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COMPUTER SIMULATIONS IN ECONOMICS

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1. Introduction

Economics investigates the consequences of interlinked individual decisions within specific institutional frameworks such as markets. As the idealizations required to make models of interdependent decision-making under multiple constraints analytically tractable are unavoidably heroic, it would seem an obvious advantage to start with theoretically and empirically more palatable assumptions about the individuals and their institutional environment and let the computer worry about determining the consequences of those assumptions. Furthermore, many other applied fields of science routinely run computer simulations to evaluate the effects of possible interventions and policies. As the social science with most political clout, should it not be expected that virtual experiments about possible economic policies would be central to economic practice?

The financial crisis brought two key constructs necessary for the tractability of central economic model templates into critical focus: the representative agent and equilibrium. Together the ubiquitous use of the representative agent and the equilibrium assumption arguably preempted inquiries into the effects of different heterogeneities, biases, herding effects, in particular, and endogenously generated crises in general. The failure of standard economic models to foresee or (for a long time) even analyze the crisis constituted for many a fundamental crisis in economic theory (e.g., Kirman 2010, 2016; Stiglitz 2018). A natural way to implement more realistic behavioral assumptions, heterogeneities, imperfect information, interdependencies, and out-of-equilibrium dynamics is by computer simulation.

There are four main kinds of simulation: discretizations, microsimulations, Monte Carlo, and agent-based simulations (see Gilbert and Troitzsch (1999) for a detailed description of these methods). Of these, the first three are widely and increasingly used in economics, while the fourth, albeit also being used more and more, continues to face methodological resistance. Boumans (2020), however, argues that simulations are already widespread in economics and charges methodologists for lagging behind in describing the change. Most of the arguments in philosophy of science on simulation in economics concern agent-based models and Monte Carlo. The reason might relate to the context in which the different kinds of simulations are used. Discretizations are mainly used in macroeconomics, and microsimulations are used in policy design. The epistemic role of discretization qua simulation is minuscule compared to other methodological issues, and while microsimulations do raise philosophical questions, thus far these have been treated in specialized outlets.

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The lackluster reception of simulations can be contrasted with the relatively rapid spread of experimental methods, first in terms of behavioral economics (see Nagatsu, Chapter 24) and now in terms of natural and field experiments emphasized by the proponents of the credibility revolution (see Favereau, Chapter 25). Furthermore, economics is certainly an increasingly *computational* science. As the profession has grown increasingly data driven in what has become known as the credibility revolution, many undergraduate programs now put as much emphasis on learning R as they do on the basics of demand theory. The resistance to simulations, therefore, is clearly not solely due to any general methodological conservativeness in economics.

In this chapter, we first review central debates in the emerging philosophy of simulation subfield. After that, we will briefly discuss key types of simulation in economics: Monte Carlo methods, dynamic stochastic general equilibrium models, which constitute the mainstream in current macroeconomics, and the more marginal subfield of agent-based computational economics. As we will see, the general philosophical questions concerning simulation take very distinct forms in the context of economics. We argue that the methodological peculiarities present in all of these cases provide us with interesting lessons about the way in which the role of theory is conceived in economics.

2. Philosophical Questions in the Use of Simulations

Is there a specific "epistemic kind" of computer simulations with distinctive epistemic properties and problems? In a provocative opening article for a special issue of *Synthese*, Roman Frigg and Julian Reiss (2008) set the challenge for the emerging philosophy of simulation to explicate what novel philosophical issues simulations pose that are not common to all scientific modeling. Frigg and Reiss argue that the epistemological, semantic, and ontological questions pertaining to computer simulations do not differ from those concerning models in general and that simulations do not call for a "new philosophy of science." Although computer simulations certainly share many general methodological questions with analytically solvable models, the philosophical literature has delved into issues ranging from the implications of discretization (Lenhard 2019) all the way to the possible demise of anthropocentric epistemology (Humphreys 2009).

We begin by reviewing the debate on the definition of computer simulation as a way of locating putative features that might philosophically distinguish simulations from other models. Intuitively, not all computational exercises count as simulations, as some are simply, well, computations. Furthermore, not all simulations are carried out with computers. Physical analogue simulations like wind tunnels seem to have epistemological characteristics that render them distinct from ordinary physical experiments (see Trenholme 1994; Dardashti et al. 2017; Durán 2017 for accounts of analog simulations). Although the consensus seems to be that there is something distinct about simulations, not surprisingly no agreement has been reached on what exactly that distinct feature is.

2.1 What Is a Computer Simulation?

Paul Humphreys (1991) originally defined computer simulations simply as any computer-implemented method used to explore properties of mathematical models without analytical solutions. As Durán (2021) points out, this is in line with a long tradition of interpreting simulations in terms of solving intractable mathematical problems. This would tie the definition of computer simulation very closely to (1) (a set of) existing mathematical models and (2) whether it allows for closed-form solutions. The first constraint is problematic because not all computer simulations, such as cellular automata and agent-based models, are direct computational realizations of existing mathematical models. The second constraint is problematic because it is not clear why the mathematical fact of not having an analytical solution would necessarily correspond to a categorical difference in the way a model represents the modeled system.

Partly due to these concerns, Stephan Hartmann (1996) suggested that a simulation is a process imitating a process. This definition emphasizes both the representational function and the *dynamic* nature of simulations. Simulations are always simulations *of* something, and they represent something *unfolding* through time. This falls within the second major tradition of conceptualizing simulations in the sciences (Durán 2021). This definition is also applicable to physical analogue simulations more broadly. Hughes (1999), Humphreys (2004), and El Skaf and Imbert (2013), among others, have questioned whether time and dynamics are really essential to simulation. They agree on the importance of representation and the processual nature, however, and propose that simulations trace a trajectory of a (mathematical) system in its state space. Most often this is a trajectory along the time dimension, but not necessarily.

Although most simulations of *economic phenomena* include such a dynamic aspect, even this more abstract definition rules out an important class of computational techniques in economics that many consider to be simulations: Monte Carlo methods. We consider Monte Carlo methods more closely in Section 3.1.

2.2 The Epistemology of Computer Simulation

The debate about the definition of simulation is only interesting if there is an interesting and distinct "epistemic kind" to be defined. The question of the putative special epistemic status of computer simulations is motivated, on the one hand, by the similarity of simulation to experimentation and the consequent possibility of uncovering truly surprising and novel results and, on the other hand, by the seemingly unavoidable fact that computer simulations are "nothing more" than mere models with no direct causal interaction with the modeled reality.

The epistemology of computer simulations also depends on what the simulation is used for. Here, we discuss only "scientific" uses and leave out, for example, purely pedagogical functions. Yet, even this leaves a great number of rather distinct roles in economics. Computer simulations are used in forecasting (including nowcasting), the evaluation of policy scenarios, theoretical modeling, market design, and, depending on where the lines of simulation are drawn, various forms of data analysis.

Furthermore, the use of computer simulations involves several distinctive epistemological questions pertaining to the functioning of the computer simulation as a system in its own right, such as whether the program is doing what it is supposed to do (that there are no errors in the code) and whether the digitization introduces biases into the computation (e.g., truncation or rounding errors). Many of these questions belong squarely within the domain of computer science (e.g., to "stability theory"). We will not discuss this literature because it is vast and because we are not aware of any economic simulations in which verification could be done formally (see, e.g., Beisbart and Saam (2019) for some contributions to this literature). The philosophical question is whether these, or some other features of simulation, render computer simulations epistemologically distinct from analytically solvable models, analogue simulations, or experiments.

At first glance, the practice of simulation is often strikingly similar to that of "material" experimentation. Like an experiment, a simulation is first assembled and then left to run "on its own." Like an experiment, a simulation yields not conclusions but masses of raw data requiring further analysis and interpretation. Like experimentation, simulation practice involves looking for errors in the procedure, benchmarks for what kind of results ought to emerge, and so on. As Francesco Guala (2002) points out, both forms of inquiry require distinct judgments of internal and external validity. In macroeconomics, the use of simulations is often characterized as numerical experimentation, and Reiss (2011) even defends the use of simulations in economics by arguing that they should be used because they provide experimental data that are otherwise lacking in economics (see Norton and Suppe (2001) for a similar argument in climate science). Accordingly, Winsberg (2001, 2003, 2010) argued that the epistemology of simulation is motley and that it is self-vindicating in the same way as that of the experimental sciences.

Guala (2002) formulated what has become known as the *materiality thesis*: simulations are inferior to experiments in that the former cannot experiment with the material basis of the target system itself. Parker (2009) challenged the materiality thesis by arguing that what is relevant for the epistemic evaluation of simulations is relevant similarity and that material similarity is not always the relevant kind of similarity (see also Winsberg 2009). When formal similarity is relevant instead of material, simulations and experiments seem to be on a par. Parker also argued that the wider epistemic unit of simulation study, which includes all of the practices around constructing the simulation model, counts as a material experiment. A computer running a program is a physical system, and simulations thus are also experiments on a physical system.

Nevertheless, even if the user is experimenting *on* this physical system, the user is not running an experiment *about* the physical system (Durán 2013, 2018: 64–68). Norton and Suppe (2001) have argued that because, in running the simulation, the computer can *realize* the same abstract structure as the target system, it can function as an experimental stand-in for its target in a very concrete sense. This claim, however, seems questionable as at least all digital simulation essentially involves numerical approximation and numerous individual runs, rather than direct realization of mathematical structures (Imbert 2017).

Morrison (2009, 2015) argues that because simulations are in fact used as measurement devices, they should also be counted as experiments. Morrison, as well as Massimi and Bhimji (2015), provides examples of measurements in physics in which the data are practically impossible to interpret without simulation. Measurement often requires simulation in turning the raw data into a data model usable for making inferences about the target (see also Durán 2013). Morrison does not need to commit to a questionable view of measurement that does not require a direct physical causal connection to the measured quantity to make this epistemic claim, however. We will discuss simulations in the analysis of data later.

If the materiality of the computer and the surface features of the experimental praxis are irrelevant to the "deep epistemology" of simulation, perhaps simulations are more analogous to thought experiments or even should be conceived as computerized arguments? Claus Beisbart and John Norton (Beisbart and Norton 2012; Beisbart 2018) argue that computer simulations are simply computer-aided arguments: a single execution of a command by the computer is a logical inference step, and even a run of a complex simulation program therefore is nothing more than a massively long series of such steps. In this sense, a simulation (run) is a computerized argument. Even if the praxis of using, say, Monte Carlo techniques has many experiment-like features from the standpoint of the user, it does not invalidate this basic ontological and epistemological point. Simulation is computerized inference, not experimentation.

If this is correct, then this limits the epistemic reach of simulations. As Beisbart (2018) claims, "Simulations implement a model that entails the results of the simulation. The simulations can thus not refute these very assumptions. Nor can they refute other assumptions, unless the model of the simulations has independent support" (p. 191). For Beisbart, simulations are *overcontrolled*, meaning that there is nothing left for nature to say because all of the relevant causal information is already in the computer code. Morgan (2002, 2003, 2005; see also Giere 2009) puts this in the following way: that although simulations can surprise just like experiments, they can never *confound*. In other words, given that the computer simulation can only take into account causal factors that are explicitly written in the programming code, it cannot take into account confounders, namely, causal factors that have an effect on the phenomenon of interest but that the experimenter does not know about or does not know that they have such effects.

There are, in fact, two interrelated philosophical questions in the discussion that compares experiments and simulations. (1) Are simulations to be evaluated epistemically as experiments, expressions of theory, arguments, a new kind of mathematics, or *sui generis*? (2) Are simulations epistemically on a par with experiments? It is clear that even if the practice of simulation were admittedly comparable to experimentation, this would not yet be sufficient for giving simulations the probative force of experiments. What is the purpose of asking whether simulations and experiments are "on a par"? Is it to provide guidance for scientists in choosing between these methods? It is commonplace that simulations are heavily used especially when other methods cannot be employed in the first place. Roush (2019) argues, however, that this is irrelevant:

> That there are questions for which the simulation we are able to do is more reliable than any experiment we can do gives no reason to deny the superiority of a comparable experiment that we cannot, or cannot yet, do.
>
> (p. 4889)

One must thus ask, what is the relevant epistemic situation in which one can compare them? The epistemic situation determines what the researchers know about the world and the applicability of their methods (Achinstein 2001). Roush (2019) argues that, in comparing the two, one must start from an epistemic situation in which one has a given question about the actual world to which one does not know the answer. One is then asked to make a choice, and Roush argues that one should choose an experiment if the correct answer is determined in part by something one does not know. If the arguments for the epistemic superiority of experiments are to have a normative punch, one would have to come up with circumstances in which it is possible to conduct both an experiment and a simulation (see also Parke 2014).

The problem is that, despite the barrage of arguments for the superiority of experiments, it is very difficult to apply such arguments to actual cases from science. Perhaps the superiority question is ill-posed and we should start asking different questions. However, the discussion has not been useless. The supporters of the superiority thesis have contributed by describing the various limitations to the probative force of simulations, and the defenders of simulations have shown various ways in which simulations usefully contribute to empirical research.

2.3 Epistemic Opacity and Understanding

Even if we accept that what the computer does is a long series of deductive steps and that the content of the results cannot go beyond the content of the assumptions (barring considerations of truncation, etc.), to treat a single run as an argument is not very enlightening from the epistemological perspective, as any such run is *epistemically opaque* to the user: the relationship between the stipulated initial conditions and the simulation result cannot be "grasped" by a cognitively unaided human being (Humphreys 2004: 147–150; Lenhard 2006). Even if the simulation results were, in some sense, already built into the program, it is impossible to deny that the results are often novel to the simulator. This brings us to the questions of novelty and understanding.

Barberousse and Vorms (2014) point out that any interesting sense of novelty in simulation results should be separated altogether from that of surprise; both computations and experiments can produce previously unknown, yet not really surprising results. Furthermore, even though what the simulation does is to "unfold" the logical content of the programmed computational model, computer simulations can clearly produce surprising novelty in terms of results that are *qualitatively* different from the assumptions of the computational model (El Skaf and Imbert 2013). Many agent-based models especially produce phenomena that are, in some sense, *emergent* relative to the model assumptions. Paul Humphreys (2004) has defined such qualitative novelty as conceptual emergence: a result R is conceptually emergent relative to a conceptual framework F when its adequate description or representation requires a conceptual apparatus that is not in F. Note that this characterization renders novelty to be relative to the concepts used to describe the results.

A further and distinct question from the novelty of simulation results is whether simulations can provide explanations of the modeled phenomena in the same way that analytical models do. Simulations are subject to considerably milder analytic tractability constraints and, hence, can do away with many of the distorting idealizations and tractability assumptions. However, the very fact that a simulation model can do away with such assumptions often makes the simulation model itself difficult to understand. Complex simulations can increase predictive power, but is our understanding improved by replacing a puzzling phenomenon with its puzzling simulation?

Whether simulations are somehow explanatorily special depends, to some degree, on which theory of explanation one favors. Proponents of agent-based simulations usually favor a causal-mechanistic conception of explanations and argue that social mechanisms can only be adequately modeled by agent-based models (although Grüne-Yanoff (2009) denies that agent-based simulations could provide causal-mechanistic explanations). Social simulationist Joshua Epstein crystallized the appeal of agent-based *generative* explanations in his motto, "If you didn't grow it, you didn't explain it" (1999; see Davis 2018). Even though economists are usually committed to some form ~~for a discussion~~ of methodological individualism, they have not embraced this idea of generative explanations in economics. Lehtinen and Kuorikoski (2007) argued that an important reason for mainstream economists' resistance to agent-based simulations is their implicit adherence to non-mechanistic criteria of understanding, and specifically to a conception of economic understanding in line with Kitcher's unificationist model of explanation: economic understanding is constituted by deriving results using a small set of stringent economic argument patterns (analytic model templates). In agent-based simulations, the derivation part of this activity is outsourced to the computer, and the modeling assumptions may not resemble the premises in these argument patterns.

We will next survey the most important uses of simulations in economics and the more specific philosophical questions related to them.

3. Simulations in Economics

3.1 Simulating Data and Monte Carlo

The preceding discussions have diverted attention from important differences between different kinds of simulations. Note that the vast majority of data that economists use for the purpose of testing their theories are observational, not experimental. Hence, in economics the more relevant difference is between observational data and data generated with a simulation (Reiss 2011). The distinctive methodological questions related to simulations that concern data manipulation and analysis may have important consequences for genuine substantive questions.

Let us first consider an example from macroeconomics. According to the real business cycle (RBC) theorists in the 1990s, macroeconomic fluctuations are caused by technology shocks. RBC theorists provided empirical evidence for this claim using specific data-analysis techniques. Their argument was that their vector autoregression (VAR) model fit the data better when technology shocks were added as a regressor. (See Henschen, Chapter 20, on VAR.) Hoover and Salyer (1998) used a simulation to construct simulated data sets to show that the increase in fit provided by the technology shocks was entirely due to the specific statistical methods used by the RBC theorists, irrespective of whether or not the data were generated with a process including technology shocks. The conclusion is that the specific data-analysis technique is unreliable because it seemingly indicates a statistical relationship between X and Y, even when we know that X is not there. We could not have known this by looking at empirical data because we cannot control whether the true data-generating process (DGP) contains X – this is what the empirical research tries to find out in the first place. Simulated data are thus necessary for this kind of counterfactual analysis.

Monte Carlo experiments often study the properties of statistical distributions and statistical research tools and concepts. We saw earlier why Monte Carlo simulations are so prevalent in econometrics: econometricians and statisticians are constantly developing new statistical tools for analyzing and processing data, but how do we know whether, say, a proposed estimator performs adequately? As these techniques have grown more complex and computationally taxing, in practice the only way to test such performance is to generate simulated data and then see whether the estimator finds the right characteristics of the data. One cannot use empirical data for this purpose because their DGP usually cannot be precisely known. In contrast, because the simulated DGP is created by the modeler herself, it can be used as a benchmark in evaluating the performance of estimators: overcontrol is necessary for this kind of study. (See Spanos, Chapter 29, on the philosophy of econometrics.)

As Winsberg (2015) notes, the standard definitions of simulation that emphasize mimicking a target system with a computational model do not quite do justice to Monte Carlo methods. Monte Carlo simulations often do not mimic any spatiotemporal objects or processes, and the randomness on which the method is based is not normally meant to be a claim about the object or process of interest (Beisbart and Norton 2012; Grüne-Yanoff and Weirich 2010). Grüne-Yanoff and Weirich (2010) argue that, as Monte Carlo methods lack the mimicking aspect, they should be counted as calculations not simulations.

The randomness is typically the result of using a *pseudorandom number generator* (PRNG), an algorithm that gives rise to a complicated deterministic process that produces data mimicking the distributional properties of genuine random processes. Thus, there is also a mimicking relation in Monte Carlo methods, but it holds between the generated data and a probability distribution. Nevertheless, mere mimicking should not be sufficient for distinguishing simulations from calculations, as one simply typing 5 + 7 on Matlab and producing the resulting datum 12 may be said to mimic the result of writing 5 + 7 on a piece of paper but, clearly, performing this calculation on Matlab does not count as a simulation.

Is the question of whether Monte Carlo should be considered a simulation merely a terminological one? One way to consider what is relevant with definitions is to determine which aspects of a method are epistemically important. The aforementioned mimicking relation between the data generated by a Monte Carlo method and the distributional properties of a mathematical object is only relevant if it fails, and it can fail only in those cases in which the difference between genuinely random and pseudorandom numbers makes a difference. Monte Carlo methods are quite multifaceted, however, in that what is being mimicked with a model that embeds the PRNG varies. The mimicandum can be at least a spatiotemporal target system (see Galison 1996), a data-generating process for a variable, the distributional properties of empirical data, and a mathematical function (as in most applications of Markov Chain Monte Carlo). This suggests that some applications of Monte Carlo should be counted as simulations, whereas some are mere calculations.

We propose that the relevant difference lies in whether the correct performance of the PRNG along with the other deductive capabilities of the computer is sufficient for the epistemic evaluation of the model. If it is, the method counts as a computation. If one also needs to consider whether the *model* that embeds the PRNG correctly mimics its target, the method counts as simulation. Here we are using the word "target" to mean whatever a model represents. Consider, for example, a Monte Carlo study that endeavors to model a data-generating process that in reality contains a systematic bias. The model generates simulated data by using a description of the data-generating process that uses a normal distribution from a PRNG. Clearly, it is not sufficient to provide epistemic justification to this model by appealing to the fact that its PRNG correctly mimics the normal distribution. One also has to consider whether the bias as well as other aspects of the DGP is correctly modeled.

This is not the case with Monte Carlo methods that count as computation. Consider, for example, calculation of the value of π with a Monte Carlo method that uses the uniform distribution (see, e.g., Grüne-Yanoff and Weirich (2010) for a simple description). The correctness of this calculation depends on how precise the calculation must be. If it does not need to be very precise, the epistemic performance is guaranteed as long as the pseudorandom numbers from the PRNG are not too far from the underlying analytic distribution and the computer is not making any mistakes in the calculation.

3.2 DSGE

Whereas Monte Carlo methods were argued to be the most prevalent form of simulation in economics, dynamic stochastic general equilibrium (DSGE) models are arguably the most influential ones, in terms of both theory and policy. They are also controversial, and, as with Monte Carlo, their popularity is accompanied by an insistence on viewing them as simply computational methods and downplaying their role as true simulations.

The DSGE modeling strategy stems from combining Kydland and Prescott's (1982) real business cycle model with added New Keynesian assumptions about wage setting, imperfect competition, and sticky pricing. The main reason for resorting to computer simulation is that because the whole economy is to be represented in the model, the systems of equations are not analytically solvable because of excessive complexity. This complexity has been seen as necessary because of the demand for microfoundations, which in turn are taken as a necessary condition for a macroeconomic model to function in policy analysis. The New Keynesian add-ons have been introduced to remedy the dramatic empirical shortcomings of the RBC model built on rational expectations and the representative consumer. Nevertheless, despite these fixes, whether the core DSGE still fundamentally misrepresents the most important structural macroeconomic relations remains a contested issue (see Kuorikoski and Lehtinen 2018).

One of the biggest changes in the last decade is that DSGE modelers are now frantically developing models in which the assumption of a single representative agent is replaced with two or three different representative agents to model important heterogeneities (e.g., Kaplan et al. 2018; Ravn and Sterk ~~forthcoming~~). This has had important methodological consequences. Authors are now often reporting that they have simplified the model because otherwise it would take too much time to compute the results. This reflects the fact that the models have become so complex that the modelers are facing computational trade-offs between making more realistic assumptions concerning heterogeneous agents and less realistic assumptions concerning other aspects of the model. More importantly, the loss of analytical tractability makes it difficult to figure out which features of the models are really making a difference in the results (e.g., Acharya and Dogra 2020).

Another interesting aspect of current DSGE modeling is that there is very little critical discussion about the implications of the discretization of the underlying analytical model (Velupillai and Zambelli (2011) provide some remarks). Johannes Lenhard (2019: 33) argues that because real numbers must be represented by truncated values in such models, the information lost in truncation may lead to major errors when the procedure is iterated a large number of times. Lenhard is using climate modeling as a case study, and he criticizes "a common and influential belief about simulation models based on the incorrect idea that the discrete versions of models depend completely on the theoretical models" (Ibid., 24). Economists working with DSGE models seem to share this belief. Indeed, macroeconomists seem to accept this kind of computation *precisely because* it does not have any independent epistemic contribution in their models (see also Lehtinen and Kuorikoski 2007; Lenhard 2019: 137). We are not arguing that it would be important to analyze the role of truncation errors in DSGE models. These models have so many other more serious problems that discussing the problems of discretization might merely misorient the efforts to make them better.

3.3 Agent-Based Macroeconomics

For many, the inability to predict and analyze the financial crisis constitutes a damning indictment for the representative agent and rational expectations underlying the DSGE paradigm. After the crisis, agent-based macroeconomic models have become much more popular than before. Agent-based (AB) models provide much more flexibility in modeling heterogeneity, interaction with limited information, and computational capacities (e.g., Borrill and Tesfatsion 2011). There are now several groups engaging in AB macro modeling, and many of them no longer feel the need to discuss methodology. Instead, they just produce their models. This is a sign that the agent-based approach is taking off in an important way. Furthermore, some eminent economists like Joseph Stiglitz are now using AB models (Caiani et al. 2016), and the chief economist of the Bank of England is writing methodological papers in favor of them (Haldane and Turrell 2018, 2019). In addition, there are plenty of papers marketing the agent-based methodology in macroeconomics (Fagiolo and Roventini 2017; Caverzasi and Russo 2018; Dilaver et al. 2018; Dosi and Roventini 2019)

Yet, while there are plenty of AB macro papers being published, we have not seen a single one at the very top of the journal hierarchy. We will now look into aspects of AB models that many macroeconomists consider unattractive. Comparison of the reception of DSGE simulations, which can be appreciated within the first tradition of viewing simulations as computational solutions to intractable sets of equations, and agent-based macro models, which more clearly embody the second tradition of viewing simulations as imitations of processes (Durán 2021), sheds light on how economists see the proper place of computer simulation in their field.

Windrum et al. (2007) presented four interrelated categories of criticisms of AB models:

1. There is little or no understanding of the connection among the set of highly heterogeneous models that have been developed.
2. Lack of comparability between the models that have been developed. With many degrees of freedom, almost any simulation output can be generated by an AB model.
3. The lack of standardized techniques for analyzing and constructing AB models.
4. Empirical validation of AB models is difficult or is currently lacking.

Let us consider these criticisms in reverse order. Although many early agent-based models in macroeconomics were not tested with empirical data, we do not know of any argument to the effect that it would be intrinsically harder to empirically validate simulation models than analytical models. After all, it took more than two decades to develop the Kydland and Prescott (1982) model into one that can be estimated and that provides at least a reasonable fit to empirical data (Smets and Wouters 2003). Empirical validation of models also includes making predictions about the future development of the economy. In this respect, agent-based models are still unsatisfactory: we do not know of any agent-based models that are competitive in forecasting comparisons and tournaments. On the other hand, methods of estimating agent-based models are being developed (e.g., Delli Gatti and Grazzini 2020), and it might only be a matter of time before agent-based models mature sufficiently to be used in forecasting too [see Fagiolo et al. (2019) for a philosophically oriented account of these developments].

The lack of standardized techniques for developing agent-based models is also, at least partly, a contingent feature of their current state of development. Practically all DSGE modelers use the same program (Dynare) to discretize their analytical models, and as the community of agent-based modelers grows, it may well develop a similar specialized language to specifically study macroeconomic AB models.

In contrast, the first two criticisms point to deeper difficulties in agent-based modeling. The flexibility of AB modeling is also its most problematic characteristic [as acknowledged by agent-based modelers themselves, e.g., LeBaron and Tesfatsion (2008)]. Consider, for example, de Grauwe's (2011) model. This agent-based model is able to generate macroeconomic fluctuations endogenously. Given that the financial crisis of 2008–2009 could not be explained with DSGE models at the time, this could be taken as a major achievement. The problem, however, is that this model, just like many other agent-based models, provides sufficient but not necessary conditions for a result (e.g., Tubaro 2011). "Growing" the crisis simply does not suffice as an explanation because it leaves open the possibility that the result could have been obtained with different assumptions (see also Silverman 2018). This is why a demonstration of the robustness of the results is particularly crucial for agent-based models (e.g., Muldoon 2007; Durán and Formanek 2018).

More generally, given that agent-based models are more opaque than other kinds of simulation, it is important to be able to make systematic comparisons between different models. This kind of comparability is important in order to solve what Lehtinen (forthcoming) calls "Duhemian problems"; as similar macro outcomes may be the result of several different individual behaviors and several possible mechanisms translating such behaviors into macro outcomes, it is important to be able to see how any given modification to the model affects the outcomes. The solution of Duhemian problems requires comparability among models and, thus, some common elements in a family of models. From this perspective, heterogeneity among the agent-based models is inexcusable when it is combined with the ease with which one can generate different results with agent-based models. It is no wonder that DSGE macroeconomists consider engaging in agent-based macro as a "walk on the wild side" (see Napoletano 2018).

Before we see how the agent-based macroeconomists have reacted to these difficulties, it is useful to consider a perspective from the natural sciences. Lenhard and Winsberg have argued (Lenhard and Winsberg 2010; Lenhard 2017) that it is practically impossible to solve Duhemian problems in climate modeling, which is also based on discretizations (for a dissenting view, see Jebeile and Ardourel (2019)). They argue that one can only see the effects of changing a module in the results concerning the whole globe. These models also require the calibration of a large number of parameters, they exhibit important feedback loops between model parts, and the parts are knit together with ad hoc Kluges. Whatever the plausibility of these arguments is in the context of climate modeling, they are surely not less plausible in macroeconomics. The main difference between climate models and macroeconomic models is that climate models are partly based on established physical theory that is not questioned by anyone, whereas DSGE macro is based on "microeconomic" theory (of intertemporal utility maximization) that is commonly taken to be false, even by those who argue in favor of DSGE models (e.g., Christiano et al. 2018).

Such problems indicate that it is very difficult to solve Duhemian problems with a single model. Macroeconomists proceed to solve Duhemian problems by studying a benchmark model (see especially Vines and Wills 2018): the DSGE model provides a benchmark in the sense that it contains elements known to all researchers, and the macrolevel consequences of modifications and additions to the benchmark therefore remain mostly tractable.

Agent-based modelers have taken the criticism that their models are not comparable with each other or with the DSGE models seriously. Some have tried to provide a stripped-down version of what they consider to be the consensus way of diverging from the DSGE models, even labeling the resulting model a "benchmark" (Lengnick 2013; Caiani et al. 2016). Dawid and Delli Gatti (2018) discover an emerging consensus in their review of agent-based macroeconomics. Dawid et al. (2019) provide extensive documentation on one of the main agent-based macro models so as to facilitate using and replicating it. Finally, Gobbi and Grazzini (2019) provide a bridge between DSGE and agent-based approaches by constructing a model that is solved with agent interaction, but otherwise employs all of the standard assumptions of DSGE models. They show that as long as one uses rational expectations with intertemporal maximization and so on, the results of the AB model are highly similar to the DSGE benchmark. This suggests that the assumption of an instantaneous general equilibrium does not drive the results.

Even though all of these efforts will surely help to make agent-based models more acceptable to broader audiences, some obstacles remain. DSGE models were widely adopted by major central banks between 2004 and 2011. Central banks require first, that their macro models can provide a systematic causal account of what is happening in the economy and what would happen if various policies were to be implemented, and second, that they can predict the future at least as well as other models. The first DSGE model that is commonly thought to be good enough was that of Smets and Wouters (2003). At present, agent-based macro models are almost, but not quite, good enough in these respects for these purposes. Moreover, central bank economists must be able to demonstrate the economic mechanisms at work in the model in such a way that the decision-makers do not need to trust the modelers but rather can see for themselves how the model comes up with its results – the model has to provide a coherent economic story and agent-based models are not always transparent in this way.

4. Conclusions: Simulation and Theoretical Progress in Economics

Paul Humphreys (2009) stated that the growing importance of computational methods within most fields of science forces us to fundamentally reconsider the traditional anthropocentric epistemology, in which knowledge claims are, in the end, evaluated in terms of the perspective and cognitive capacities of human agents. The reception of computational methods in economics can be seen as a conservative reaction to this *anthropocentric predicament*. Simulation methods that can be conceptualized simply in terms of computational solutions to analytically intractable mathematical problems (Durán's first tradition of understanding simulation) are readily accepted in the mainstream methodological palette, whereas simulations that are more about directly mimicking dynamic processes in the modeled phenomena rather than computational extensions of existing theory (Durán's second tradition) face methodological resistance.

Simulation is thus accepted as a computational tool for calculating the consequences of economic theory, but theory building itself is still seen as lying solely in the purview of the mind of the human economist. Many economists have been taught to consider theory solely as a matter of analytical mathematics. Consider, for example, Lucas:

> I loved the Foundations. Like so many others in my cohort, I internalized its view that if I couldn't formulate a problem in economic theory mathematically, I didn't know what I was doing. I came to the position that mathematical analysis is not one of the many ways of doing economic theory: it is the only way. Economic theory is mathematical analysis. Everything else is just pictures and talk.
>
> (Lucas 2001: 9)

From this point of view, the fact that simulations do not have axioms or theorems, and that they look more like experimentation, generates resistance among economists to engage in them (Fontana 2006). This also means that conceptually emergent results from agent-based simulations cannot easily be accepted into theory as there is, by definition, no way to derive them from the axioms.

We hypothesize that this is also an important difference between the role of simulations in physics and in economics. In physics there is plenty of trust in physical theory as an accurate description of physical phenomena, and computer simulations built on this theory can therefore be regarded as good, or even superior, substitutes for material experiments (along the second tradition). In economics, theory is seen probably for good reasons more as a tool for thinking about rather than a literally true description of economic behavior. Econophysics is an interesting comparison case in this respect, but we cannot pursue this project here (however, see, e.g., Schinckus and Jovanovic 2013 and Jhun, Chapter 23).

Humphreys (2009) also outlined two possible ways in which the anthropocentric predicament can develop further. In the hybrid scenario, the role of the human cognitive agent remains epistemologically essential, and the computational aids need to be developed in such a way that respects human cognitive limitations. Economists accept the hybrid scenario in data analysis, forecasting, and policy analysis, but not yet in theory development. In the automated scenario, the computational aids are no longer built with human limitations in mind and science becomes fully automated. Although much has been said about the revolutionary nature of artificial intelligence (AI) and big data in the sciences in general, it remains to be seen how the mainstream will react to this challenge in their own field.

Related Chapters

Favereau, J., Chapter 25 "Field Experiments"
Henschen, T., Chapter 20 "Causality and Probability"
Jhun, J., Chapter 23 "Modeling the Possible to Modeling the Actual"
Nagatsu, M., Chapter 24 "Experimentation in Economics"
Spanos, A., Chapter 29 "Philosophy of Econometrics"

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