EDITORIAL

Agent-based modeling in social sciences

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In the natural sciences, computational methods have become a central part of the research process. In disciplines as diverse as physics, cosmology, chemistry, and pharmacy, computer-based modeling drives scientific progress and has become a standard tool for scientific research (Morrison 2015). The picture is slightly different in the social sciences, including economics, and business; when it comes to statistical analysis, for instance, computers indeed find their place there. However, computers play a less central role in that their contribution to theory-building, constructing models, and simulations has so far been only minimally exploited. When it comes to analyzing complex systems, non-linear dynamics, and phenomena of emergence, early contributions by Schelling (1969), Axelrod (1980), Epstein and Axtell (1996), and Arthur (1994) have already demonstrated that computer simulations can help overcome some restrictions of classical (economic and game-theoretic) modeling. However, early simulations tend to lack a thorough empirical validation and are sometimes based on highly idealized or even empirically false assumptions. This raises the question of the utility of such models. With this introduction, we hope to demonstrate the value of these contributions for modern social sciences, particularly economics, and business.

This special issue of JBE is part of growing evidence that computational methods are becoming more important. An analysis of SSCI data shows that the number of papers related to computer simulation and agent-based modeling has grown steadily in recent years.

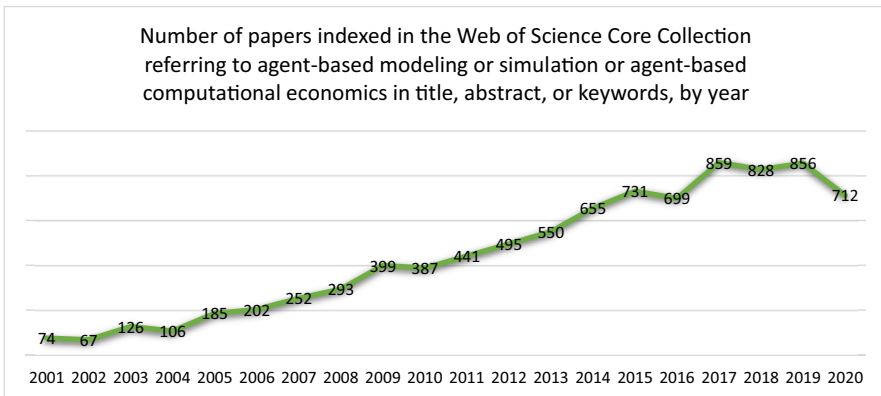✉ Johannes Marx
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Number of papers indexed in the Web of Science Core Collection
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There are good reasons that interest in computational methods and agent-based computer simulations is growing. One need only look at the agent-based models that have become canonical, including Schelling's segregation model (1969), the El Farol Bar Problem of Arthur (1994), Axelrod's simulation of evolutionary game theory (1980), and the bounded confidence model of Hegselmann and Krause (2002), to see these methods' potential.

Thomas Schelling's (1969) Model of Segregation was one of the first prominent contributions worth exploring. Initially, it was intended to model spatial segregation processes in urban areas driven by individual choices regarding where to settle. A population consisting of two types of agents is set on an $M \times N$ grid. The agents have a minimal goal orientation: they want to find a space to settle down and prefer a neighborhood of people "match their group" with a certain threshold. In the simulation, agents move around on the grid to find a suitable, non-occupied place. A place is suitable if a sufficiently high proportion of actors of one's own group is located in that neighborhood. If places do not meet agents' expectations, they move on to another empty space. Schelling showed that segregation processes occur even if agents are "tolerant" in the sense that they would be happy to live in a neighborhood with only a moderate percentage of neighbors of their same group. Segregation in his model can occur even with a threshold of one-third of own-group neighbors.

The Schelling segregation model became canonical because it shows that individual intentions should not be derived from behavioral patterns at the macro level. He gave us strong reasons to believe that there may be other drivers of segregation processes, such as the occurrence of cascade effects. A cascade is understood here as a chain of small events inevitably giving rise to sometimes unforeseen consequences. In Schelling's simulation, such a consequence would be one agent moving out of a neighborhood and changing the ratio of neighbors in a new context, causing other agents to move and thus driving unintentional segregation. The Schelling model is, therefore, an example of simulating negative externalities and so-called non-intended effects of actions at the micro level that cause surprising macro-level effects, which are often the subject of social science.

The El Farol Bar Problem, introduced by Arthur (1994), is another famous model. Every Thursday night, a large number of people want to go to the El Farol in Santa Fe, New Mexico. The bar, though, is relatively small and becomes crowded quickly. The agents in the simulation have a conditional preference: they only want to go when the bar is not overcrowded. Agents form beliefs regarding whether it is worth going to the bar the subsequent Thursday and behave accordingly. Experience of previous Thursdays inform the updated beliefs of the agents. Arthur showed with this simulation that there are no pure strategies to solve this kind of decision problem.

Arthur's simulation characteristically represents a particular feature of complex decision situations that is also the focus of game-theoretic analyses. This situation corresponds to what is known in game theory as a "minority game." The problem for the agents is that the choice of the optimal decision strategy depends on the decision behavior of the other actors. All agents must, therefore, form beliefs about the beliefs of other agents. The results are obtained under strong restrictions: agents cannot communicate with each other and have to form these beliefs and decide simultaneously. The model can be used to show how self-coordination based on rationally formed beliefs can lead to non-efficient social outcomes.

Axelrod's Tournament is a computer tournament initiated and published first in an article by Robert Axelrod (1980) and in an extended version in the book *Evolution of Cooperation* (1984). In its first iteration, leading experts in game theory were invited to submit strategies for a two-person prisoner's dilemma game in the form of a round-robin tournament. The strategies competed against each other, each playing against itself, a random strategy, and every other strategy submitted. All these strategies were fixed and defined in advance. Thus, agents could not take advantage of knowing opponent strategies. The surprising winner of the initial tournament was a simple strategy called "TIT FOR TAT" that always began with cooperation as the first move and then reciprocated (or echoed) its opponents' moves in its own subsequent moves.

The winning strategy in Axelrod's Tournament exhibited several specific characteristics: among other things, it was nice (i.e., it was never the first to defect), began with a cooperative move, responded to cooperative moves with its own cooperation, did not aim to score higher than an opponent (what Axelrod termed as "don't be envious"), and was not overly complex. Axelrod used the simulation to elaborate conditions under which cooperation can evolve in situations that display the characteristics of the prisoner's dilemma. One important result of his further iterative simulations is that clusters of agents with "nice" strategies can survive even in a population with primarily uncooperative agents. However, Axelrod showed with the tournament that there is no optimal strategy independent of the strategy chosen by the other players.

Our last canonic example is the bounded confidence model of Hegselmann and Krause (2002), in which the authors analyzed the opinion dynamics within a group of interacting agents. They elaborated on the conditions under which deliberation processes lead to consensus or polarization among the agents. In the model, agents begin with a particular opinion that can be represented as a real number, which allows for ordering agents by their initial opinion. The agents have

a minimal goal orientation. They try to form an opinion based on their initial belief and the beliefs of others. To develop this informed opinion, they interact continuously with other agents. However, the agents update their opinions only when other agents' opinions are close enough, that is, in their confidence interval. Hegselmann and Krause analyzed how and why an initial opinion profile transforms to a specific final distribution of opinions. Among other things, they were able to show that even agents with initially close opinions can develop in different directions and end up at opposite ends of an opinion spectrum.

The scope of applications of these few example simulations is quite broad, spanning from the analysis of spatial distribution patterns and social sorting mechanisms to the analysis of complex social optimization problems, from the investigation of the conditions under which cooperation evolves to the modeling of opinion dynamics in groups. And these are only a small sampling of the diversity of early simulations (see Retzlaff et al 2021). These early models are still prominent in teaching and research and are frequently cited.

However, one may doubt whether these early contributions are still useful for current explanative research. For example, perhaps empirical segregation processes seem to follow other, more complex rules than those of Schelling. The El Farol model is quiet with respect to the social embeddedness of the agents; perhaps agents communicate with other agents or decide in groups. "TIT FOR TAT" may win a round-robin tournament based on an iterated prisoner's dilemma given the set of competing strategies, but should we trust the general conclusion that its strategy would be rational in empirical situations? Do agents really form their opinions through deliberative processes, employing a weighted averaging of the opinions close to their own? What role does the weight of arguments play in this process? Questions of these sorts suggest that many people would likely be concerned about taking these models as causal explanations of the empirical phenomena described.

These concerns show that the scope and utility of such simulations is not completely clear. It does seem odd that such highly idealized models as those of Schelling (1969), Arthur (1994), Axelrod (1980), and Hegselmann and Krause (2002) remain so prominent. Why are we still using them in research and teaching? One reason could be that the range of causal questions that can be addressed by scientists is broader than what is covered by the classical understanding of what counts as a causal explanation (Hempel and Oppenheim 1948).

Even the contributions in this special issue show that researchers aim for their models to serve a wide variety of purposes. On the one hand, we found articles that adopt a counterfactual starting point. Jani (2021), for example, analyzes investment behavior by modeling the consequences of different reference points for evaluating an outcome as a gain or a loss. With his agent-based model, he reveals conditions under which the riskiest or the safest investment option emerged as the most prevalent.

Eismann (2021) uses an agent-based simulation to discuss the diffusion and persistence of false rumors in social media networks. She analyzes how the searchability of information can hinder actors seeking to evaluate the trustworthiness of a rumor's source and, thus, contributes to the emergence of social rumors.

Cabrera et al. (2021) explore a related research question by studying the effects on the quality of public opinion of the communication structure in social media networks. They draw on the theory of the spiral of silence in their computer simulation to examine the effect of the number of separated communities and their connectivity on the emergence of homogeneous public opinion and the occurrence of global spirals of silence.

Schulz and Mayerhoffer (2021) adopt a counterfactual starting point while replicating empirically observed firm size and age distributions, such as so-called "superstar firms". Their model provides a mechanism for revealing these patterns based on parsimonious economically driven learning taking place in a network.

Koch et al. (2021) have a slightly different focus, aiming to provide policy recommendations for the design of crowdfunding platforms that avoid the negative externalities of overfunding. They propose a taxation mechanism to internalize the negative effects of overfunding and use agent-based modeling to evaluate the outcomes.

On the other hand, some authors address empirical questions more directly. For example, Lorscheid and Meyer (2021) use data from psychological experiments to calibrate empirically their model of the performance of team decisions. Their agent-based model reveals mechanisms that at least partly explain the outcome of the patterns they found in the experiments.

Finally, Stummer et al. (2021) develop a simulation to model the effects of technology choices on future markets. They use a multi-method approach and combine scenario analysis that generates multiple pictures of the future along with an agent-based simulation to predict the potential consequences of today's technology choices on the characteristics of potential future markets.

This brief review already points to the variety of possible applications of agent-based modeling. In these contributions, agent-based modeling is applied in counterfactual settings but also for explanatory demands, policy recommendations, and even predictions. Even if there is a trend at present that favors more empirically calibrated models that come close to what counts traditionally as an explanation in social sciences, other applications of agent-based modeling have their legitimate place. We argue that agent-based models can be used to address a huge variety of research questions that correspond to many different explanatory demands. Typically, research questions that call for a classical explanation follow this form: "Why x?" However, many other legitimate questions can be addressed—for example, does Y always cause X? To investigate questions of this sort requires suspending concern about concrete details of the facts under investigation and focusing instead on the general pattern underlying the observed phenomena.

Furthermore, one can also question whether Y is a necessary condition for X to arise. Questions like this can be investigated in a counterfactual design using an agent-based model simulating conditions that are possible but empirically not given. For example, one could show that it would be sufficient for X if Z is given. Such an investigation would be possible even if Z is not given in the real world and is only a possible state of the world.

All these research questions belong to the family of explanatory endeavors. However, as current literature on the philosophy of science shows, these different research questions go along with different kinds of standards with respect to what

counts as a good answer. The canonical understanding of an explanation provided by Hempel and Oppenheim (1948) is too narrow to capture all types of applications.

Among the different ways explanatory questions are addressed through agent-based modeling, we focus here on two typical approaches: (1) how-actually explanations, which largely correspond to the common understanding of covering-law explanations; and (2) how-possibly explanations, which address other kinds of causal research questions, such as counterfactual ones (Grüne-Yanoff and Verreault-Julien 2021).

1. A large group of agent-based simulations seeks causal explanations for real-world events. This type of explanation is called how-actually explanations. How-actually explanations provide answers to the question of how and why things occur. Often, agent-based simulations do not provide simple explanations in which a few independent events drive the causal process. Rather, simulations often involve entangled actions, such as chains of actions, where actions of individual agents have feedback effects on the actions of other agents, and in which the order of actions may matter. Nevertheless, these types of explanatory ventures can, in principle, be reconstructed as a series of single covering-law explanations.

   Several criteria must be met for these explanations to be considered successful. There are internal criteria: the character of the argument, for example, must be deductive, and the premises should include a law-like general statement. Other criteria refer to the relationship between the premises used in the argument and the external world, and are thus external criteria. How-actually explanations, for example, require that the premises of the argument and the explanandum be true. All internal and external criteria must be met in how-actually explanations.

2. Other simulations, including the four canonical contributions described above, do not provide descriptively accurate explanations of real-world phenomena. They do not provide an empirically true description of the motivations and beliefs of the agents. Even the explananda of the simulations do not directly correspond to concrete, real-world phenomena. Often, this type of simulation is characterized by the fact that the explanandum is not a concrete empirical phenomenon, but rather is a macro pattern that is highly abstracted or idealized. In addition, the explanatory factors in the simulation may be highly idealized or even counterfactual. For example, they do not ask why X occurred but address whether it would be possible that Z can bring out X.

   To address these types of questions, it is not necessary that Z exists in the real world. This becomes obvious, for example, when one considers the modeling of actors' motivations and beliefs in the Schelling segregation model or the lack of social structure or misinterpretation in Axelrod's Tournament simulations. Such an approach is inappropriate if we are looking for classical explanations in terms of the covering-law model. However, such simulations serve other purposes. They allow users to ask, for example, whether it would also be possible for X to arise even if Y were not the case. Explanations of this

sort, called how-possibly explanations, aim to identify possible factors and mechanisms that could produce the explananda.

Still, even in these sorts of explanations, all internal criteria must be met; external criteria, though, are not completely satisfied in such simulations. This does not mean "anything goes" in terms of modeling choices. Sugden (2000), for example, argues that these types of explanations are possible in the sense that they could become true.

Against this backdrop, the explanatory role and scope of the early contributions described above become clearer. They did not aim for how-actually explanations of concrete phenomena, but focused on general patterns and mechanisms underlying the targeted phenomena. The modelers were concerned with explanatory questions that belong to the type of how-possibly explanations.

Simulations are not restricted to how-possibly explanations. Current simulations may adopt those earlier general mechanisms for more specific contexts and research questions. Of course, computer simulations can be designed to meet the internal and external criteria of how-actually explanations (see Klein et al 2018). Those simulations use empirical information to define input parameters to calibrate their models.

Both types of explanatory endeavors have their merits, and address different explanatory questions. The difference between a how-actually and a how-possibly explanation helps in understanding different styles of application and performance of agent-based modeling. Furthermore, it may even shed new light on the old debate over instrumentalism and realism. Many misunderstandings could be resolved by acknowledging the legitimacy of different types of explanatory questions researchers address. Along these lines, we hope this Special Issue will provide insights into the broad scope of possible applications of agent-based modeling. The contributions here illustrate the tremendous potential of agent-based modeling for addressing a wide variety of causal research questions. The distinction between how-actually and how-possibly explanations discussed above is a reminder that different research perspectives may come with different standards. In this sense, we hope to contribute to a fair and realistic assessment of the power of agent-based modeling.

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