Perspective

Agent-based modeling: A revolution?

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A clear consensus among the papers in this Colloquium is that agent-based modeling is a revolutionary development for social science. However, the reasons to expect this revolution lie more in the potential seen in this tool than through realized results. In order for the anticipated revolution to occur, a series of challenges must be met. This paper surveys the challenges suggested by the papers of this session.

Agent-based modeling (ABM) has been gaining growing acceptance and enthusiasm in various fields of social science in recent years. Whatever disagreements may have emerged in this Colloquium, there is one point of clear consensus: ABM holds out the promise of a revolutionary advance in social science theory. Two questions are worth asking. What are the reasons ABM is thought to be revolutionary, and what important next steps in developing ABM as a tool for social science are needed in order for this revolution to occur?

There are numerous precedents in history of a new tool catalyzing revolutionary developments in the science that used that tool. It was developments in lens grinding which allowed the creation of telescopes that made astronomy possible. Similarly, the microscope was necessary for bacteriology. And the study of physics was fundamentally transformed by the invention of the calculus. So, there is no reason to doubt the plausibility that a new modeling technique might have profound implications for those sciences that make use of it. There are also, of course, many examples of brilliant tools whose impact was much less profound. The revolutionary tool innovations are distinguished from those with lesser importance not by the technological virtuosity of their creation, but by the needs they served in the sciences that adopted them. So, to evaluate this proposed revolution, what matters is not the computer science advances that make ABM possible, but rather the social science challenges that make it necessary.

Surveying the papers of the Colloquium, one can discern three generic reasons cited for the potential importance of ABM to social science. These are: (*i*) the unsuitability of competing modeling formalisms to address the problems of social science, (*ii*) agents as a natural ontology for many social problems, and (*iii*) emergence. In the remainder of this paper, I consider each of these reasons in turn. Each one provides a vantage point to suggest important next steps in developing ABM. If the anticipated revolution is actually to occur, and the potential of ABM is to be realized, these steps will need to be taken.

The most fundamental reason for the enthusiasm for ABM is the dissatisfaction with the restrictions imposed by alternative modeling formalisms. The most widely used alternatives are systems of differential equations and statistical modeling. Both of these competing tools have made important contributions to social science, but both are viewed as imposing restrictive or unrealistic assumptions that limit their use for many problems. The list of assumptions that have been objected to is lengthy, but it includes linearity, homogeneity, normality, and stationarity.

Except where these assumptions relate to ABM's advantages as a representational system (dealt with below), this relaxing of assumptions is not unique to ABM but is a property of computational science generally. The need to pose problems in a form tractable for mathematical analysis or proof often requires assumptions that can be relaxed with computer simulation. This is the reason that computer simulation has the promise of allowing us to examine issues that have been avoided in theoretical disciplines based on mathematical derivation. In the social sciences, simulation may allow more aggressive exploration of the implications of, for example, imperfect rationality, the effects of learning and information, and social and institutional structure.

These important new uses of computational science are presently impeded however, by a lack of clarity about the uses of computational models and the requirements for credible arguments using them. When viewed simply as a means of accomplishing complex mathematical modeling, computation becomes strictly less reliable than mathematics, unable to achieve the global proofs available with mathematical abstractions. As a consequence of the tendency to pursue computational science with methodologies of mathematical modeling, its use is often restricted to two special cases. Computational modeling is widely accepted as useful for problems where models can be devised that accurately predict the future behavior of systems of interest. Where this is not the case, however, its use is restricted rather to the relatively weak role of generating suggestive examples. As a consequence, computational models have provided insights and hypotheses but few theories or problem solutions for exactly those very nonlinear systems where potentially they might be most important. This is a major reason why, for example, there has been very little use of ABMs to recommend public policy.

This limitation is unnecessary. Once it is recognized that computational modeling is more appropriately understood as an example of experimental mathematics, akin in its standards of rigor to experimental science, issues of credibility and acceptable methodology become much more tractable. The standards of rigor for computational science is much better founded on the views of Karl Popper and evolutionary epistemology (1, 2) than it is on the standards of deductive proof (3). And by understanding computational science as the search for credible arguments based on computational experiments, credible uses of ABMs to argue for public policy become feasible. (For example, see the growing use of computational modeling in general and ABM in particular in studies of global climate policy; ref. 4.)

The second reason advanced for the importance of ABM is its naturalness as an ontology or representational formalism for social science. This advantage is indeed significant, as the centrality of agents in these models provides a place to express the enormous amount of data and knowledge about the behavior, motivations, and relationships of social agents, be they hu-

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man individuals or institutions. In contrast to other formalisms such as differential equations or statistical models, which apply primarily to aggregated data, agent-based models are a strikingly powerful formalism for exploiting exactly that category of information which is the focus of many social sciences.

As a consequence of this clear advantage, work in ABM to date has emphasized issues in representation, and the literature reflects this emphasis, with most papers focusing on the representational design of models. However, there are numerous other topics that need to be addressed if ABM is to achieve its potential, including case loading, uncertainty analysis, the calibration of models to data, and methodologies for using models to answer specific questions or to solve problems. It is to these topics the field must now turn if it is to advance.

The final reason advanced for the importance of agent-based modeling is its power to demonstrate emergent phenomena. The idea of "emergence" is one of the touchstones of what has come to be called "complexity science" (5–7). In social science, topics such as the emergence of cultural norms or institutions from the interaction of individual activity are indeed very important and not well addressed by competing modeling formalisms. So, the demonstrated ability of ABM to discover examples of such emergent dynamics from knowledge about the behavior of members of a society is potentially quite useful.

However, as long as demonstrations of emergence are confined to the use of computer graphics for attractive demonstrations, the scientific importance of emergence and of ABM demonstration of emergent phenomena will remain small. Most papers, in this Colloquium, and in the general literature, rely on human observers to declare emergence to have occurred based on graphical computer outputs. Formal definition of what is meant by emergence is the exception rather than the rule, and quantitative tests that a given model achieves the sort of emergence advertised are rare.

An important advance in ABM-related research will occur once this situation is improved. Emergence is fundamentally a multiresolution concept with, as has been famously noted, micromotives leading to macrobehaviors (8). Thus, emergence can be characterized by a measure of macroscopic behavior achieving a threshold value in a simulation built from microscopic behavior. The discipline of defining these macroscopic measures and using them to assess behavior quantitatively across ensembles of alternative agent-based models can make rigorous what has often been polemical. And when this rigor becomes routine, the advertised revolution in social science will be well on its way to being achieved.

Thus, agent-based modeling can be understood as a field that has made significant progress and stands on the threshold of demonstrating its importance beyond the narrow confines of aficionados. The reader will find evidence of the excitement of a field in transition in the papers of the Colloquium presented here.

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