Agent-Based Modeling's Open Methodology Approach: Simulation, Reflexivity, and Abduction*

John Davis
Marquette University and University of Amsterdam

Abstract: This paper argues that agent-based modeling's innovations in method developed in terms of simulation techniques also involve an innovation in economic methodology. It shows how Epstein's generative science conception departs from conventional methodological reasoning, and employs what I term an open rather than closed approach to economic methodology associated with the roles that reflexivity, counterfactual reasoning, and abduction play in ABM. Central to this idea is that improvements in how we know something, a matter of method, determine whether we know something, a matter of methodology. The paper links this alternative view of economics and economic methodology to a social science model of economics and contrasts this with standard economics' natural science model of economics. The paper discusses what this methodological understanding implies about the concept of emergence.

Keywords: agent-based modeling, simulation, generative science, reflexivity, abduction, social science model of economics, emergence

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1. Introduction

Proponents of agent-based modeling (ABM) and agent-based computational economics (ACE) believe that these approaches employ new and different methods of investigation compared to those employed in standard economics, and also believe that employing these new methods makes it possible to reconstruct economics on a sounder basis (e.g., Gallegati and Kirman, 2012; Tesfatsion, 2006). Whether economics might rest on a 'sounder basis' is a methodological question regarding the nature of the grounds on which our explanations rest. Though 'method' and 'methodology' are often used interchangeably, method concerns the techniques, tools, and means of scientific investigation, whereas methodology concerns the philosophy of science of economics and the basis on which scientific explanations are validated and judged in regard to whether they produce knowledge. If we suppose, then, that the introduction of new methods in economics improves economic explanations, does this mean that their introduction not only improves the content of economics but also improves our understanding of economic methodology in regard to what counts as a good explanation in economics? That is, does the introduction of new methods in economics not only produce new economics but also new economic methodology?

Many, perhaps most, might answer this question in a negative way, denying that the basis on which explanations are evaluated in economics changes, and holding that economic methodology provides a given set of unchanging standards by which we evaluate all theories and approaches in economics whatever methods they employ. Our methods and tools may develop and allow improvements in economics, but the methodological grounds for evaluating economic theories do not change. This view, however, is inconsistent with the fact that historically economic methodology has undergone and continues to undergo evolution and change (Hands, 2001). We do not reason about the grounds for believing our theories in the same way today as we did in the past. The unchanging standards view is also contrary to what some proponents of ABM and ACE claim, namely, that their methods involve a new type of economic methodology (e.g., Epstein, 1999, 2006). This paper defends this latter view, and argues that economics, its methods, and economic methodology all evolve together. More specifically, it argues in the present connection that the adoption of new methods of investigation associated with ABM and ACE also advances economic methodology and our understanding of the nature of explanation in economics, thus justifying the view that these methods make it possible to reconstruct economics on a sounder basis. The grounds for this position, I will argue, are captured by the idea that improvements in *how we know something*, a matter of method, improve *whether we know something*, a matter of methodology.

The paper distinguishes the two views about the nature of economic methodology above as follows. I characterize the idea that economic methodology provides a given set of unchanging standards as *closed* approach to economic methodology, and associate it with standard economics' strict reliance on traditional inductive and deductive methodological arguments and with idea that methodology should be seen solely as epistemic evaluation. In contrast, I characterize the idea that economic methodology evolves as *open* approach to economic methodology, and associate it with the role that reflexivity, counterfactual reasoning, and abduction play in ABM and ACE and with the idea that methodology should also understood as practical activity.

To provide a further basis for this opposition, I link it to two ways of understanding economics' subject matter and consequent nature as a science. I pair the view that economic methodology provides a given, unchanging standard of evaluation with a *natural science model* of economics – the idea that economics' object of investigation is unchanging just as the laws of nature are unchanging in the natural sciences. Then I pair the view that economic methodology evolves with a *social science model* of economics – the idea that economics' social world object of investigation makes it a science that evolves together with the evolution of the world it investigates. Figure 1 summarizes my view of how standard economics and ABM/ACE economics differ in methodological terms.

Figure 1

Methodological differences between standard economics and ABM/ACE economics

	Economic methodology	Methodology approach	Science model
Standard economics	Unchanging	Closed	Natural
ABM/ACE economics	Evolves	Open	Social

Section 2 begins with an examination of simulation as a new set of methods and tools employed in ABM and ACE, and asks: how do these new methods and tools produce a new conception of economic methodology? This question is addressed by reviewing Joshua Epstein's influential argument that agent-based computational models and simulation analysis employ a distinctive 'generative' approach to economics and social science which is fundamentally different from both traditional inductive and deductive approaches to economics and social science. To explain this, I emphasize the link between *how we know something* and *whether we know something*, and argue that what is distinctive about ABM and ACE economic methodology is its character as a practice, specifically a *reflexive* practice based on counterfactual reasoning.

Section 3 discusses how recent philosophers of science understand the methodological claims in ABM/ACE and in Epstein's argument, and focuses on the innovation that simulation analysis involves in creating a role for a *how-possibly* type of explanation. Here I interpret this in terms of roles played by *abduction* and *retroductive* explanation, and argue that together with the reflexivity concept, this type of explanation provides the conceptual foundations needed for treating ABM/ACE as an open, practice-based approach to economic methodology.

In Section 4 I go on to argue that understanding ABM/ACE in this way can be shown to imply that it operates with a *social science model* of economics, which contrasts with conventional economic methodology's commitment to a *natural science model* of economics. I first distinguish these two types of science models in terms of their different views of economics' subject matter and its relation to the nature of economic investigation, and then argue that ABM/ACE's use of simulation analysis implies that it operates with a social science model of economics. To further illustrate this argument, I then map the range of activities the ABM/ACE approach involves and presupposes in Figure 2.

Section 5 turns to a controversial issue that continually arises in debates regarding the ABM/ACE approach, namely, interpretation of concept of emergence. My view is that this concept is best analyzed following a stage-setting discussion of the more basic methodological matters treated in the previous sections. This section first discusses Epstein's thinking regarding the concept's interpretation, and then links its interpretation to the idea of modeling economics as a social science rather than a natural science. Section 6 concludes the paper with remarks about the nature and role of economic methodology in economics.

2. Simulation, generative social science, and reflexivity

Simulation methods are now widely employed in science.[1] They are used in standard economics in computable general equilibrium models (CGE), but are constrained to computing equilibrium paths of macroeconomic variables. Agent-based models differ in allowing macroeconomic regularities to form in a bottom-up way as a result of agent interactions. This makes agent-based simulations 'generative' in the specific sense that, absent this top-down coordinating mechanism, the macrostructures and regularities that result are strictly the product of the agent interaction process. Yet 'generative' has meanings additional to this one. One concerns the idea of emergence, which I discuss in Section 5. The meaning I discuss here concerns whether a generative social science involves an innovation in economic methodology.

Epstein explicitly argues it does in making the case that the ABM and ACE research program constitutes a scientific advance on standard economics (Epstein, 1999, 2006).[2] Agent-based models move from given a microspecification simulation of agents and their behavior, that is, the *explanans*, to the macrostructures and regularities that result from their interaction, that is, the *explanandum*. Epstein first points out, then, that it has been shown that computational models are sufficient for generating macrostructures of interest. Second, he asserts that, once a candidate microspecification has been shown to produce a macrostructure of interest through repeated

[1] For a recent review of the philosophical literature on simulation with particular attention to the social sciences, see Grüne-Yanoff and Weirich (2010).

[2] His argument follows from his Sugarscape book with Robert Axtell (Epstein and Axtell, 1996), which in turn reflects the checkerboard models of racial segregation of Thomas Schelling (Schelling, 1971, 1978) who was a pioneer of agent-based computational modeling (albeit without the benefits of the digital computer). Epstein (1999) is republished as the first chapter of Epstein (2006)

application of a given set of agent-interaction rules, the demonstration that this macrostructure has been 'grown' from that microspecification then counts as a necessary condition for the explanation of that macrostructure. He summarizes this by saying: "If you didn't grow it, you didn't explain its emergence" (1999, p. 43), or logically, $(\forall x)\ (\neg Gx \supset \neg Ex)$.

Why should having 'grown' or generated a macrostructure from a given microspecification count as a necessary condition for its explanation? Consider conventional inductive explanations that employ statistical rules to move from data to general propositions. Those rules are applied to data and are independent of that data. We trust induction to reach general conclusions about many things, but our faith in it is independent of the relationship between any given set of data and the general propositions it supports. In contrast, macrostructures that simulations produce are derived from the agent-interaction rules that generate them, so that our general conclusions necessarily depend on – are 'grown' from – the basis from which they are derived. Consider also conventional deductive explanations. In this case, we explain particular phenomena as instances of general propositions. We also have confidence in many of our deductive explanations, but left unexplained is how we arrive at the general propositions from which we make deductions. In contrast, in simulations we can see how we arrive at general relationships, which are 'grown' from their explanation in micro-relationships.

Thus, compared to both conventional inductive and deductive explanations, what generative explanations provide is an account of how particularity and generality are linked. Accordingly, it seems fair to say that the claim that ABM and ACE research program provides a distinctive innovation in economic methodology rests on the idea that *how we know something*, the particulars of the matter, is fundamental to *whether we know something*, our general conclusions about it, and that this connectedness is absent from standard inductive and deductive economic methodology. Epstein also discusses how generative explanations include elements of inductive and deductive explanations (Epstein, 1999, pp. 43-4), but this does not alter the main point here, and I will instead emphasize something else central to simulation and this emphasis on how we know something, namely, its practical character as a process or activity.

Both inductive and deductive explanations involve a logic of explanation based on universal rules regarding relationships between particular phenomena and general propositions. Those universal rules prescribe how such explanations are made in a well-defined way to produce determinate results. In contrast, generative explanations based on simulation describe an open-ended set of activities whereby microspecifications and macrostructures are continually adjusted to one another, often in a largely experimental manner, until a macrostructure 'of interest' is produced. Moreover, since such macrostructures are often unexpected, further simulations using alternative microspecifications tied to what else might be 'of interest' typically follow, so that rather than the closed, well-defined approach inductive and deductive explanations involve, generative explanations are better characterized as an on-going activity or an investigative practice. In this respect, I claim generative explanations indeed do indeed constitute an innovation in economic methodology: they substitute the idea of explanation as a *process and* *practice* for the conventional idea that explanation is an epistemic logic subsumed under specific universal rules.

In Figure 2 in Section 4, I map the multiple different investigative dimensions and research activities that simulation and the ABM/ACE approach involve to show that generative explanations involve a whole set of practices distributed across different interacting scientific communities. This conclusion is important to contrasting the social science model of economics of ABM/ACE with the natural science model of economics of standard economics, the subject of Section 4. Here, however, my goal is to elicit the first of two main methodological concepts I argue lies behind the idea of generative explanation as a practice, namely, the concept of reflexivity – in the next section I discuss the second concept, abduction.

The concept of reflexivity in the current connection concerns feedback effects on some process that influences its performance.[3] In causal terms, feedback effects modify the underlying causal process, and potentially produce evolving pathways for social and economic processes.[4] Why reflexivity is important to the ABM/ACE approach lies in its process or activity conception of scientific explanation as a continual adjustment of microspecifications and macrostructures to one another in an effort to produce macrostructures 'of interest.' What simulation involves for researchers is a continual evaluation of how the determination of a particular set of microspecifications reflexively 'feeds forward' to produce specific macrostructures, and how the goal of producing different macrostructures 'of interest' reflexively feeds backward on a re-determination of microspecifications. Epstein points out that simulation activities continue until the repeated application of a given set of agent-interaction rules produces macrostructures 'of interest.' Thus, the investigative activity involved is intrinsically a reflexive one of assessing and re-assessing feedback effects from microspecifications to macrostructures and from the latter to the former until researchers are satisfied with some outcome.

It is important to note, then, what is distinctive about reasoning in reflexivity terms in this type of scientific explanation process. Simulation methods construct artificial worlds that resemble and imitate real worlds, and consequently they compare what *could* be the case in terms of how that artificial world is microspecified with what *could* be the case in resulting macrostructural terms *were* the real world to closely resemble the artificial one. That is, reasoning in reflexivity terms involves counterfactual reasoning, where this involves specifying conditions that could hold but are contrary to the facts regarding what is believed to hold, sometimes referred to as 'other possible worlds' reasoning. Counterfactual reasoning, then, should be contrasted with 'unconditional' reasoning that assumes existing facts as its basis, employs standard logic, and

[3] The reflexivity concept also operates in logic, for example, in connection with paradoxes of self-reference, e.g., the Cretan liar paradox, Bertrand Russell's set of all sets not members of themselves (Irvine and Deutsch, 2016), Oskar Morgenstern's (1928) prediction paradox, etc.
[4] I formally set out how feedback effects can modify main causal processes and can account for cumulative causation in Davis (2016).

assumes away 'other possible worlds.'[5] In language terms, counterfactual reasoning is expressed in the subjunctive mode while unconditional reasoning is expressed in the indicative mode.

Note, then, that standard economic methodology, which relies on conventional inductive and deductive forms of explanation, is typically expressed in the indicative mode and employs unconditional reasoning. Above I characterized such explanations as closed in virtue of their reliance of well-defined procedures of evaluation. Added to this now is that they are also closed in their avoidance of counterfactual reasoning. In contrast, generative explanations, which employ a practice conception of economic methodology, are developed around possibilities, and depend on a series of conjectured cases intended to identify macrostructures that might be produced. I characterized such explanations above as open in virtue of this practice-process conception. Added to this now is that they fundamentally depend on reflexivity and counterfactual reasoning. In fact, this conception has been suggested by recent philosophers of science in connection with the idea of *how-possibly* explanations. In the next section I turn to this view and the role I argue is played by the second main methodological concept associated with ABM and ACE approaches: abduction.

3. *How-possibly* explanations and abduction

Why have so many economists resisted adopting ABM and ACE simulation methods? One reason is that they have been trained in econometric methods and know little about programming methods. Another reason is that this would displace equilibrium as a central concept in economic explanations, and many economists find it difficult to imagine economics without that concept. An economic methodology reason is that simulation methods fall short of what many economists believe good explanations in economics ought to involve, as argued by Aki Lehtinen and Jaakko Kuorikoski (2007). On their view, ABM/ACE simulation methods are inconsistent with "the prevailing image of *understanding* among economists" that emphasizes "analytical rather than numerical exactness and adeptness to logical argumentation rather than empirical knowledge of causal mechanisms" (p. 306). Standard models in economics simplify complex economic relationships by representing them in terms of a small set of idealized relationships that are analytically tractable using standard mathematical methods. Analytical tractability, or formal exactness, in the explanations that this produces (also facilitated by the equilibrium assumption) is then valued over numerical exactness, associated with representations of the world that are less idealized and more detailed, thus more descriptive of actual economic relationships.

For Lehtinen and Kuorikoski what accordingly defines simulation in economics is "imitating an economically relevant real or possible system by creating societies of artificial agents and institutional structures" (p. 307). Imitation, then, replaces idealization, and this shifts the weight

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[5] In logic, counterfactual reasoning is associated with hypothethical or counterfactual conditionals as compared to material conditionals (cf. Arlo-Costa and Egré, 2016) and also with possible worlds reasoning and modal logic (cf., Menzel, 2016).

of explanation from formal exactness to numerical exactness. At the same time, the freedom created by giving up the equilibrium assumption constraint opens-up a simulated system's dynamics to a wide range of descriptive possibilities. Then, the activity of sorting through the multitude of new possibilities for a system's performance also gives simulation a quasi-experimental character that contrasts with emphasis on precise calculation of a specific set of outcomes in conventional modeling. Thus, Lehtinen and Kuorikoski's answer to why most economists have been reluctant to adopt ABM and ACE simulation methods is that those methods are inconsistent what they call the "*economists' perfect model*" (p. 306), one based on idealization and precise argument.[6]

Consider in this connection, then, how simulation in economics employs abduction as a third form of explanation distinct from both induction and deduction. Abduction, also retroduction, especially as associated with the thinking of Charles Sanders Peirce and pragmatism (cf. Burch, 2014), moves from an observation to the theory thought most likely to account for that observation, that is, it infers a best possible explanation of that observation. Abduction appears to resemble both induction and deduction, but neither is it the case that the theory arrived at is a generalization from evidence, nor is it the case that that the observation can be deduced from the theory inferred (because other theories might also explain the observation). By comparison with induction and deduction, then, abduction is not a closed, rule-driven form of inference with well-defined procedures but rather an open, conjectural form of inference characterized by incomplete and contestable explanations. In this respect, it clearly falls well outside standard economics methodological vision emphasizing formal exactness that Lehtinen and Kuorikoski emphasize. At the same time, the incompleteness and contestability of abductive explanations characterizes ABM/ACE modeling quite well, especially in relation to the nature of simulation as an open-ended, on-going type of investigation better seen primarily as a practical activity than as an exercise in an epistemic logic.

In particular, ABM/ACE modeling is abductive when seen as a generative social science that aims to account for how macrostructures of interest might be grown from microspecifications. Epstein's principle is: if you didn't grow it, you didn't explain it. It might also be stated: if you didn't find a specific set of microspecifications from which that macrostructure of interest can be grown, then you are not in a position to show how you have explained that macrostructure. That is, if the idea behind generative explanations is *how we know something* tells us *whether we know something*, one's problem is not so much showing that a macrostructure can be grown from a given set of microspecifications as much as determining what possible microspecifications for that macrostructure – the *how* – might even be found to exist in the first place. I characterize this focus on discovery as the special virtue of abductive explanations. The emphasis, that is, rests less on demonstrating inferential connections (to the best possible theory) and more on discovering the possible grounds for inference for whatever matter is at hand.

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[6] Also, they argue, the idealization view lines up with what many economists believe to be a fundamental objective of all science, explanatory unification, as discussed by Philip Kitcher (1993).

This, it seems fair to say, leads us to think about ABM/ACE modeling and abductive generative explanations as involving *how-possibly* type of explanations (Marchionni and Ylikoski, 2013; Ylikoski, 2014). A *how-possibly* explanation answers a conjectural *what-if* question, and so emphasizes the exploration of a subject matter rather than demonstrating that certain relationships must characterize that subject matter. Indeed, *how-possibly* explanations do not rule out other possible explanations – the problem of equifinality – but rather focus on what might be learned were a certain conjecture entertained.

Of course, the idea that we might be interested in *how-possibly* something might be the case could well strike some as substituting guesswork in science for rigorous investigation. However, this response assumes that we already largely know the main mechanisms that explain behavior in economics. It also ignores that *how-possibly* explanations of candidate behavioral mechanisms are still subject to the same testing and examination via robustness analysis that any theoretical inquiry involves (Kuorkoski, Lehtinen, and Marchionni, 2010; Marchionni and Ylikoski, 2013). Perhaps, then, the reason that some are inclined to dismiss *how-possibly* explanations is that they suppose economics has been quite successful in identifying the mechanisms that account for behavior, and thus see no need for any kind of exploratory investigation of such mechanisms. Yet the impact of bounded rationality theory on standard choice theory in economics tells us that there is still considerable space for theoretical conjectures and *how-possibly* explanations regarding behavioral mechanisms in economics. So clearly there exists a role for abduction in economics, as ABM and ACE modelers have shown.

Let me summarize the argument in this and the previous section. A generative economics/social science is different from traditional approaches in economics in that the simulation method it employs emphasizes the link between *how we know something* and *whether we know something*. This makes ABM/ACE pre-eminently a practical activity rather than simply a matter of epistemic evaluation (though the latter is certainly included in the former), and provides the basis for characterizing it as an open type of economic methodology rather than a closed one. I then argued that when we examine how simulation research is carried out, we see that the reflexive nature of the research process involved emphasizes counterfactual reasoning while the *how-possibly* character of the explanations it produces emphasizes abduction. Neither of these important methodological concepts plays a significant role in standard economic methodology. Thus, they provide the grounds for the claim that the ABM/ACE approach constitutes an innovation in economic methodology. Just as that approach involves new methods and new theories, so those new methods and theories are accompanied by new approach to economic methodology. In the following section, then, I discuss why ABM/ACE employs a social science model of economics, lay out in Figure 2 a mapping of ABM/ACE practice, and contrast this with what a natural science model of economics involves.

4. ABM/ACE and the social science versus natural science models of economics

Broadly speaking, I distinguish the natural sciences and the social sciences as follows. In the natural sciences, the development of science can influence what things actually happen in the world, but does not alter the principles or laws that explain how world works. In contrast, in the social sciences, I argue that not only does the development of science influence what things actually happen in the world, but it can also alter the principles or laws that science develops to explain how world works. Why this difference?

In the natural sciences the object of investigation, nature, is inert in the sense that it lacks a capacity for understanding science, and thus cannot change in response to the development of science. In the social sciences the object of investigation, at least where agents are involved, is active in that it possesses a capacity for understanding science, and therefore can change in response to the development of science. This capacity for understanding science, then, creates the possibility that agents fundamentally change their behavior as a result of the development of science, so that the principles or laws that social science develops need to change to explain changes in behavior. In economics, I call this view the *social science model* of economics. Of course, it is also possible that having a capacity for understanding science does not fundamentally change agents' behavior, and that the principles or laws that science develops do not change with the development of science. In economics, I call this view the *natural science model* of economics, because it treats its objects of investigation as *de facto* natural objects.

Proponents of the natural science model of economics, the dominant position in economics, often point to such things as the law of demand or the principle of comparative advantage as evidence that the principles or laws in economics do not change, but relative to natural science have comparatively little to justify a natural science model of economics. Proponents of the social science model of economics sometimes point to the idea of cumulative causation, but this conception is controversial and not well defended. In any event, a methodological argument can be made that the ABM/ACE approach employs a social science model of economics. I set out this argument in terms of interaction between the effects science is assumed to have on the world and the effects the world is assumed to have on science – a two-way causal relationship that implies that the content of economics and the world evolve together.

In regard to the effects science has on the world, first note the role that imitation plays in simulations. As emphasized by Lehtinen and Kuorikoski, simulations aim at "imitating an economically relevant real or possible system by creating societies of artificial agents and institutional structures" (p. 307). Second, note that ABM/ACE simulations often reveal unexpected links between agents' behavior and macro patterns of behavior, as for example in how Schelling with his checkerboard models of racial segregation showed that the behavior of agents with relatively mild preferences regarding neighbors can produce highly segregated neighborhoods. Should agents, then, believe that simulations often imitate the world, and should they regard the unexpected outcomes those simulations produce as undesirable, ABM/ACE researchers assume that agents will change their behavior, thus showing that science can have effects on the world.

Yet this opens the door to the reverse causality where the world affects science, since should agents change their behavior, this means that simulations of this behavior need to be revised to capture this changed behavior. Moreover, should new simulations of this changed behavior produce further unexpected results that again influence agent's behavior, then agents' behavior needs to be re-modeled again, and so on and so on, such that there is a continual two-way interaction between science and the world. The open character of ABM/ACE methodology discussed above, then, ultimately reflects the approach's social science model of economics. I emphasized that the idea that ABM/ACE methodology is open-ended is tied to the connection between how we know something and whether we know something. We can see, then, that this connection is continually being re-determined in virtue of economics' changing relation to a changing subject matter.

To further illustrate this, let us extend this two-way interaction between science and the world to the relationship between ABM/ACE research and economists. Economists, of course, are also real world agents, so if ABM/ACE research affects the behavior of real world agents and vice versa, then the same interaction should apply to ABM/ACE research and economic researchers. That is, here also there is a two-way interaction between that research and the behavior where each influences the other.

In this case, however, there is an additional dimension to consider. In the Schelling example, for convenience I said that 'agents' may change their behavior in response to what Schelling's analysis reveals, but did not disaggregate different types of agents and different types of behavioral responses. Here I add in that complication by emphasizing the multiple kinds of investigative activities that impinge on ABM/ACE research, thus disaggregating types of researchers as agents and thus types of behavioral responses, in order to exhibit a more complex set of interaction effects between researcher behavior and ABM/ACE research. Figure 2 exhibits these multiple investigative activities, and associates them with different, relatively independent research groups in the following way.

In the (1) – (2) relation, those engaged in object-oriented programming (OOP) and those engaged in ontological analysis each influence the another. OOP in economics agent-based modeling employs computational entities meant to reflect agent relationships in economics, and the nature of agency is a central concern in ontological analysis. In the case of the (2) – (3) link, OOP underlies the computational models developed by another set of researchers in micro specification terms, and the development of these models in turn influences OOP researchers. The (3) – (4) relation involves ABM/ACE researchers running simulations that produce macro regularities that are possibly of interest. However, whether a particular macro regularity is of interest depends on what many other groups of researchers know about empirical macro regularities, so (5) influences evaluations of (4), while unexpected results in the case of (4) can also stimulate empirical research in the case of (5). This (4) – (5) interaction, then, leads to re-specification of micro models, (6), which builds on previous model specifications, (3). All this occurs, moreover, in the context of theories of micro-macro relationships that many different researchers debate, or (7), and finally, micro-macro theories are influenced by researchers engaged in methodological analysis, here specifically epistemological analysis, regarding the nature of explanation in economics. (I have italicized (3), (4), and (6) to identify them as ABM/ACE activities in contrast to the others.)

Figure 2

The multiple investigative activities associated with ABM/ACE explanations

(2) Object-oriented programming	↔	(1) Ontological analysis
↕		
(3) *Micro model specification*	→	(4) *Macro regularities*
↕	←	↕
(6) *Micro model re-specification*		(5) Macro evidence
	↑	
	(7) Micro-macro theories	
	↑	
	(8) Methodological analysis	

Again, I frame the social science model account of ABM/ACE in terms of the two-way interaction between the effects ABM/ACE has on the world, now in the form of the activities of the different research groups distinguished in Figure 2, and the effects the world, that is the activities of these different research groups, has on ABM/ACE. In regard, then, to the effects ABM/ACE has on the research groups in Figure 2, to the extent that ABM/ACE produces unexpected applications and uses of the activities of these groups, it is fair to say this may impact their research activities. Independent research groups typically have their own views of the use, significance, and interpretation of their research. Should its application in ABM/ACE show their research has new, unexpected uses, significance, and interpretation, then this may influence their future development. Consider, for example, the (4) – (5) interaction. Unexpected macro regularities resulting from ABM/ACE simulation analysis is likely to lead to new empirical investigations of macro relationships to test their scope and significance.

However, the disaggregated nature of economic activity across groups in Figure 2 adds a further dimension to how ABM/ACE influences the activities of research groups. Since many of these groups also interact with each other, changes in any one of their research activities may also influence other groups as well as subsequent modes of interaction between them. This then adds a further set of influences of ABM/ACE on research activity.

Finally, the reverse causality, where how ABM/ACE research activity, specifically the italicized (3), (4), and (6), is influenced by various other research activities that impinge upon it, should be clear. For example, much of that research is framed by micro-macro theories that economists have long debated, (7), and considerable accumulated evidence regarding macro regularities, (5). So, the influences that ABM/ACE research has on these different activities has return effects on itself – the two-way street relationship of the social science model of economics.

Of course, it could still be the case that the *de facto* natural science model of economics is correct, and that there exist deep underlying principles and laws governing behavior in economic life that the many different research activities and groups in economics all seek to identify. However, the range of different types of research activities not only distinguished here but also in economics as a whole casts doubt on this. Since different research activities generally identify different kinds of enduring relationships, practically speaking there is limited consensus across economics regarding what basic principles and laws govern behavior. Economists do speculate about deep relationships, but different schools of thinking subscribe to different views about what these relationships are. The multiplicity of such views across research activities and between schools, then, makes the case that a single set of principles and laws governs economic behavior to little more than speculation.

5. Emergence in the ABM/ACE

Epstein's thinking about the meaning and usefulness of the concept of emergence reflects an ambivalence many ABM/ACE researchers seem to share. In their *Growing Artificial Societies* Epstein and Axtell defined "emergent phenomena" as "stable macroscopic patterns arising from local interaction of agents," and distanced themselves from the idea such phenomena need to be 'unexpected' or 'surprising' (1996, p. 35). Terms such as these were employed in the 1920s by British emergentist thinkers such as Samuel Alexander, C.D. Broad, and C. Lloyd Morgan who argued that emergent phenomena are 'unexpected' or 'surprising' when associated with properties of wholes (or macro properties in ABM/ACE terms) that are not deducible from their constituent properties (or micro properties in ABM/ACE terms).[7] Epstein, however, argues that for every computation there exists a corresponding logical deduction. Generative explanations consequently imply deductions, and agent-based modeling and emergentism understood in terms of non-deducibility must be incompatible (Epstein, 1999, pp. 43-4).[8] Thus, emergent phenomena are characterized only as "stable macroscopic patterns arising from local interaction of agents."

Epstein nonetheless allows that the ideas of 'unexpected' or 'surprising' still have meaning and use, and a more modest emergentism is still consistent with ABM if we avoid a logical confusion associated with the earlier use of the concept previously articulated by postwar philosophers of science. Specifically, rather than think of emergentism in terms of properties of things in the world, we ought to see the concept as applying to propositions expressed in formal languages (Hempel and Oppenheim, 1948). Then, that 'unexpected' or 'surprising' propositions about the properties of wholes that are not deducible from propositions about the properties of their constituent elements can be seen such only relative to the current state of knowledge expressed in such languages, and not seen as referring to ontological features of the world.

> Emergence is not an ontological trait inherent in some phenomena; rather it is indicative of the scope of our knowledge at a given time; thus it has no absolute, but a relative character; and what is emergent with respect to the theories available today may lose its emergent status tomorrow (Hempel and Oppenheim, 1948, p. 263).

Or in the words of Ernst Nagel: "emergent" only "baptizes our ignorance" (Nagel, 1961, p. 371), and the confusion that earlier British emergentist thinkers suffered was to think that the concept should be understood in ontological rather than in epistemological terms. Accordingly, 'unexpected' or 'surprising' are simply subjective terms, as Epstein and Axtell originally suspected, and ought to be employed only to register the state of current thinking about micro-macro relationships.

This conclusion, then, assumes that the ontological and epistemological can be strongly separated, rejects saying that the concept of emergence has ontological meaning, and restricts its meaning and use to our orientation toward the state of knowledge at any point in time. Yet this conclusion is inconsistent with seeing ABM/ACE as employing a social science model of economics. On that view, economics and the world it investigates evolve together with each influencing the other, so that what is the case ontologically is not independent of our knowledge of the world.

One motivation, then, for restricting the concept of emergence to epistemology is the discomfort many feel with the mystical connotations of its use in ontology. To say there exist genuinely emergent phenomena paradoxically suggests that some phenomena lack a basis in existing

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[7] See C.D. Broad on this in particular (Broad, 1925, p. 61).

[8] To be clear, Epstein points out that the converse does not apply: not all deductive explanations are generative, since 'if you didn't grow it, you didn't explain it.'

phenomena – an *ex nihilo* idea often associated with religion. Fair enough. However, what earlier proponents of a strong concept of emergence actually argued was that emergent (macro) phenomena are simply non-deducible from other (micro) phenomena. More specifically, they argued that pure deductive inference was unable to explain certain phenomena, which is indeed what Epstein argues in regard to generative explanations.
Epstein's central point is that one needs to be able to show how something was generated in order to explain it, and this certainly runs counter to a strong concept of emergence. His characterization of this, however, is formulated only in terms of producing adequate simulations, and ignores both the ways in which science affects and world and the world affects science together – the economics social science model idea – and also the location of simulation as an activity within a set of connected activities in economics as shown in Figure 2. When we then broaden his view in these ways, we see quite clearly that macro regularities that simulations produce are likely not deducible from their micro specifications because so many matters external to those simulations have gone into their construction and interpretation. Epstein is right to say every computation can be represented as a deductive inference. This is because this occurs after-the-fact of a successful simulation, and compacts all that went into that simulation's success into 'final' results. Yet he gives up the concept of emergence too easily by accepting the idea that the non-deducibility criterion concerns inference between propositions.

The social science model of economics, then, does support a strong concept of emergence. I distinguish, however, the way in which this follows from the Figure 2 disaggregated nature of the whole set of research activities connected to simulation research, and the way in which it follows from the basic idea that science and the world affect each other and evolve together. The former Figure 2 grounds are *ad hoc* in that non-deducibility is only a feature of the organization of science. It is possible (though unlikely) that a re-organization of science could overcome this. The latter grounds, however, provide strong reasons for emergence as non-deducibility since if science changes the world and the world changes science, one should expect this to be problematic for deductive inference, which presupposes stability in the assumptions from which we derive conclusions.

This, in fact, provides a further rationale for the idea that how we know something tells us whether we know something. In lieu of deduction and induction providing always-reliable grounds for judging what counts as knowledge, the generative science, practice-based idea of being able to show what works irrespective of its epistemological validation provides science an additional pathway. I turn, then, in the final section to brief comments regarding the idea advanced at the beginning of this paper that economic methodology evolves together with the evolution of economics and its methods.

6. Concluding comments regarding the nature of economic methodology

Economic methodology, or the philosophy of science of economics, investigates and examines the basis on which explanations in economics are validated and judged regarding whether they produce knowledge. As a meta-discourse and an important means of arbitrating developments in economics as a science, our bias is to assume, or hope, that this basis is independent of the science it evaluates. However, economic methodology being independent of what it evaluates is not a matter of its not being influenced by it. Rather, its being independent of what it evaluates is a matter of the difference between methodology's evaluative function and economics' science function (to produce knowledge about its object of investigation). Thus, economics influencing methodology does not necessarily undermine the latter, and in fact we should rather expect economic methodology to evolve as economics evolves. Or more broadly, that the methods, or techniques, tools, and means, by which economics pursues its science function evolve, causing economics to evolve, implies that how economic methodology pursues its function regarding economics must also evolve.[9]

I believe this argument stands on its own, but the deeper basis for it in this paper is the social science model of economics, which provides an understanding of the impetus for change in economics. Note that different views of why change and development occurs in economics are associated with the natural science and social science models of economics. In the natural science model, the object of investigation is essentially unchanging and does not influence economics' development. Thus, the impetus for economics' development is simply the quest for greater knowledge of a given world – arguably a classic Enlightenment ideal that arose with the eighteenth century development of natural science rather than with nineteenth century social science. In the social science model of economics, the object of investigation changes with the development of economics, and this in turn necessitates change in economics. In this case, an important impetus for economics' development is that past knowledge is always at risk of obsolescence. Rather than explain the development of science and economics in terms of an Enlightenment idealism of scientists' high motives, scientists are motivated by the potential failure of scientific theories and also potentially the need to explain new, even 'emergent' phenomena. At the same time, we know that in a continually changing world, new technologies and means of investigation often result from the changing effects of the world on science. So, the means or capacity for developing new theories also continually changes. This, it seems, is particularly true of ABM/ACE, which has developed around new simulation techniques, rising computing power and significantly larger volumes of data. It constitutes, then, an especially strong example of a science program developing together with its methods and methodology.

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[9] An example of this is the how development of econometric and experimental methods that changed empirical knowledge in economics led to a new understanding in methodology of the scope of falsification and the nature of the Duhem-Quine principle.

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