this? (Also: adaptability of who/what? How is it achieved?) In the end, the section does not really have a conclusion. In fact, this whole section seems to ultimately end up to be a (rather unconvincing) step towards arguing for theory pluralism. Do you really need to discuss uncertainty and adaptability to make that point?

-There is quite a bit of text that merely summarizes and lists previous literature. The section on uncertainty is largely like that, but so is the section on utility, preferences and well-being. What's the point, exactly? And does that point require (an attempt at) an exhaustive review? Also for the section on utility, preferences and well-being, the text there begs the question about how this relates to the earlier discussion of human motivation ... (Both sections seem to be some sort of attempt to formulate an alternative for the Homo economicus model and how traditional economics has formalized that model, but without coming to a very clear conclusion.)

-For as far as I understand the purpose of the overall text, it seems to be to critique the traditional Homo economicus model and from thereon, in one fell swoop, make the claim that a better model of human behavior (which is not specified) will change the understanding of how economies work at the macro level. That is, the main elements of essay 2 seem to be (1) human nature and (2) models of markets. That strikes me as a very strange approach for a set of essays that is about multi-level selection. Where is the link to organizational and institutional economics? What is (sorely) lacking is an analysis at the meso level of households, firms, communities, etc. This is one of the reasons why the essay strikes me as very vague: there is a continuous tendency to jump from individual-level principles (e.g. about human motivation) to the macro-level of the overall economy. But isn't that going against the very idea of the MLS paradigm?!

-I am unconvinced by the new definition of economics that is offered. In a sense, such a new definition could be seen as the proof of the pudding in articulating a new paradigm. But what is really new about the definition that is offered? The text seems to suggest that there is something new about a focus on general wellbeing, but doesn't neoclassical economics have a similar (utilitarian) focus? Also, the focus on commodities strikes me as a wrong turn ... For instance, your definition seems to leave out knowledge. If there is anything that previous evolutionary-oriented approaches have emphasized, then it is the role of (idiosyncratic, tacit, cumulative, and collective) knowledge. The same goes for the emphasis on anonymizability. This would, for instance, imply that what goes on inside a household, firms, and other organizations is not

economics? That would seem strange, and it would exclude the activities that gave rise to the term 'economics' in the first place – which derives from the Greek word for household. (This focus is also inconsistent with how 'the economy' is defined in the next section of the paper.) In fact, if anything, the definition that is offered is even narrower than Robbins' definition. But more fundamentally, it seems to be fundamentally at odds with the MLS approach itself ... On that approach, I would have expected a definition in terms of what the human ability to organize itself into flexible collectives achieves in terms of material and psychological wellbeing (or something along these lines).

-Some specific points:

-How does the paragraph on Gintis fit in? Surely, he is not part of cultural economics?!

-There is a really bad paragraph at the end of p. 32

-p.80: how does the MLS paradigm help, exactly ...?

-I found the text that makes links to identity and narrative economics fairly convincing

-My suggestion would be to impose a much stricter division between the 'positive' and 'normative' parts of the argument. Both are important, but blending them distracts from the value of each.

### Comments on Essay 3

-Why not use the term 'govern' as opposed to 'steward'? In a very important and fundamental sense, what, in contrast to neoclassical economics, you are putting central is governance. (Which reinforces my point of critique for the second Essay that you are missing the link to organizational and institutional economics.)

-p. 14 (top) has a statement of what the second essay was supposedly about ... (but I don't really recognize that in the actual set-up and text of that essay).

-Also, what are the assumptions that are mentioned there?

## Reviewer 4

### Comments on paper 1:

As you know, I have some sympathy for the view that the paper (ie, all three taken together) does not recognise sufficiently the contributions that are already out there. In my view, the paper is closer to drawing attention to a different way of approaching problems that are already recognised and with alternative solutions than to identifying a problem that people had not addressed at all. It does not do a good job at all of addressing economics research that is available and addresses the issues and situations that you discuss. Viewed from this perspective, the paper falls seriously short. The strong point of the paper is that it brings a new perspective that has value in analysing

many social science problems. People should be exposed to it and explore the framework. A case should be put forward for it, and I'm happy for the journal to do so. What needs to happen is to communicate this in a way that does not introduce the inaccuracies that appear to be present currently.

We need a clearer statement of what you're objecting to, a better way to label this model so that it is clear about what it means, and what your objections are. While you say in a brief statement in the first section that some good work exists based on neoclassical analysis, the rest of the paper tends to lump all economics into an application of a model that specifically excludes basically all the cases you analyse. By ignoring all the work that does address your cases, you exclude huge swathes of research.

There is a way to rephrase what you're doing so that it's more accurate, I believe. Let me propose some solutions, below.

(1) say up front that you are recommending that we model economics as a complex, dynamic system where everything is endogenous. That's correct, isn't it? Then add that in order to draw attention to the benefits of this modelling, you will be using an economic model that explicitly does not focus on complex dynamic systems where everything is endogenous - as a contrast. You are doing this to draw attention to your points but not as a summary of the current state of economic research. I'm saying this because you clearly don't present the relevant economic research - the paper is not any form of detailed critique of, says, the teams literature or relational contracting. Your focus is - as it should be - on presenting your model only.

(2) I would label this contrasting model as the "benchmark individualistic model" found in textbooks, not the "neoclassical model". Why? Because it looks like you're talking about economic research - which you don't review - into the issues you discuss by saying "neoclassical". As you don't review the relevant research strands, it is better not to look like you are referring to them.

(3) I think that you need to then outline the benchmark model to set the scene more clearly. I would say explicitly that the model is without any changes to incorporate externalities, group behaviour or preferences, or any form of dynamics. This could be a (short) additional section near the beginning where you say what exactly you are holding up as the reference model.

(4) Then you could say that your model will be focussed on the areas that you're excluding from the benchmark and, importantly, where all of these exclusions are dealt with at once and allowing for interactions among them. The reason to do this is that, while adjusting one element at a time might be nice from a diagnostic standpoint, it does not allow for policy-ready analysis, where there is a clear need. Your model also allows (if I understand the subtext correctly) mathematical tractability and clarity in the analysis, which is also a barrier to many changes at once to the benchmark. In essence, you claim that

your model allows something different: a clear, tractable, model that still allows many complexities, dynamic elements, and endogeneities all at once.

(5) If you are to present it this way, the question immediately arises of whether your model delivers more than a black box and what the mathematics looks like. This is not dealt with and I think it will be present enough in the minds of readers that you need to say something a bit more detailed about this. The complaint about complexity economics, despite its many advantages, tends to be that the models' "innards" are too hard to figure out. One way to illustrate this would be to contrast the approaches in a case where you already present a more standard approach (the Snower/Bosworth work, or one of the other Snower papers). That is in section II of the paper, but you could already give a sense in part I what the flavour of the answer will be. A brief review of what the mathematics looks like (ie, the style of (1) models) would be possible in the first section along with a brief discussion of why this does not end up being a black box would be sufficient.

(6) While you will deal with applications in more detail at the end, give a hint early in the paper about how you trade off the complexity and opacity of complex dynamic models against their realism. How do you determine the type of problem where the dynamic and endogenous elements are important enough that this type of modelling delivers substantial gain over the approach of searching for equilibrium in a model with significant exogenous elements? For example, are you looking at longer term problems? Governance problems? Problems where team elements stand out? Is this the case for all problems or is there a class where it is particularly important? Adding this type of discussion at the beginning, even if in brief and returned to later, could go some way to addressing model pluralism, which you advocate later.

(7) Finally, since you talk about pluralism, how should your model should be incorporated into broader analysis? How does it fit into other approaches?

In terms of tone, a paper by Bowles and Gintis in the QJE (2000) objected to the Walrasian model and, while it is not as explicit about what that model entails as I'd like before launching into its objections, it does do a reasonable job of selecting a constructive tone. As I've mentioned before, there is both an issue of ensuring that you do not build a straw man and an issue of ensuring that you do not sound overly mocking and sarcastic. I'm afraid that some of the comments do come across this way. Do have a look at their style.

Throughout paper 1 and in the other papers as well, using economics terminology for known concepts is a good way to communicate better with readers. A clear example of this in paper 1 is the concept of externalities. I'll mention other examples as I go through paper II, but please just keep this in mind as you review the details of writing.



Finally, the review of the various writers at the end of paper 1 struck me as a missed opportunity. While you go through some of what Ostrom suggests and return to her work later in the paper, the other authors are not returned to and in any case you do not review their work in enough detail to determine what of use they said. Again, I could refer to the Bowles and Gintis piece, perhaps, as an example where more information is given. Still, the quotes that you include sound grumpy at best and don't explain to someone who isn't intimately familiar with the writings. I would prefer that you remove most of this section but keep Ostrom's work and, since it is so important later, explain it in a bit more detail for later reference if you felt that you shortchanged elements of the work.

some degree, but it could be useful to have a look at Schildberg-Horisch (2018) for a recent and brief review that indicates the various directions of approach without selecting any one as better than another. From what I understand (and I'm no expert, as I said initially), this literature does not emphasise multiple group levels as the source, but since it's a recent review you should be able to look at the papers quickly and say something about your contribution here – if I'm understanding right.

At the same time, if the main point here is not the multiple levels or the externalities but rather the endogeneity, then consider whether this should be separate from the endogenous preference functions you discuss later. It could be that a single section for both might make sense.

As part of the discussion of the interplay between preferences and group identity, you consider preferences that rely on some social payoffs from a group. This is possible to incorporate into the standard model, as you point out and as Dennis' paper attempts. You might want to say more clearly at this point what your approach allows that Dennis' does not. A way to clarify the difference is, then, to outline Dennis' approach (as you do), but then go through how your approach would differ, what it would add, and what would be the same. Are the two approaches complementary or substitutes? In this example, what are the strengths of each? This would give a nice concrete example to help readers understand both how the models fit together and which types of problem would be well-covered by one versus the other. In other words develop the material on pages 28 to 29, as this would be a good example to fix thoughts.

Many terms in part II are not well defined and possibly are similar to economic concepts (and could be labelled as such to make them clearer. Morality (=prosocial preferences?), stable social relationships (=wants that are not material?), and narratives (=heuristics?), loyalty or trust-based systems (=relational contracts or gift exchange?), and the various terms are unclear. In

terms of how these can be incorporated into modelling, they can be incorporated in a relatively standard way (through arguments in the utility function and then a model generating those arguments). If the point of your discussion is that this second equation, the process generating the argument, is the strength of your model, then phrasing it as a modification of the existing approaches (again, Dennis' work could serve as a good model to build upon) that would be a great way to communicate it.

Make sure that you say things that are correct: no economic model – even your benchmark – equates wants with material wants to my knowledge and material wants get quite muddled with prosocial views in the paper. The explanation of the difficulty ranking careers needs better explanation: the problem is not that they cannot be ranked (which is straightforward in the textbook model), but rather the behavioural recommendations in the case where a tie needs to be broken. Be careful in the statements about time invariance: all markets have a time, place, and institutional component that needs to be specified to have a well-specified market at all. Preferences usually are invariant, although time variant preferences have been studied in specific contexts. Clarify what you are adding that your framework deals particularly well with. Externalities are not generally external, so I didn't really understand your comment around this.

The implications for profound uncertainty need to be developed more clearly, perhaps referring to precautionary behaviour or your later material on narratives, which seem to be adopted as rules of thumb (=heuristics?) to deal with uncertainty and complexity. Such rules of thumb would suggest that the effect of profound uncertainty on policy and behaviour is attenuated, since the rules of thumb determine behaviour, not the profound uncertainty. So this was a bit confusing. The importance of this point might best be linked to the type of problem we are modelling: when is profound uncertainty important to economic analysis (to be incorporated as, for example, precaution) and when are narratives important to include (to be incorporated as rule of thumb behaviour)? How do we deal with the costs of incorporating precautionary behaviour into policy? You could use lockdown of this cost-benefit trade-off as an example and perhaps develop this in section III, where profound uncertainty is not very explicitly considered?

Extending endogeneity to include whether the systems that we create to address challenges also tend to modify preferences is useful, but the current discussion is rather offhand. The development following educational training is not a bad example, but selection needs to be separated out more clearly from training effects. I believe that Marwell and Ames focuses on first term economics students where selection rather than training would be key, so address the evidence of training effects compared to selection more explicitly, as this has been discussed. The issue of the training falling on the prepared mind

is quite important for business school training, for example, so there is an issue of listening for what you want to hear – selection and training effects are not totally separable. Still, the issue of how we design our systems to reward longer term adaptation to more prosocial preferences is a difficult one and it would be useful to perhaps refer to section III's review here and ensure that you deal with training versus selection more rigorously in that part of the paper.

### Comments on paper 3

This section struggles to separate out the benchmark model from models of the phenomena that you discuss (for example, team and organisational structure and effects for the first part of the paper). Your objections are fully agreed by and large, but you don't explain them in a way that reflects current research into systems such as rank and yank, from what I can see. I think that the best approach for this and other examples where you are objecting to empirical implementations are to simply start with the system you are objecting to (such as rank and yank) and say how your model would modify it. Economic work on teams would also modify it, but I think that if you focus on the problem that you want to analyse and on your own solution, you don't need to address the economics literature at all. That would be much safer and would not require you to review a literature that would take you off of your main point of presenting your own model. You should discuss selection as well as training effects where they come up as this was discussed earlier and could be brought out here.

The tone gets peevish when you discuss work being marginalised as you've not really reviewed the work fully on either side of the topics you deal with in the paper. The focus should really be on your contribution and making a case about how well that works. Another paper can deal with various people whose work has or has not achieved prominence.

## Round 4

### Editor-in-Chief comments

."In this respect, they suffer from what can be called diffuse pluralism, similar to other disciplines within the human social sciences." - This ends up being a crucial concept to understand, and it's not really defined clearly in the beginning of the paper. Maybe say at the end of "other schools...integrative framework, and the neoclassical model has not fully embraced these alternative models so that their tools become standard equipment"? Is that what you mean and what distinguishes economics' situation from the situation in biology, to which you contrast it, below? In what you say below, it seems that you are making a distinction between substitutes and complements, but it is less clear

that this is what you have in mind by the end of the article. I don't think that you need to do much, but since this is really an organising principle in the chapter now, it would make sense to just add a few words of explanation here so that the distinction between economics and biology is clearer.

"With hindsight, more humility was called for. There is widespread agreement."  
- Slightly unsure of what you mean here. A few citations could be useful to illustrate the widespread agreement. Is there a review article that you could put here that summarises the literature? If you mean that other schools (which you cite above) find that the neoclassical model is not constructive, perhaps say instead that the arguments underlying other schools (such as the behavioural school) and their empirical successes suggest that neoclassical economics has not provided all the answers? Is that what you're thinking of?

"There is one clear candidate for such an integrative framework: Darwin's theory of evolution. " - Add "We argue that there is..."?

"And complex systems theorists cannot understand the nature of complex living systems without evolutionary theory." - Ah. OK. So - your view of the economics schools is that they're substitutes, not complements, and that is what distinguishes diffuse pluralism from the situation in biology, which is not diffuse pluralism? Maybe clarify this in a sentence here?

"And complex systems theorists cannot understand the nature of complex living systems without evolutionary theory." - Ah. OK. So - your view of the economics schools is that they're substitutes, not complements, and that is what distinguishes diffuse pluralism from the situation in biology, which is not diffuse pluralism? Maybe clarify this in a sentence here?

"The externalities can be coincidentally positive but they can also be harmful to other organisms and even the focal organism over the long-term." - Not sure I understand the meaning of "coincidentally" here. Do you mean "concomitantly"? I think that coincidental has two meanings, of which you mean the sense of occurring at the same time, but another word might dispel the ambiguity? Adding "positive in the short term" might foreshadow the last part of the sentence, so could be added as well? In terms of the regulatory system not addressing longer term harms, maybe explain that in a sentence as regulatory systems often are the mechanism to achieve behaviours that mimic CAS1 systems, in your terminology? Think of regulations that reduce smoking or address obesity, for example? You don't need to add much! Just explain this sentence so that someone who works in regulation could understand it.

“In classical game theory, strategies are chosen by rational actors to maximize their absolute personal utilities.” - Why do you say “absolute” here?

“A distinction is made between non-cooperative games, where individuals choose their strategies independently (such as the prisoner’s dilemma game), and cooperative games, which involve coalition formation by group members (ref). - I think that this indicates a reference to be added.

“A strategy such as ALL-DEFECT is called selfish because it gains at the expense of its partner and a strategy such as TIT-FOR-TAT is called cooperative because it helps its partner—even though the prisoner’s dilemma is called a non-cooperative game in classical game theory[9].” - I think that the distinction could be completed by adding that where Tit for Tat emerges as an equilibrium strategy in classical game theory, it is because this strategy is chosen by rational actors to maximise individual gain non-cooperatively for an infinitely repeated game, not because it is assumed. Is that what you’re getting at? Evolutionary game theory would impose such a strategy, whereas classical game theory would require it to be the result of maximising behaviour. You could add references here to (for example) tacit collusion models in classical game theory and an equivalent reference from evolutionary game theory here. That reference could also serve to direct readers for your next paragraph, which doesn’t have a reference and could use one.

“While subsequent work has included internal firm dynamics into innovation models (e.g., ref), it is difficult to implement a full Schumpeterian model with multiple levels of analysis. - To help on this, since I think I see what you’re looking for here - You might be able to just refer people to the 2015 paper from the Annual Review of Economics by Aghion, Akcigit, and Howitt. While that doesn’t really go into firm organisation (it does more about size) some of the references do, and there is a lot about selection at various levels in the reference list. If you send people there, they will likely find enough.

“Freidman also claimed that the predictive ability of neoclassical models justifies their assumptions; an argument that is undermined whenever the models cease to be predictive. - Maybe refer readers back to contexts where behavioural models tend to fare better than neoclassical models here to illustrate this point? In my area (competition policy - so I’m dealing with regulation that tries to break down cooperative behaviour), neoclassical models and classical game theory are remarkably predictive. In other areas (certain types of labour, and household models), neoclassical models are only modestly predictive - they do well at some things but not at others. So - perhaps say “for example...” following the word “predictive” and just give readers a field that is a case in point? I think that it is actually quite interesting where neoclassical models do well and where they don’t, and I expect readers would find that interesting as well to think about within your paradigm.

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