

# Putting the agent in agent-based modeling

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**Abstract** One of the perquisites of a talk like this is that I get to expound on broad themes. AAMAS is a conference about agents and multiples of agents, so I probably ought to say something about agents. Of course, my position on agents is that I am all for them. Today I'd like to make a case for actually putting agents in agent-based models. I hope that by the end of the talk you have some idea about what I mean by this.

**Keywords** Agent-based modeling · Multiagent systems · Empirical game-theoretic analysis

## 1 Background: agent-based modeling

The first part of explaining it is to provide some background on agent-based modeling (ABM). The ABM label has been adopted by a community of researchers, to refer to a broad computational approach for modeling social systems [15]. The idea is that to understand a social system, one constructs a distributed simulation model, with key elements termed *agents*, who interact so as to generate the overall system behavior. The main advantage of this so-called bottom-up approach is that it accommodates complexity in various forms. The prevailing alternative—analytical or equational models—generally require extreme simplifications in order to allow tractable inference. For example, in economics—the social-science field I am most familiar with—mainstream techniques employ liberal doses of aggregation, abstraction, and equilibrium concepts, in large part to avoid the complications of heterogeneity, fine-grained relationship structure, and dynamics. The agent-based approach to economics [22] is viewed by many adherents as a reaction to this mainstream, and thus is generally

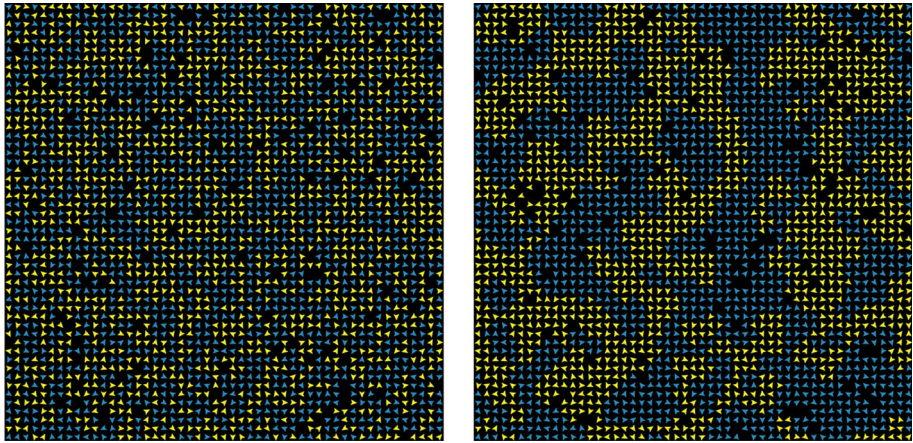
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**Fig. 1** Schelling segregation model, as implemented in NetLogo by Wilensky [28]. The initial random configuration is at *left*, and the final quiescent state at *right*

suspicious of the mainstream simplification tactics. Let me further illustrate the range of ABM with a couple of examples.

Our first example is a classic, highly and rightly celebrated. Thomas Schelling received the Nobel Prize in 2005, “for having enhanced our understanding of conflict and cooperation through game-theory analysis”. Over forty years ago, he developed a very simple, highly abstract, model of housing segregation [20]. The Schelling model operates on a grid, where “agents” of two different colors (shown in Fig. 1 in yellow and blue) decide where to locate. They start in some random locations, and at each time period decide whether to stay, or to move to another random location. They care about the color of their neighbors, but not in any extreme way. Specifically, they are happy as long as some minimum fraction of their neighbors have the same color. In the instance shown here, the threshold is 0.33. Therefore each agent is happy if as many of two-thirds of its neighbors are of different color. Nevertheless, as we see in this representative outcome, the system converges to a state that is far more segregated than that (average over 75 % of neighbors same color). This demonstration was particularly striking, as it showed that extreme levels of segregation observed in society need not be a product of extreme racial preferences. Moreover, as emphasized by Schelling, even with very simple behaviors, a system’s *macrobehavior* can exhibit properties that are a surprising function of the given *micromotives* of the constituent agents [21].

Second, let us consider a very different and much more modern example. Concentric<sup>1</sup> is a small company in Massachusetts that constructs agent-based models for decision support regarding marketing campaigns [19]. The agents represent consumers, and the model specifies the structure of preferences and patterns of communication. The ABM captures how consumers spread information and change their tastes about products, for example breakfast cereals or TV shows. Modelers run simulations to see how the population will react to a given marketing campaign, say a proposal for a particular balance of broadcast advertising and social media placement.

As we have seen, agent-based models span a gamut from Schelling to selling. In between these extremes, researchers in many disciplines have applied ABM to problems in their domains. For example, I know researchers at the University of Michigan who practice ABM

<sup>1</sup> [www.concentricabm.com](http://www.concentricabm.com)

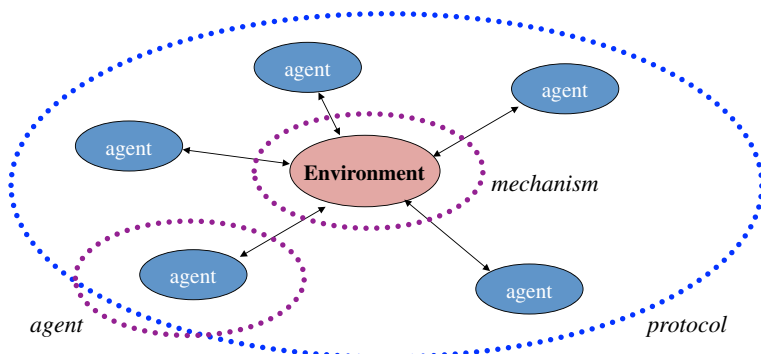
in political science, sociology, epidemiology, finance, environmental science, and economics. There are plenty of other fields with active ABM research: anthropology, military studies, public health, transportation, and so on. As apparent from proceedings at this conference, many in the AAMAS research community are also working on problems in these domains. Moreover, multiagent systems (MAS) researchers routinely employ simulations of interacting agents in our studies, which we usually cast as *computational experiments*. A few of us even call what we do ABM, but most of us do not. Are we part of the ABM enterprise?

## 2 ABM and MAS

A recent analysis published in *Scientometrics* suggests that we are not. Niazi and Hussain [17] conducted a comprehensive citation study of “agent-based computing”, as they termed it, which they characterize as the union of the ABM and MAS communities. They performed a cluster analysis, and found that MAS has a very tenuous bond in the literature to the major ABM clusters. There are certainly exceptions, for example in his keynote talk at this conference Michael Luck described an MAS project on norm emergence [13] that builds directly on ABM literature [1]. But more broadly, to use words of Niazi & Hussain: “these communities have, at times, perhaps nothing other than the notion of an ‘agent’ in common with each other”.

So what is the difference? It cannot be concern with natural (ABM) versus artificial (MAS) societies. In his keynote remarks, Michael Luck emphatically rejected that boundary line, and I could not agree more. Niazi and Hussain [17] try to explain it in terms of a distinction between (social) science and engineering. One often hears this explanation, but I do not think it holds up to scrutiny. Much ABM work directly feeds into decisions, as in the marketing ABM models by Concentric. Even Schelling’s very abstract ABM was motivated by providing insight relevant for housing policy. Similarly, agent simulations in our community are likely to be performed in service of design (in the parlance of economic theory, *mechanism design*), but to do that well also calls for a scientific understanding of how configurations of agents behave in different circumstances.

Figure 2 presents a standard MAS diagram, where a set of agents interact through some environment. The ellipses delimit the three possible scopes open to a designer in a multiagent domain in any given circumstance. In one, we may be faced with the problem of designing a single *agent* to participate in a multiagent system. In another common setup, we may have the opportunity to design or influence a piece of the environment, that is, the *mechanism* governing at least in part how agents interact. Some MAS work imagines that we could have the ability to design the entire system, that is, dictate a *protocol*. But notice that regardless



**Fig. 2** Alternative scopes for design and analysis in multiagent systems

of which version of the design problem we may carve out in a particular work, our model has to consider the behavior of the entire system. Whether we are designing a mechanism or a single agent, our outcome depends on what all the agents do in consequence. This is just another high-level way of arguing again: there is no clean division of labor we should expect to find in the agents world between science and engineering. So that is not a basis for separate ABM and MAS enterprises.

For example, here are three questions that ABM researchers typically ask, and I would argue are often asked and are almost always relevant in MAS studies as well.

- (1) Can we explain some observed multiagent phenomena through a generative model?
- (2) How will the system respond to some shock or policy intervention?
- (3) What outcomes will/could arise in some postulated environment?

Such questions are relevant to design at any of the scopes illustrated in Fig. 2.

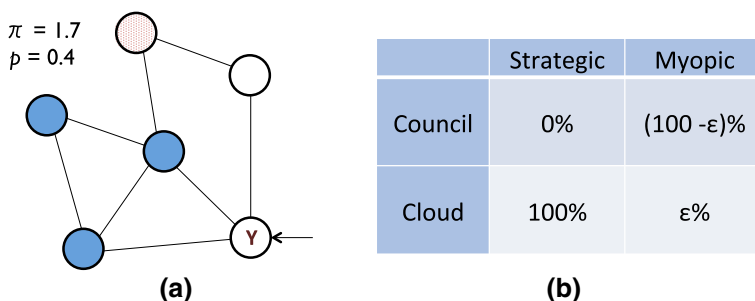
So what is there to gain by strengthening the bridge between these communities? I believe that there are benefits in both directions. It would probably be most useful to emphasize the potential contribution of ABM to MAS, as talking to the AAMAS community about its great value would be cheap pandering. But while I could catalog the capabilities and knowledge that ABM offers, I would rather just go ahead and pander by focusing on the MAS contribution side of the ledger.

The first and most obvious capability that AAMAS research offers is *more sophisticated agents*. This community knows about designing agents, and ABM should benefit from the state of art in agent design, including complex knowledge structures, reasoning from models, and learning from data and experience. The ABM field has tended to focus on very simple agents, and in many respects this is a desirable feature. The Schelling model is all the more striking due to the simplicity of the individual agents and yes, simple behaviors are easier to implement (and Schelling also gets a pass because he did not have a computer). The keynote presentation at this conference from Iain Couzin wonderfully demonstrated the power of simple models for explaining collective behavior in biology [2, 16]. But the agents in our models typically represent people or organizations—augmented with computers, not unassisted locusts or fish.

The second major area where MAS can contribute is on the application of strategic principles for selecting among agent behaviors. This addresses what I argue is really *the* fundamental issue in ABM methodology: On what basis should the modeler incorporate specific agent behaviors? Since the choice of agent behaviors drives the system outcomes, any conclusions from an ABM study hinges on the justifications the modeler has for these choices. My view is that ABM research often does employ well-justified criteria in agent design. For example, designers often seek to find behaviors that fit observational data, or employ evolutionary search to arrive at an agent population. What I have not seen is a general articulation of principles for selecting from a large space of agent behaviors, that could accommodate agents of the complexity and sophistication that we tend to develop in this community. I believe that is something we can contribute to, and will address that in detail shortly. But first let us move to an example demonstrating the sensitivity of choice between simple and sophisticated agents.

### 3 Agent sophistication matters

The example is taken from my recent work with Travis Martin and Grant Schoenebeck [14]. The domain is *network science*, which is another distinct field but aligns well in key respects with the ABM community.



**Fig. 3** **a** An instance distinguishing myopic and strategic behavior. **b** Optimal adoption achievable in particular graph structures, for strategic and myopic agents

*Cascade models* [8, Chapter 19] attempt to capture the spread of something—diseases, opinions, habits, technology adoptions—whatever, on a network.<sup>2</sup> In our work we are particularly interested in *decision cascades*, where the state of the network node corresponds to some decision, by something we could call an agent. Predicting cascades can be important for many applications. For example, consider a situation where we have visibility over a network and wish to influence a cascade—say for a product adoption—by managing the sequence by which the agents are exposed to the decision opportunity.

Our work adopts the framework introduced by Chierichetti et al. [4]. In this model, the nodes choose between two alternatives, labeled YES and NO (think product adoption). Nodes have an inherent preference type for one of these options, drawn independently, with  $p < 1/2$  the probability of inherently preferring YES. The utility of an agent depends on whether it satisfies this underlying preference, and how well it matches the choice of its neighbors. Specifically it gets  $\pi$  for matching its own preference, and one for each neighbor match. The objective of the *scheduler* is to choose a decision ordering, potentially adaptive, so as to maximize the expected number of YES choices by the agents.

Chierichetti et al. [4] follow much of the cascade literature by assuming agents make their choices myopically. What that means is that they choose based on the current state, refraining from considering the future other-agent choices and moreover ignoring their own influence on the future choices of undecided agents. Consider the instance of Fig. 3a. In this example, the indicated node at lower right is directed to choose, and acting myopically, it considers only the neighbors who have already decided. The agent has an inherent YES preference, which is worth  $\pi = 1.7$ , but it already has two neighbors who chose NO (solid, blue), and so that overrides. The agent therefore chooses NO.

A strategic agent, in contrast, is forward looking, and attempts to reason about the choices of its undecided neighbors as well. This requires some sophistication, as well as more knowledge of its environment. With such capabilities, it reasons as follows. If it chooses NO, then it will get two units of utility for matching the already-decided neighbors. Its undecided neighbor chooses next, and since that node has one decided YES neighbor and one decided NO neighbor, will choose its own type. This result will be NO with probability  $1 - 0.4 = 0.6$ , and therefore the total expected utility for a strategic NO choice is  $2 + 0.6 = 2.6$ . If on the other hand our agent chooses YES, then it gets 1.7 for satisfying its own preference. Thinking ahead, it also realizes that its undecided neighbor will then be surrounded by two YES nodes, and so it will also choose YES regardless of its own type. The expected utility for choosing YES is therefore  $1.7 + 1 = 2.7$ , and the agent chooses YES. So we see that a strategic agent

<sup>2</sup> Cascade models of information transfer also feature interestingly in Couzin's research on schooling fish [5].

may behave differently from a myopic agent, and it stands to reason that this could affect the performance of any scheduling policy, and for that matter the performance achievable by any schedule.

We have shown [14] that in fact these differences can be arbitrarily large. As indicated in Fig. 3b, we have identified a class of graph structures (which we call *Council* graphs) where the scheduler has no ability at all to promote adoption by strategic agents, yet it can find schedules that achieve arbitrarily high performance by myopic agents. But we can also identify graphs (called *Cloud* graphs) where strategic agents can be induced to adopt 100 % of the time, but the myopic agents are impervious to influence. Now this is not an argument that modeling agents as strategic is inherently better than the simple myopic model. What it does demonstrate, quite starkly, is that assuming one category of behavior or the other has pivotal consequences. It is not harmless to simplify, any more than one can benignly assume that agents are highly sophisticated. In other words, this just underscores the suspicion that many already harbor about agent-based models: they are potentially very sensitive to assumptions one makes about agent behaviors.

#### 4 Criteria for selecting agent behavior

Which raises the obvious question: How can or should we justify our choices? There is no option of not choosing—whether one is following ABM methodology or not—as it is just a fact that outcomes of multiagent interaction depend on what agents do. Possible bases for selection include the following:

- (1) plausible heuristics (simple, obvious, easy to implement)
- (2) rationality (game-theoretic solution concept)
- (3) historical observations of agent behavior
- (4) historical observations of system outcomes
- (5) evolutionary stability
- (6) result of reinforcement learning process

The first two are the cases we considered in the decision cascade example. The ABM and network science communities have typically taken the first approach, whereas mainstream economics and much of the work in the AAMAS community is prone to rely on rationality assumptions to select agent behavior. Neither approach is universally correct, and neither even offers complete basis for selection. There may be many plausible simple behaviors that produce drastically different outcomes, and there may be multiple equilibria of a game. So we need some additional criteria. One natural source is historical data, either on the agent or system level. The ABM community very often appeals to the latter (#4 above), justifying agent behaviors according to their ability to generate system outcomes exhibiting pre-identified stylized facts of their domain.

Other reasonable criteria to apply include evolutionary stability—which captures systems subject to natural selection, or correspondence with results of some learning process, particularly if there is reason to consider the subject system as made up of agents who learn in a certain way. ABM researchers often appeal to criteria like these. However, there are computational challenges to applying some of these criteria, particularly in a rigorous and systematic way. Numbers 1 and 3 directly yield behaviors which can be simulated to yield direct outcomes, but even coming up with the candidate behaviors for the other criteria can pose computational difficulties. However, I argue that these difficult ones are actually the ones that can support principled justifications, and so should be our focus if our goal is to



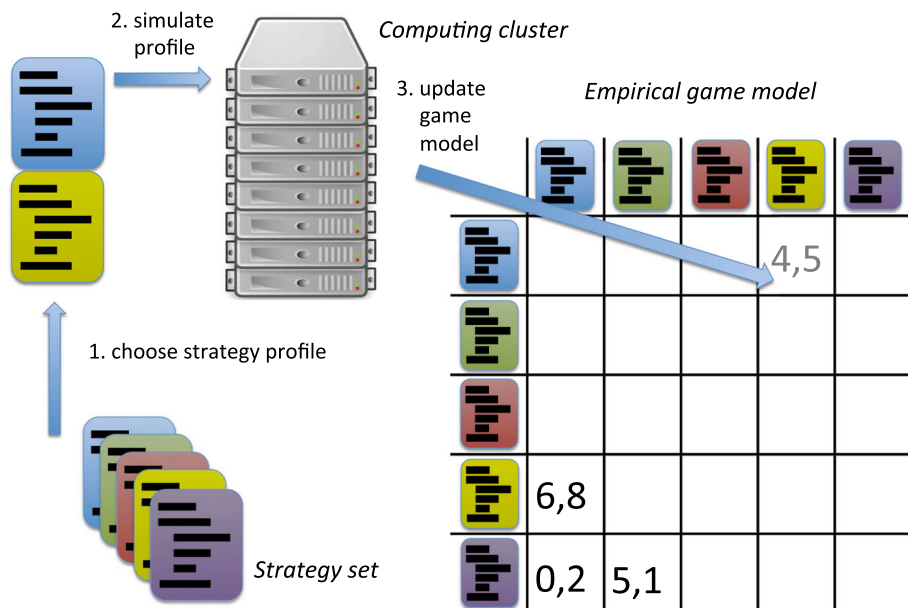
produce compelling conclusions from agent-based models. It is no coincidence that much of my group's work over the past several years has been how to address the computational problem for criterion #2 (rational, or game-theoretic selection) in complex environments.

## 5 Empirical game-theoretic analysis

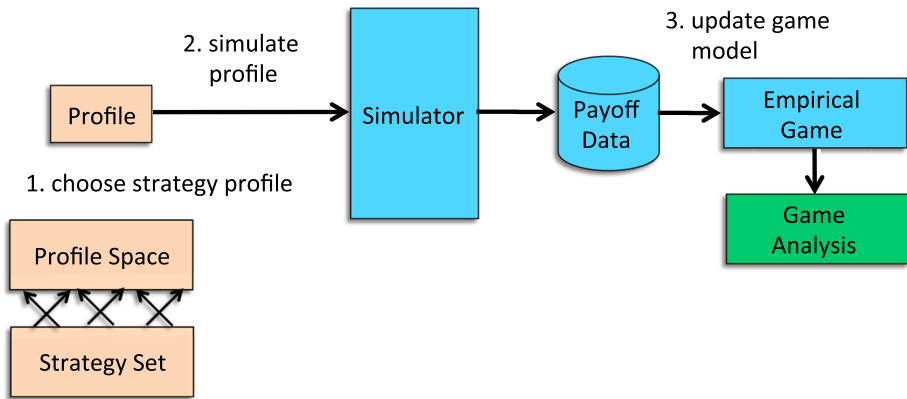
What we do, simply stated, is construct game models via agent-based simulation. Figure 4 presents a cartoon version. We start by implementing agent strategies as programs that govern their behavior in the simulated environment. We then simulate various combinations, or *profiles*, of these agent strategies, which generates data about the payoffs the agents get in those profiles. In a normal-form representation as shown here, each simulation outcome can be viewed as a sample for the payoffs in a particular cell of the game matrix. Once we have filled in a sufficient number of cells with a sufficient number of samples, we can start to reason about the resulting game model.

Simulation-based game construction is part of a broader process, which I have been calling *empirical game-theoretic analysis*, or EGTA [25]. Figure 5 presents a partial flow diagram, annotated with the steps shown in the game construction figure. In the EGTA methodology, we first define a set of strategies (the program documents of Fig. 4), based on heuristics or whatever, typically with tunable parameters. We then simulate profiles over these strategies, collecting payoff results in a database, and use this data to induce a game model, which we term the *empirical game*. Since the result is just a game model, we can apply standard solution concepts and algorithms.

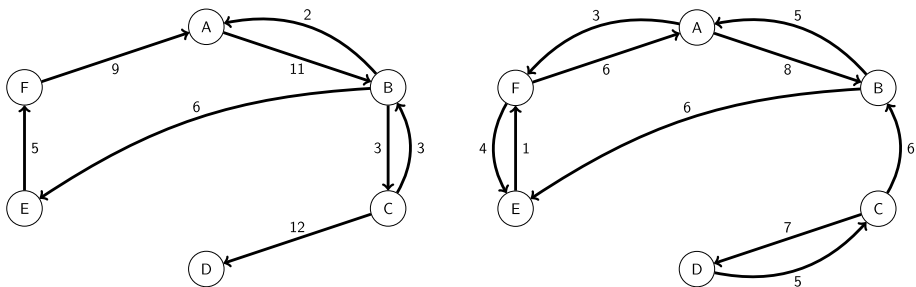
Let us now proceed to a series of examples of this approach, in an assortment of our recent research projects.



**Fig. 4** Constructing a game model via agent-based simulation



**Fig. 5** Empirical game-theoretic analysis (EGTA) process



**Fig. 6** Simple credit network. Initial credit relationships (*left*). Credit after the following transaction sequence (*right*): F buys one unit from E, E buys two units from C (routed through B), D buys five units from E (routed through all nodes)

## 5.1 Credit network formation

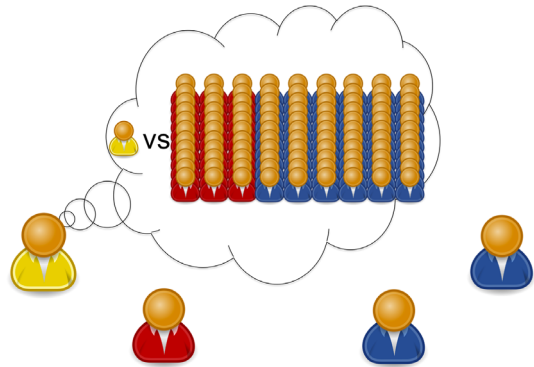
The first example is from work with Pranav Dandekar, Ashish Goel, and Bryce Wiedenbeck, on *formation of credit networks* [7]. The credit network formalism emerged from several researchers in the last decade as a way to represent distributed trust. A credit network is a weighted directed graph where the weight represents the degree of trust of one agent for another, in terms of its willingness to accept IOUs as payment for service.

Consider the example of Fig. 6. Initially, E trusts F for 5 units, as indicated by the weighted edge in the initial network at left. If F draws on one unit of that (i.e., purchases a unit of service from E, paying with an IOU), we update the graph by decrementing the weight by one, and creating a new edge from F to E of weight one (reflecting the fact that E now holds an IOU from F). A special feature of this model is the transitivity achieved by the ability to route credit over paths. For example, E can transact with C even though C has not offered E credit, by routing its payment through B, and updating accordingly. The network can also support more complex patterns involving multiple paths at once.

In a separate paper, Dandekar et al. [6] showed that this model is very effective at supporting distributed transactions, if we have a sufficiently well-connected network. So the question we ask in our study is: Will self-interested autonomous agents actually form viable credit networks? The reason this is in question is that deciding to issue credit entails



**Fig. 7** Correspondence between a 100-player full game profile and the payoff to the first player in a four-player DPR profile



a basic tradeoff. On the plus side, issuing credit facilitates transactions, which are profitable. However, there is a risk that the agent's counterparty will spend its IOUs then run away, leaving the agent with a significant loss. To answer the question, we undertook an empirical game-theoretic analysis.

I describe some key features of the game, deferring to the paper for full details. Our scenario has 61 agents, who decide how much credit to issue and to whom, based on various heuristic criteria. For example, an agent may decide to give credit to the nodes it considers least likely to default, or those most likely to engage in profitable transactions. We started out with 17 heuristic strategies from this space.

Even this modest number of strategies cannot be considered in any exhaustive manner. For an  $N$ -player game ( $N = 61$  in our case), and  $S$  strategies, the number of strategy profiles is  $S^N$ . Our game is symmetric, and that helps a little, but the profile space is still exponential in  $\min(N, S)$ . To deal with games of many players, what we actually do is *approximate* them by games with a much smaller number of players—in this instance six.

The idea of *player reduction* is that our simulation still includes all the agents, but the game model we generate is reduced. For example, suppose we have a 100-player simulation, and we want to model it as a four-player game. The issue is how do we take outcomes from the full simulation, and map them to payoffs in the reduced game.

Our specific technique is called deviation-preserving reduction (DPR), which was presented by Bryce Wiedenbeck at AAMAS a few years ago [27]. To get the payoff for player 1 playing the yellow strategy in the four-player game, we simulate a 100-agent scenario, where one agent is playing yellow, and the other 99 are divided evenly among the three other strategies represented in the four-player profile. In the example of Fig. 7, the other-agent context has 33 playing red and 66 blue. Similarly, player four playing blue gets its payoff from the situation where one agent plays blue and there are 33 other-agents each playing yellow, red, and blue. This abstraction yields dramatic complexity reduction, and we have shown in various experimental settings that DPR produces good game approximations, compared to other known player-reduction methods.

Now that we can build a game model from a feasible number of simulations, what would we like it to tell us? One question is: What kinds of credit networks will be formed in equilibrium? Under the environmental condition we call “global risk”, agents have common knowledge about the default risk of all other agents. In this model, if agents decide to issue credit to the most trustworthy agents, they will all give credit to the same individuals, producing a star-shaped credit network. On the other hand, agents following the same strategy under incomplete information about default risks produce very different credit networks. In our

“graded risk” model, agents’ beliefs about default risk come from experience, and they have more experience with those they are close to on a social graph. Therefore, if you decide to give credit to the most trustworthy person you know, it will probably be one of your friends—not because your friends are more likely to be trustworthy than strangers, but because you can distinguish trustworthiness among your friends but not among strangers.<sup>3</sup>

So what do we find? We ran EGTA on twelve environments, half with global information about default risk, and half with graded social-network information. Within each of those categories there are environments with high, medium, or low average default risks, and high or low average transaction value. In all of the global risk environments, we found some equilibria using default-based heuristics, leading to star-shaped credit networks. When information about risk is incomplete, the agents either do not issue credit (based on high default risk and/or low transaction value), or base decisions on transaction patterns. There are several interesting findings here, but for our current purpose the most important is simply the illustration of a systematic EGTA process revealing how various environments can induce varying agent strategy profiles, and therefore different system outcomes: in this case qualitatively distinct credit networks.

We also evaluated our original question: Will viable credit networks form? What we found is that when the potential welfare is high—that is, agents can profit significantly from a working credit network—the self-interested agents actually manage to form a network that produces the lion’s share of that. They do not get all of it, and the agents may fail totally to issue credit when the potential profits are small. But that is actually quite a sensible reflection of what happens in real-world network formation scenarios given network externalities.

## 5.2 Agent-based simulation and evolutionary stability

As the credit-network study illustrates, EGTA generally relies on hand-designed heuristic strategies or families of strategies. Its claims for principled behavior design are founded on its application of prespecified criteria to select among the heuristics, using evidence from simulation data. In EGTA, naturally those criteria are based on game-theoretic concepts. As noted above, ABM sometimes employs something quite analogous, appealing to *evolutionary stability* concepts.

The typical way that ABM studies employ evolutionary analysis goes roughly as follows. Starting with an initial population, the method enters an iterative cycle of (1) simulating population interactions, (2) evaluating fitness, and (3) selecting a next-generation population based on relative fitness. The problem is that this process maintains no information about simulation results or fitness evaluations from prior generations as it goes forward. It may often repeatedly evaluate the same agents, which is fine as long as the simulation-based fitness computation is trivial. However, I suspect that this framework has anchored the community on agents and environments that can be simulated or evaluated very cheaply, which has added an unnecessary constraint on agent complexity.

The alternative is to adopt an approach like I presented for EGTA, where simulation results are maintained in a persistent database. The cylinder of Fig. 5 is labeled “payoff data”, but of course payoff is just the game-theoretic term for fitness. Simulation of strategy profiles is cumulative, and so additional sampling just refines the resulting model. To generalize the EGTA framework beyond game-theoretic solution concepts, all we need to do is replace the “game analysis” box of Fig. 5 to apply evolutionary stability concepts or whatever other selection criteria one would like to adopt. The important thing from a methodological perspective

<sup>3</sup> If a random stranger seems more trustworthy to you than any of your friends, that’s rather sad.

is that the investigator has pre-specified the criteria for agent selection in the computational experiment. That allows any results to be tested and subsequently refuted if one finds strategy profiles that overthrow the study findings according to the investigators' own criteria.

### 5.3 Trading agent competition: Ad Auctions

Let us proceed to a couple more examples of using game-theoretic behavior selection to evaluate strategic agent response. The first is an example from one of the Trading Agent Competition (TAC) scenarios. When I first started conducting research on trading agents, one of my concerns was that our results may have limited credibility if my research group wrote all the agents involved in a complex scenario simulation. Fifteen years ago, Peter Wurman and I proposed an international research competition based on complex market scenarios, to get a community of researchers to generate strategy ideas, relatively unpolluted by our research hypotheses [26]. The first TAC was held at ICMAS-2000 [10]. The event has been held annually ever since, employing a variety of interesting market games.

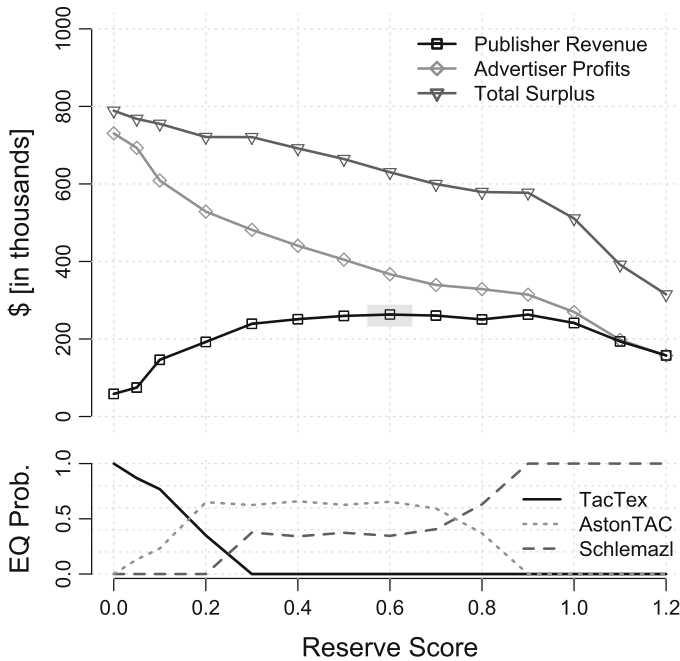
The study I highlight here is from the TAC Ad Auction (TAC/AA) game, a scenario based on bidding on keywords for search ad placement [11]. The tournament gives us one way to select the “best” agents, which are presumably the ones most relevant for evaluating impacts of market interventions. However, these agents were designed for the particular TAC/AA scenario. How could we use these agents to extrapolate to different market conditions?

For example, one important question in search advertising is how the publisher should set *reserve prices*: the minimum prices for showing ads. It is not legitimate to investigate this by varying prices while holding agent behavior constant, as the agents would surely react. It would have been difficult to persuade the TAC participants to rewrite their agents to cover different reserve prices, so instead what we did to address the question [12] was use the best agents from the tournament, and derived new equilibrium mixtures of these agents as the reserve price varies. As shown in Fig. 8, TacTeX [18], the 2009 winning agent, predominated in equilibrium for low reserve prices (including the settings used in the tournament). However, as we raise these prices, the other agents—which apparently did not tailor as precisely to the given setting—start to do relatively better. As we see here, advertiser surplus (the middle curve) goes down monotonically as reserve prices increase. The search publisher's revenue goes up for a while, then down, thus has an interior optimum. The increase to the publisher is less than the loss to advertisers, so overall welfare goes down as reserve prices increase.

### 5.4 Algorithmic trading

The final example is from my recent work on algorithmic trading. Over the last decade or so, autonomous agents have virtually taken over trade execution on financial markets, accounting for a large fraction of trading activity. The overarching question we are concerned with in this research is how algorithmic trading affects financial markets, and how we can design markets to work effectively in the context of automated traders. In one investigation, Elaine Wah and I studied a high-frequency trading technique called *latency arbitrage*, where high-frequency traders profit from price disparities across fragmented markets [23]. We found that this practice is harmful to market efficiency, and showed that moving from continuous double auctions (CDAs) to discrete-time markets improves overall surplus while eliminating the latency arms race.

Our second study addresses the more benign practice of *market making*, addressing the question of whether market makers (MMs) generally benefit investors by providing liquidity



**Fig. 8** Equilibrium mixture, publisher revenue, advertiser profits, and total surplus varying as a function of reserve-price settings

to financial markets. Preliminary versions of this work [24] were presented at this conference and workshops; I summarize the analysis here as a final example.

To see how allocative inefficiencies arise in CDAs, and the role of MMs in alleviating them, take this simple example. Suppose a market with four background traders: two buyers and two sellers. The buyers have values  $b_1$  and  $b_2$ , and seller values are  $s_1$  and  $s_2$ , with  $b_1 > s_1 > b_2 > s_2$ . Let us further assume for this illustration that the traders submit orders at their valuations. Suppose that the orders arrive at the market in descending order of value. Then buyer 1 trades with seller 1, and buyer 2 with seller 2, achieving a total surplus of  $(b_1 - s_1) + (b_2 - s_2)$ . The socially optimal allocation, in contrast, would have buyer 1 trading with seller 2, for a total surplus of  $b_1 - s_2$ . The difference between the optimal and achieved surplus is  $s_1 - b_2 > 0$ . We can attribute this loss to the vagaries of the sequencing, combined with the greedy matching implemented by the CDA mechanism. However, only one-third of the possible orderings of these bids (8 out of 24) would result in the optimal allocation, with the remaining two-thirds under-performing by  $s_1 - b_2$ . Now suppose there is an MM who continually maintains buy and sell offers in the auction, with a small spread between them. As long as the MM's offer to buy is within the interval  $(s_2, s_1)$ , and its offer to sell falls within  $(b_2, b_1)$ , then for this sequence of order arrivals, buyer 1 and seller 2 will trade with MM, and the allocation will be efficient.

The MM promotes efficiency in this example by providing *liquidity* to the market. In the absence of MM, when buyer 1 arrives, it has nobody to trade with. Seller 1 fills the vacuum and makes a profitable trade with this buyer, but at a price quite removed from that which would match supply and demand aggregated over time. An MM with quotes approximating this long-run price, in contrast, allows arriving bidders to trade at near prevailing prices.

Equally important, it prevents bidders who should not trade based on their valuations from doing so.

The question is whether this beneficial effect of market making generally prevails. We can easily show that it does not universally hold, by constructing instances where the MM simply siphons off surplus from background traders. We expect, though, that such instances are relatively insignificant or atypical, and so conduct an EGTA study to determine whether MMs improve welfare for investors in equilibrium in representative environments.

Our EGTA study explored a range of environments, differing on number of background investors (25 or 66), investor reentry rate (slower or at the same frequency as MM updates), time horizon, and variance of changes in the fundamental valuation of the security traded. For each environment, we evaluated two empirical games: with and without the presence of a MM. We included a parametric range of background trader strategies, extensions of the simple zero intelligence strategy introduced by Gode and Sunder [9]. We similarly considered parametric variations on a heuristic MM strategy, similar to that defined by Chakraborty and Kearns [3].

Not surprisingly, the background strategies in equilibrium varied by environment, and based on the presence or absence of an MM. We find, as might be expected, that MM presence generally improves market efficiency: in almost all environments, equilibria with MM exhibited greater total surplus (sum of MM and investor profit) than those without. However, whether the MM is beneficial *to the background investors* (i.e., whether overall surplus gain exceeds MM profit) is more ambiguous. In environments with impatient investors—those in thin markets (fewer background traders) or relatively few opportunities to trade with each other—the MM typically provides benefit. If the investors are patient, for example with long time horizons affording sufficient reentry trading opportunities, the MM is more prone to detract from investor surplus. With multiple competing MMs, the benefits to investors are much more reliable. The key lesson from this study is that the value of liquidity provision is not absolute, but relative to trader preferences and market conditions. Liquidity providers may bring great value in some markets, but for thick markets with patient traders, they may extract value unless held in check by competition.

## 6 Conclusion

Let me quickly recap these examples. In the credit network domain, we found that viable networks were formed in equilibrium—as long as there is sufficient profit potential—and that qualitative behavior is shaped by environment conditions. In TAC/AA, we found that the advertiser mix in equilibrium is shaped by publisher reserve pricing policy. And in the MM study, we were able to compare situations with and without market making, by deriving heuristic equilibria in each instance. The more fundamental point illustrated across these domains is that the EGTA methodology gives us a way to evaluate the impact of environment conditions on qualitatively interesting outcomes. I could exhibit many more examples, but I have probably already hammered enough on this nail.

By now it should be clear that “Putting the *agent* in agent-based modeling” is about joining the approaches of the AAMAS and ABM research communities. Understanding, predicting, and influencing multiagent behavior is a core AAMAS competence, and addresses real problems across many sectors of society. The ABM community would claim a similar competence, and trying to maintain technical distinctions between our enterprises seems to me a mistake, and untenable anyway in the long run. Bridging these fields will require a process of community alignment, as well as technical developments to facilitate integration

of methods and concepts. I have focused on one particular technical issue, namely the need to address the sensitivity of model conclusions to choices of agent behavior. My methodological suggestion is to adopt pre-specified criteria, and impose some discipline in the process by which search and analysis are interleaved to characterize the selected agent configurations. I illustrated this with my own work on empirical game-theoretic analysis, which takes exactly this approach using game-theoretic concepts for agent strategy selection. Of course, I would be quite pleased if some of you take away from this talk an interest in finding more about this line of research. But my greatest hope is that we will see advances from many of you bringing a range of alternative yet principled and disciplined criteria to bear on multiagent modeling, and expand the capabilities and scientific influence of simulation-based approaches to multiagent reasoning.

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