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PANEL DISCUSSION

# The Future of Agent-Based Research in Economics: A Panel Discussion, Eastern Economic Association Annual Meetings, Boston, March 7, 2008[1]

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## INTRODUCTORY REMARKS

**LeBaron:** Agent-based economics, and more generally agent-based social sciences, have been around in various forms for over 30 years. The advent of higher speed computing and new tools for the computational learning fields led to a major increase in activity in the early 1990s through today. Research activity continues to increase at the current time, but the field still remains somewhat of a "niche field" inside economics. Certain conferences and certain regions (such as Europe) are well populated with agent-based activity. However, at mainstream conferences inside the US one would have a hard time in finding agent-based researchers.

Why is this so, and what might be fruitful directions for the field to go in? One key problem that is often cited is that agent-based modelers have still not come up with a "killer app." This would be an economic model that is relatively simple, and understood by mostly all economists, but for which agent-based approaches give surprising, and hopefully empirically valid, results. Why hasn't this happened yet? I think there are several reasons.

First, agent-based modelers have tended toward economic realism by building fairly detailed and complex platforms to work with. In their recent book John Miller and Scott Page [2007] stress a methodology of building simplified computer experiments that produce sharp controllable results. There are two problems that have led to models which are often more complicated than needed, and these are related to the use of computers in economics. Researchers often feel that once you are using a computer you shouldn't skimp on model details. You might as well go all the way if you've given up on analytics a long time ago. However, simplicity is still useful in being able to interpret your computational results. A related issue is a feeling that if you make a lot of simplifications to get to a highly stylized model, then you ought to be able to get some analytic results. It is often depressing, but I find the usefulness of analytics disappears very quickly (even for representative agent rational expectations models). I think a world of more stripped down, but still computational, models will lead to more thinking along general principals, and cross model comparisons.

An interesting corollary to this has been the relative difficulty in getting institutions to appear from the "bottom up" in an agent-based framework. In the early days of computational modeling this was kind of the basic dream. A world would start out as a general soup of interacting purposeful agents, and from this we would see the emergence of recognizable socioeconomic entities such as governments and markets. This has proved much more difficult than originally thought, but it remains an interesting and important goal for agent-based models. Related to this is the general understanding of institutions and their importance in guiding economic decision making. A key benchmark has been the so-called zero intelligence model [Gode and Sunder 1993], where researchers concentrate on the institution alone, and agents are assumed to be as simple as possible (often random subject to constraints).

There is also much hesitation in economics about computer modeling in general. Some of this is extreme caution about new things, but some of this may have validity. Analytic theories have the advantage that anyone can read them, and push them around, and modify them as they see fit. This is not really true with computational models. This has been a known barrier for some time in the agent-based modeling (ABM) world. Questions about what software to use, how to distribute and test code, have all dominated discussions of agent-based researchers for quite some time. I actually think some new tools are starting to become available which may help in this area. In particular, the computer language Netlogo can be very useful for building small-scale models that researchers want to put out and have others play with. As a language it has many good features. It runs on most platforms. It is relatively easy to get it up and running, and is also pretty simple to program. Finally, it has excellent graphics and animation, which can be tied to very nice user interfaces. (User interfaces seemed like a waste of time to me 10 years ago, but now I'm not so sure. They may be an important part of selling agent-based models to the world.) Unfortunately, I don't think any computational platform is perfect, but I'm still hopeful more and better tools will continue to appear. Getting more people than a handful of very motivated Ph.D. students to use your code is a very important thing for this field.

Finally, I'm not sure whether we are all that good at the construction of heterogeneous agent models. It is not something that our skill sets are all that well developed to do. Well-crafted economic models form excellent thought experiments that you can't get out of your head. Theorists do not get at these models by random chance. Our abilities at constructing models of heterogeneous interacting agents are still primitive, and it will take time for us as model designers to make progress in this area.

## Validation

The most often discussed topic in ABM is empirical validation. General acceptance of these models will need some ability to align with and/or explain features of real-world economic data. It is clear that this is an important part of the ABM agenda, but the field should probably not become too obsessed with this.

Agent-based models share many empirical validation features with other economic models, and some discussions of validation often overlap with problems that could be given for all realms of empirical science. However, there are some special characteristics that are a little tricky. First, agent-based models do have many degrees of freedom. It is not just parameters, but the choice of entire learning algorithms is up to the researcher's discretion. Also, most of these models have been released from strict optimizing behavior. There are several ways to handle the degrees of freedom problem. One is to boost the number of facts that you fit. This simply pushes the bar higher. Agent-based models have the advantage of generating both micro and macro time series. One can then line up with cross sectional and time series results. You can also generate "perfect" data sets of panels, which could then be compared to some rougher imperfect panels from the real world. Another interesting area is to use human experiments as a kind of calibration area to get parameters for use in the agent-based models. Fit learning algorithms to experimental data, and then take these algorithms in to a larger scale computer model.

Agent-based simulations often have many interesting time series features. Non-linearities and chaotic dynamics make them very interesting to study, but also make them difficult to estimate and work with. Also, they may exhibit path dependence and non-ergodicities, which again cause serious difficulties for estimation. Finally, many simulations look at interesting ways in which agents interact with each other either in space or through social networks. Both of these bring new empirical challenges, since they are not part of the standard econometric toolkit we are used to.

Our interpretation of empirical exercises is probably most effective for realms where there are other more traditional competing models available (such as finance). A kind of empirical race between the different approaches can be very useful. However, there is no well-defined way to declare a winner. Finance is an interesting case of this where fitting empirical features requires fairly complicated preferences for a representative agent, but agent-based models can fit many features with much simpler preferences inside a multi-agent learning dynamic. There also may be features (such as trading volume) where standard models have nothing to say, but the agent-based model provides useful information.

Finally, it is important to keep data fitting issues in perspective. We often learn more from wrong models than from carefully tuned ones. If a biological modeler were trying to simulate evolution, should all the models that didn't generate the appropriate distributions of life forms on Earth be thrown out, or is something learned from the failures about the evolutionary process. Some models that are far from the data can still be important as counter-factuals, and interesting thought experiments.

## Policy

How will these models be used for policy recommendations? They probably will be used at two extremes. First, as parts of large-scale computer simulation systems. Examples of this are already in use in some governments and government agencies. These are large and are very complex. One example would be traffic simulations, which can be programmed at a very realistic level of detail. However, I think policy will also be influenced by simple stylized models of small to intermediate size. I think agent-based models will be much more likely to play an advisory role rather than as large macro models estimated and run on the entire economy. Also, they may have much to say about institutional design. An example might be the current credit crises. Agent-based simulations could make predictions about systemic risk in credit markets, but to do this well they might need a lot of confidential information on cross holdings. Without this confidential information they may be useless in predicting a system crash. However, they might be able to predict the stability of various trade networks, and which types of cross holding networks form under different policy regimes. Related to this, their big strength might be to advise during periods of stress, when various markets are not well approximated by standard equilibrium relationships.

**Chen:** The theme of this panel is the future of agent-based research in economics. One main issue that concerns most of us is: can agent-based computational economics become a part of the mainstream in the future? Maybe we can amuse ourselves a little by constructing an agent-based model to do the forecast, hence the title of my discussion, "An Agent-Based Model of Agent-Based Economics." As a simple start, we may consider the familiar two-type model, such as the fundamentalist-chartist model in finance. This model can be formulated into a jump Markov process, and the solution to the resultant master equation shall be the answer to our concern. The only thing we need to know about the jump Markov process is the *transition rates*. Roughly speaking, we have to know how likely it will be for a non-agent-based economist to convert to agent-based research, and how likely it will be for a cadet to initiate his career using agent-based models.

A number of determinants have already been mentioned in the list of questions submitted to the panel, such as job opportunities and *research publicity*. Using these performance-based criteria (fitness functions) is very standard in agent-based economic modeling, so they should be readily included into the *transition rate function*. However, there are also utility-based criteria. Familiar psychological impacts, such as *herding*, may also be taken into account. Other than that, I want to single out another three equally important utility-based criteria, which received relatively less attention during the past discussion of the transition rate function.

The first one is *beauty*. One attribute of beauty is whether we can conceptually harness what we are modeling. Of course, a parsimonious model is easy to harness. Another attribute of beauty is whether we can expect the unexpected, that is, novelty and surprises. The success of Thomas Schelling's [1978] *agent-based segregation model* is that it has both of these attributes of beauty and thus has become a classic in the ABM literature. It is unfortunately true that many current agent-based models are "notorious" because of their large number of parameters. Nonetheless, complex models are not necessarily "ugly," if it has a *modular design* [Simon 1965]. Modulization is a powerful "decoration" for complexity. It allows the users or the prospective followers of the model to take *incremental procedures* to re-display or re-examine the models so that the transparency and comprehensibility of the model can be enhanced. Gode and Sunder's [1993] zero intelligence agent serves as a good illustration of such a design.

The next criterion is *mobility*. Mobility has two parts. The first part is the mobility of our mind about a given model or issue. Each model or issue has its boundary, partially defined by its assumptions, parameters, etc. Many times we have the desire to know what will happen beyond the boundary. For example, what would be the lessons if the CARA type of risk preference is replaced by the CRRA type? Mobility measures how easy we can move to these different scenarios. One advantage of the agent-based model is its readiness to simulate many very different "*what-if*" scenarios, as well as to conduct robustness checks or sensitivity analysis. This highly mobile environment is very beneficial for us to conduct thought experiments. The second part is the mobility across different disciplines. We now have evidence that ABM is not just a language uniquely owned by economists, but is also a language widely shared by other social scientists. The driving force of *computational social sciences* is, in fact, ABM and simulation. In addition, ABM's emphasis on agents or *software agents* has also increased its exposure to behavioral economics and experimental economics. The latter's state of art is no longer a lab with only human subjects, but a lab comprising both human agents and software agents. New theory and tools applied to agent engineering further bring in ideas from cognitive sciences, neural sciences, and artificial intelligence. Maybe in the future we can have another panel on "*The Future of Economics in the Agent-Based Interdisciplinary Era.*"

The last criterion is *freedom*, that is, the capability to maximize the enjoyment of doing research. Agent-based tools free us from the usually stringent analytical constraints so that we can address either the same questions using much more relaxed assumptions or new questions, which can hardly be reached under the conventional constraints. According to my personal observation, many economists who invest in agent-based research do so largely because of this consideration.

I can certainly add more determinants, such as the technology of the ABM, to the transition rate function, but I am afraid that would cause the resultant master equation or the Fokker-Planck equation difficult to solve. What is the future of agent-based research in economics? Maybe we want to keep the curiosity instead of making a hasty prediction. After all, the study of the complex systems from John Conway's Game of Life to Stephen Wolfram's cellular automata are all filled with interesting unpredictable patterns. Can agent-based research become a dominant approach in economics someday? Want to bet?

**Sunder:** In these remarks, I shall assume that ABM has economics as its end objective and end result. Economics is a social science which concerns the behavior and properties of communities or institutions populated by real live human beings. How does ABM help advance economics? What are these advances and what could they be?

I believe ABM has contributed, and can continue to contribute, to economics in spite of the fact that the ABM label itself emphasizes neither the human nature of the agents, nor the communitarian nature of economic phenomena.

To the contrary, I shall argue that ABM's contribution to economics arises precisely because of these differences. But it is important that my perspective on ABM is one of the contributions to economics through use of this technology and not on this fascinating technology itself. I understand that for many scholars, the ABM discipline is of deep interest in itself for many reasons.

Allow me to use a parallel to clarify my point. Like ABM, statistics is a deep discipline with a long history and extensive literature of its own. Application and use of statistical reasoning and modeling to economic questions has contributed greatly to accomplishments of economics; and these contributions have been widely recognized.

In addition, attempts to use statistics for addressing substantive economic question has led, over the past century, to the evolution of a new cross-discipline of econometrics which has developed a tradition and extensive literature of its own.

Today, statistics, econometrics, and economics coexist with parallel, partially overlapping yet distinct identities. While there are plenty of scholarly contributions which could go either to economics or to econometrics journals (and the same is true of econometrics and statistics journals), the same is not true for economics and statistics. For economics, statistics is and will remain an instrument of research, no matter how valuable its applications to economics become.

The same is also true of economics and mathematics with mathematical economics being the bridging sub-discipline between the two. I am not qualified to assess the contributions of econometrics and mathematical economics to statistics and mathematics, respectively. However, it is clear that the importance of these bridging sub-disciplines to economics arose from their contributions to substantive problems of economics.

Perhaps it is not inappropriate to think of a parallel relationship among economics, agent-based economics, and agent design. The last of the three draws from, perhaps even lies substantially in, the domain of computer science and artificial intelligence, and draws on their knowledge base and technologies.

Agent-based economics could be thought of, like econometrics, as a bridging sub-discipline between ABM and economics. As a specialized branch of ABM, agent-based economics focuses on agent models developed specially to address the problems of economics. When we use ABM to address problems of general interest in economics, they are contributions to economics itself.

The future of ABM in economics will depend on our ability and willingness to address substantive problems in economics. Statistics and its economics specific branch econometrics have found an important place in economics, not just because they developed better estimators and discovered their properties (that is development of the method itself) but because they were better able to estimate, for example, the effect of education on productivity of labor.

I think it is reasonable to say that the general body of economists has a similar attitude to ABM and other methods. The future place of this method depends on contributions of the ABM technology to address substantive problems of economics as a social science. The more successful we are in this endeavor, greater will be the acceptance of the method in economics. In making this assertion, I have said nothing new; because this applies to all disciplines.

So, what is the general area of economics to which ABM can make substantive contributions?

Physical sciences deal with discovering the universal laws of nature that apply across time and space and concern the behavior of inanimate objects or symbols [see Sunder 2006]. At the opposite end from the Science Hill on the Yale campus lie the humanities departments of literature, religion, philosophy, art and music, etc. Literature looks not for universal laws that govern the behavior of humans but eternal truths about our nature. Even though each human being is unique, endowed with free will to do as we wish, yet the eternal truths of love, hate, courage, greed, jealousy and fear appear repeatedly throughout human history and literature.

Social sciences try to create a space for themselves between the sciences and the humanities. Since the object of study in social sciences is our own sentient selves, we remain uncertain about the ground under our feet. We are not quite sure of exactly what we humans are.

On the one hand, we wish to have the honor of being a science and accordingly we seek universal laws that might explain and predict what we do. This pursuit leads us to model ourselves as a stone rolling down the hill under external force of gravity or a leaf blown about by wind. For the stone, the leaf, as well as the *homo economicus*, universal laws applied to fixed characteristics of the objects of study help us understand what happens to them.

On the other hand, we are reluctant to believe that we are like a rock or a leaf, and let go the belief that we have free will to choose what we eat, and where we go. Is our behavior simply driven for external forces and our own predefined properties?

Unfortunately, free will so essential to our sense of self, and universality of laws we seek in order to become a science, do not mix well.

That is where the "social" part of social science comes to help. It is possible that even as individual free will may preclude the possibility of predicting individual behavior, aggregate level outcomes of larger groups of individuals may be subject to fixed and discoverable laws. It is this possibility that holds a rich promise of substantive and substantial contributions of ABM technologies applied to the problems of economics as a social science.

As our friends in psychology examine individual behavior, ABM has already yielded some interesting results in identifying systematic properties of market institutions populated by simple agents.

So my hope and expectation is that ABM can and will flourish as a method of making contributions to problems and economics by serving as a bridge between individual behavior (the domain of psychology, and captured by ABM technology) on the one hand and aggregate outcomes (which is the primary topic of interest in economics) on the other.

In summary, I am optimistic about the role of ABM in economics as a method of addressing substantive problems of economics — especially understanding the properties of social and economics institutions populated by agents of various kinds — something we can design, manipulate, and examine using agent based models (ABMs).

## QUESTIONS AND ANSWERS

**Leanne Ussher**, Queens College of the City University of New York: As part of this session, we invited economists and other scholars to submit questions for the panelists.

From Andrew Lo, Massachusetts Institute of Technology: *"To scholars in the non-agent-based community, what do you think are the top three insights that have come out of the agent-based literature which could not have been obtained through any other means?"*

**LeBaron:** For financial markets most of their key insights have been empirical. There are certain features of financial time series for which standard models offer few explanations. These include fat-tailed distributions or persistence in volatility and the dynamic properties of trading volume. The standard financial models remain very quiet on these and yet agent-based models pretty much replicate these features, and do it with relative ease. What I think is surprising about this is they are extremely robust across many different styles of models. Outside of the ABM world, you'd be very hard-pressed to get these kinds of features. A second insight into financial models is related to heterogeneity and its contributions to how some of these features evolve, I find that many of them are connected to the heterogeneity of the strategies that are in play in a given time. Unfortunately this feature is very hard to quantify.

**Chen:** Yes, I also considered three, but the first one is exactly the same as Blake said about agent-based computational finance, so I think I will skip that one. And the second insight is to make us know more clearly about the relation between micro and macro, or even in an approximate way. Thomas Schelling's segregation model is an early example, but we can move further from just simulation to a little bit more analysis. I think nowadays we already have some analytical tools to help us understand the micro and macro relations in a bit more precise way, such as stochastic aggregation, the Fokker–Planck equation, bifurcation and chaos. It's good news for ABM.

The third insight is surprises, novelties, and unpredictability from models. I think most economists are not used to this feature, but this is what one should have if our model loyally represents reality, and we often expect the unexpected from the real world. Can we also expect some unexpected things from our model? I think most mainstream models would not allow us to take this liberty, but agent-based models may be inherently unpredictable and may have this potential. This is certainly useful in particular when we are modeling innovation. See, for example, Chen and Chie [2007].

**Tassier**, Fordham University: Our next two questions come from Rob Axtell, George Mason University. They both deal with comparisons of neoclassical models and agent-based models. *(1) "To date, most agent models have departed in significant ways from neoclassical models. Has this been the most useful approach, or would it be better to have agent models first reproduce neoclassical results and then make systematic departures?" And related to this (2) "Many agent models demonstrate that neoclassical results are unrealistic and/or unlikely to be realized in the real world. What is the best way to present such findings?"*

**LeBaron:** Actually in my own research agenda I stay close to neoclassical models; for some agent-based people I'm too close; and they feel like I'm a secret spy. I think there's one key thing to that and that's in the sales to the rest of the world. Using neoclassical tools is somewhat helpful. If you can get all of these amazing results using a completely amazing tool set then the rest of the world may not be that impressed. They will be much more impressed if you are using tools that they are familiar with to do really amazing stuff. I don't think the actual parts of the neoclassical toolset are all that bad. Once they are opened up to heterogeneity many interesting things are possible. For me, it's also good for software testing; it's nice to have someplace where you know the models are supposed to go. Then you can put the parameters in a region and make sure the model does what it is supposed to do. Eventually, as we get better with these models, we can release some of the connections to neoclassical models. For now, I have to stay pretty close to it. I wouldn't demand that from others; but I demand that from myself.

**Chen:** It depends on whether the two can be correlated in some ways. If they do, then it would be a good strategy to present the agent-based model as the extension or generalization of the familiar neoclassical economic model. Doing it in this way will facilitate the communication between agent-based economists and neo-classical economists. I call this strategy a "projection strategy." In fact, you go back to see the early development of agent-based economic models, such as the agent-based cobweb models, agent-based overlapping generations models, etc., they are all models built by this strategy. Nevertheless, if the two are completely orthogonal to each other, then this strategy is neither applicable nor desirable. For example, if one wants to simulate the large picture of human history or the society of ants, there is really no good neo-classical model as a benchmark.

**Ussher:** These are from David Colander, Middlebury College, *(1) "Since there are so many arbitrary assumptions in any agent-based model, how does one know when one can pull insight from the model, and not see it as arbitrary?" and (2) "What standardized agreed upon procedures can be established on* ABM *that would allow one to separate out 'useful' agent models from arbitrary ones?"*

**Chen:** I think his first question is a little subtle and I'm not quite sure that question has been put in the way that I perfectly understand. Earlier I mentioned that we should consider agent-based models as complex systems, and the research strategy to follow is modular design. This allows the modeler to have an incremental procedure to understand this complex model little by little. The incremental procedure can make us handle both the questions raised above, including the parameters and the dimensionality of the model.

**LeBaron:** Obviously in many ways there are some empirical issues that we can try to pin down. I mentioned some of these in the validation section. One grand hope is to start linking experimental results at the micro-level with some of these parameter choices; learning algorithms are not that arbitrary, and so that's a possibility. I also agree with the modular design approach and certainly learning algorithms may be able to be broken down in the future. Learning algorithms have often been giant black boxes, but they can often be pulled apart into certain key components, usually involving fitness, and search rules. How do you evaluate good strategies? How are you comparing different strategies? How far around the world of possible rules are you looking, and how frequently are you updating things? I find those three parameters are often sort of the key ones that impact model dynamics. The details of the genetic algorithm often don't matter, but these three components of the learning algorithm often do matter. As we move along we may yet be able to better compartmentalize the world of adaptation itself. We may be able to write in a small number of components, which address the sensitivity of these different things.

**Tassier:** Our next question comes from John Duffy, University of Pittsburgh. I would like to ask his question and follow it up with a related one of my own. *"Current publication formats seem ill-suited to communicating findings from agent-based models. Yet publications in journals and books remain a mainstay of academic success. What is your advice on effectively communicating findings from agent-based models?"* I add to that, since publication issues are especially important for graduate students going on the job market and young economists looking to get tenure, *"What advice do you have for graduate students interested in ACE methodologies, and how can graduate students use ACE methodologies to best sell themselves?"*

**LeBaron:** First is the sales component for agent-based work. Obviously we have a huge computing component to almost all of these models. Distributing software for these models is critical. Of course, the software is going to go along with any kind of journal publication; the code has to go out with it. Just as you want anyone to play with a theory that you put out, you want this code to go out with your publication. Of course with the web, we're totally capable of doing that. Some journals are capable of publishing the code on their own. I know a few that can. As I mentioned before, one of the problems is that there are so many different languages. We have to live with that for the moment. We need to understand that distributing the code is a very critical and necessary thing, and maybe we can congeal on some of these platforms that are very easy to run like Netlogo. I also think that if you want people to play with your code, just distributing your code is not enough. If you distribute code, you get some very energetic Ph.D. students somewhere trying it out, but you won't get a general population. Again, Netlogo lets you very easily build a web-browser based version of your model. Then other people can sit there and play with the dials and switches. And that's really what you want. People will see the interactions and what happens with your model, and that can be critical for publicizing it. We have the web capability to do that kind of publication now. How that gets combined with the journal process, I'm not quite sure. But it's very necessary.

Then you mentioned, what trajectory should new students and people trying to get publications out follow? That's a tough question, because the general economics world is still very hesitant about this endeavor, and computer modeling in general. I think the many things we've mentioned before are all important; pushing simplified models and again staying close to a neoclassical setting. Model tweaking of some neoclassical result can be useful, even though that may not be the greatest thing to do in the long run. If you are an experimentalist, experimental work along with agent-based stuff is more accepted I think. There are some journals that are generally a little more accepting of agent-based work than others; *Journal of Economic Dynamics and Control* and the *Journal of Economic Behavior and Organization* are now quite accepting of this. It's still hard to get things into the main top journals.

**Chen:** In addition to what Blake has already said, I think the hard thing is that a top journal will require you to give insight for the phenomena that you have generated. I think that because many agent-based models are not built in the modular way, it's hard for the author to actually dig down and trace all the possible roots to find out what the mechanics are and to find what kind of insight accounts for some interesting phenomena that was generated. So I think as Blake already mentioned there are some more sympathetic journals. It depends on how hard the author would like to be pressed to find out the exact mechanics for the insight.

**Ussher:** I want to ask my questions because a lot of people say zero intelligent agents *a la* Gode and Sunder aren't really zero intelligence, since there is some optimizing component there, in the sense that one can say they're satisficing. Or some might say that by re-trading they are actually optimizing. The questions are *(1) Is the best approach to go with "zero-intelligence," or is along the lines of mechanism design, should we have policies that incentivize agents to make their behavior endogenous to the system? Or is it better to look at institutions assuming this zero intelligence or random behavior? The second question is (2) Can we truly separate institutions and the environment from agent behavior, especially in that wealth is in the zero intelligence agent endowment, and it changes; and if we can, is therefore zero intelligence the best way to go? Should we use this as a benchmark for all our agent-based models to set up the priors to see how the institutional framework is affecting the outcome, and then add on components to the behavior of the agents?*

**Chen:** Yes, I totally agree. Agent-based models are composed of two parts, the embedded surroundings and the agents. Studies which focus on the adaptive behavior of agents (reinforcement learning, genetic algorithms learning) with a given surrounding, for example, an auction design used in the electricity market, are equivalent to a robustness test of the specific design, because you actually test your design with all kinds of possible agents. On the other hand, studies which focus on different designs with the same types of agents — be they rational, bounded rational or zero-intelligence — are closer to conventional analysis of market design; basically they don't question too much about the agents, just the design itself. However, the same design may perform differently with respect to agents with different intelligence or culture. A design that runs very well in one country may fail in the other one. So, both institution and agents can matter. Given that they are two variables instead of one, we should not be confined with a "partial derivative" approach, but a "total" one. Of course, total derivatives cost more time and money since we are running more simulations.

About the suggestion on benchmarks, I also agree. As I mentioned in my introductory remarks, what we need to manage the potential complexity of the agent-based model is to use a modular design, which allows for an incremental procedure to examine the model. Also, as what I have said earlier, the total derivative analysis is preferred to the partial derivative one. It provides us a balanced study of mechanism design using agents; hence, in that sense, I would not suggest to separate the environment from agent behavior. Their combined effect should not be excluded a *priori*, which means that if we can use zero intelligence for the benchmark part, then on the institution side, we may start with some benchmarks as well.

**LeBaron:** I will say a couple of things. I will try to go between question one and two. I have some things to say about institutions. I have often thought that ABM was almost going to lead to a rebirth of institutional economics because we actually have to have an enormous amount of institutional detail, even in relatively simple models, we really have to talk about exactly how people are trading. And the zero intelligence stuff has also pushed the institutional issue to the forefront. I think when we look at behavior we are looking at behavior that is very much constrained by institutions; and the weighting we have put on rationality *vs* institutions in the past has weighed too heavily on rationality. An interesting book on this is *Being There* by Andy Clark [1997]. There are some psychologists out there who talk about how these institutional constraints limit the choice sets. In some cases — and this is what is bizarre to economists — limiting the choice set can sometimes be a good thing. I think modeling institutions in finance is fun because at the trading level it is pretty easy to get the institutions right, such as the various order books. Electronic trading systems have changed the world for us in finance because I think that institutions are pretty easy to model. Institutions in the rest of economics get trickier.

In the early era of ABM, the hope was actually to evolve institutions. The dream was to send a bunch of somewhat semi-purposeful agents out there in the computer and come back a week later and to see firms and banks and stock markets. That has been a dream, unfortunately, that is way too far out. Thinking about evolving organizations is a really interesting question but very hard.

Your second part was on the zero intelligence benchmark. I totally agree with this. I think that's what zero intelligence does in this world. It's not always the best model for modeling behavior because I think people do more than that in some contexts, but it is a very good benchmark to look at, starting with the simplest possible agents and see how they do. If you say that we end up in a lot of situations where zero intelligence guys do just as well as anybody else in terms of replicating features, we may have some trouble.

**Tassier:** One area that I think has not been exploited as much as it could be is the synergies between agent-based economics and experimental economics, I would like to ask a question that Blake has posed himself, "*How should* ABM *be tied to the related fields of experimental economics and behavioral economics? What can be learned from progress in these fields?*"

**LeBaron:** I'm not an experimentalist but I do agree with this interaction. There are some people who will run an experiment with a few people in the room and then go out and run it with a million computer algorithms to see the comparison. I think it has really contributed a lot to our understanding of the world. Most experiments are looking at worlds with some element of learning going on. The subjects are clearly changing and adjusting over time as they try to figure out these experiments. There may be a rationality benchmark, and sometimes they get there, sometimes they do not. And you are looking at this world with lots of imperfect behavior. That precise theoretical point of rationality is kind of nice, but you are often not there and you want to understand where these behaviors are coming from. Agent-based models can be very useful for helping to understand what it is about the learning process that might be going on. So I think they have been, in my small reading of it, useful. I think some experimentalists, a large wing of experimentalists, are just wedded to doing experiments. And that's okay. I haven't done any experiments; many of us in agent-based finance, with the exception of Cars Hommes who has done a little bit of this, are guilty of not using the experimental results to a larger extent. Very few people in the finance world have pulled some of these experimental results off the shelf and used them in various financial markets, as a kind of micro-basis for the things we're doing.

Finally, here's a very radical idea on experiments — an interesting thing that I've only seen a very few people doing. Here's another test in the realm of tests that are not standard empirical tests of agent-based types. It is what is called a "Turing Test" of artificial intelligence. Can you tell if you are interacting with a person or with a computer terminal? A Turing Test for experiments is that you can populate your experiments with a combination of computer agents and human subjects, and either you, looking at the data later, or a human subject in the experiment cannot tell the difference. Is trader A a human or a computer? That is very interesting. I've only seen one group try that. That is a group at a research lab at IBM, they're calling them ShopBots. It's an interesting and very different style of testing in term of experiments. Actually mixing computer and human subjects in the same experiments and seeing what happens.

**Chen:** I would say that experimental economics is a kind of agent-based economics, which uses only human agents. On the other hand, agent-based economics is a kind of experimental economics, which uses software agents instead of humans. Human agents can differ from software agents in one important parameter, that is, their degree of autonomy. Assuming that you design a software agent, then, implicitly or explicitly, the different degree of autonomy is limited. Now, if you can use a human agent, then his degree of autonomy could possibly be large, but basically unknown. Hence, it is a new research area if we can replace software agents with human agents in ABM since we now have a different control of this autonomy parameter. In this case, we can consider human agents as classes of agents with a different degree of autonomy. In fact, this thinking is consistent with the recent trends in natural computing. In natural computing we recently have experienced the use of nature processes to facilitate computing, such as chemical computing or biological computing (DNA computing, RNA computing). The former integrates chemical processes into computing, whereas the latter integrates biological processes into computing. In this vein, one should take advantage of human agents as part of the agent-based computing environment.

As I have mentioned in my general remarks, a laboratory that can integrate human agents and software agents may open a new agenda for experimental economics and agent-based economics. In an earlier stage, agent-based economics was used to replicate the results of experiments with human subjects. This development can be traced back to Arifovic [1994]. In this kind of study, you have the experimental outcomes on the one hand, and you ask whether the outcome can be replicated using agent-based economics. Blake just mentioned the Turing Test. But this mirror-only relation changes. Because you can have markets comprising these human agents and software agents and you may ask whether the human agent behavior will be different if they learn that software agents are competing with them.

So this is kind of a new question, which did not happen in the early stage. Keep on moving, you can have something like collaboration instead of competition. In this case, human agents can have a few software agents under their control and work for them. What kind of software agent would they like to recruit and what determines the recruiting decisions? Actually the whole thing is evolving, once we merge the two we are going to face some new questions that have never been addressed before.

**LeBaron:** One last thing on behavioral economics, the list of quirks that we have from experiments is pretty long. What we don't know as macro and finance people is just how well these behavioral quirks aggregate up. Agent-based technology is perfect for trying to figure out whether things like loss aversion still show up at the aggregate level, even though we certainly see them in the laboratory in individuals. That has not really yet been done, but I think that will be a big cross use between experimental and agent-based research; figuring out which behavioral things matter to macro and finance, which ones aggregate up.

**Ussher**: We can now open it up to questions from the audience. Are there any questions about the future of research in agent-based economics? I have another question. *Excluding Schelling, who won the Nobel Prize, when is* ABM *going to be recognized by the Nobel Prize committee? What year? How far out does it go? Soon, or decades away?*

**LeBaron:** Decades. I don't think never. I think it's interesting, if you read all the commentary on Schelling, his agent-based model is not even mentioned. I found that frustrating. You can find more easily that he was the science advisor on *Dr. Strangelove* than you can find that he had written a book on agent-based economics. I think it's going to happen, but it will take a long time.

**Chen:** The year 2005 when Thomas Schelling won the Nobel Prize was just the right time because it is time ABM moves to a burgeoning stage, which should more or less endogenously generate a Nobel Prize Winner. {AUDIENCE LAUGHTER.}

**Joyce Jacobsen**, Wesleyan University: *If you were advising a promising undergraduate who wanted to get into* ABM *in economics, where would you tell them to go to graduate school?*

**Chen:** A few students have asked me this question, because our university is the one in Taiwan that promotes agent-based computational economics. One of my undergraduate students is very interested in agent-based finance, so I recommended him to go to Brandeis. {AUDIENCE LAUGHTER.} Really! I'm not joking. There are already a few leading scholars at different universities, even though there is no complete research program designed for the education of agent-based economics. At this stage, we should be satisfied with finding a good master who can advise the young generation on their Ph.D. theses. So, if there is another student who would like to do research on agent-based finance, I will recommend him to Blake.

**LeBaron:** I get this question a lot. In the US we are kind of spread around a lot. At Brandies I am a lone entity. It is a little dangerous to send someone to one place that doesn't have more than one person. There are a couple of places that have critical mass. There are some regions; New York City has a critical mass. Michigan has always had a pretty active crew there. Actually George Mason is trying to build up an operation. So there are a few places that are trying to have more than a small amount. Europe has some big operations. Cars Hommes runs a big operation at the University of Amsterdam. Around Europe you will find some other teams in various places.

**Ussher:** In my own experience, being in Europe recently and going back there, is that there is an enormous amount of government funding; there are many institutions that have people doing this kind of thing, interdisciplinary. In the US you have a few isolated academics that have people following them, but it's too dispersed.

**Philipp Harting**, Bielefeld University: For example, the research project we are working the EURACE project, funded by the European Commission. This project includes a lot of research units all over Europe. For example, in Italy Mauro Gallegati is a member of the team. Herbert Dawid is a member of the team. In Europe this type of modeling approach is much more accepted I think than in the US.

**Ussher:** *Do you have funding of Ph.D. students?*

**Harting:** For example, we are Ph.D. students and we are on scholarship from the European Commission.

**Chris Reubeck**, Lafayette College: *Maybe the panelists can comment too on the idea that in the US there is a stronger base of just being wed to neoclassical economics, where outside the US there is not bias toward neoclassical economics.*

**LeBaron:** I totally agree with that. That's part of it, but the other part, which people have mentioned, is funding. The EU I think is very different with that.

**Reubeck:** In terms of getting into journals, Professor Chen mentioned that the idea that the major journals want you to draw some fundamental insight that is more difficult to draw. I think that is where my bias is against ABM. *How is it that we can gain more insight? What can we do to answer that call? I think there are good reasons for these journals to ask for the fundamental knowledge from the papers they get. How can we do that?*

**Chen:** You mean to argue with the editors?

**Reubeck:** No, I think the editors are right.

**LeBaron:** He's taking the counterpoint on that. I think I was trying to discuss this in my talk on modeling methodology of pushing toward simpler models. Also, be a little less intent on the empirical side of things. Early on there was a stress to hit every empirical number out there, and at the expense of giving up simplicity, which allowed deep insights. Again, concentrate on the economics of the situation, and don't get too overwhelmed with computer technology. I still believe I spend 95 percent of my time on economics. I think that is the right proportion. Think about the economics of the situation. Try to think about the traditional approaches and then kick yourself out of the box as best you can. If you have a standard Ph.D. training its hard to kick yourself out of the box. But try as best as you can to realize that the standard approach may be wrong. Hopefully you can then figure out how to push it with agent technology.

**Ussher:** I want to question you about having to start from the neoclassical position because it is saleable and has already been well thought out. *But in the heterodox community, there are already many results, without the* ABM, *that are simply more or different type of results, that are riddled throughout heterodox thought that are ripe for choosing, they are sort of perfect for this kind of area, and if we want to one day go to Europe and get a job and get funding, that would be thing to do. By remaining neoclassical, we isolate ourselves, by appealing to the mainstream and their not liking our results.*

**LeBaron:** It's definitely going to be a tougher sell, and tougher making inroads to acceptance using a heterodox approach. Right now to get your foot in the door, neoclassical is probably the best way to proceed.

**Chen:** We must use neoclassical economics in a constructive way. Certainly, for the marketing purpose we may like to connect our work to some familiar neoclassical baseline models. In addition to that, as I mentioned earlier, without using modular design, one dangerous thing is that if we move too fast, we may be easily trapped in a difficult situation where we may lose the cause-and-effect chain. Hence, we are just simply watching things happening, but have no insight gained from them. Modular design can help us back up some kind of observations before we totally lost the sight. Nonetheless, even under very simple agent-based models, we still need some good intuitions that provide us with the base to understand and expect what may happen. We also need them to guide us on the direction to scale-up the agent-based model. On these regards, neoclassical economics would help.

## About the panelists

**Blake LeBaron** is the Abram L. and Thelma Sachar Chair of International Economics at the International Business School, Brandeis University. He is a research associate at the National Bureau of Economic Research, and was a Sloan Fellow. Professor LeBaron has also served as Director of the Economics Program at The Santa Fe Institute in 1993. LeBaron's research has concentrated on the issue of non-linear behavior of financial and macroeconomic time series. His current interests are in understanding the quantitative dynamics of interacting systems of adaptive agents and how these systems replicate observed real-world phenomenon.

**Shu-Heng Chen** is a professor in the Department of Economics of the National Chengchi University. He now serves as the director of the AI-ECON Research Center, National Chengchi University; the editor-in-chief of the journal *New Mathematics and Natural Computing* (World Scientific); and an associate editor of the *Journal Economic Behavior and Organization*. He has also served as an associate editor of *IEEE Transactions on Evolutionary Computation*. His research interests are mainly on the applications of computational intelligence to agent-based computational economics and finance.

**Shyam Sunder** is the James L. Frank Professor of Accounting, Economics, and Finance at the Yale School of Management and Professor in the Department of Economics. He is an accounting theorist and experimental economist. His research contributions include financial reporting, dissemination of information in security markets, statistical theory of valuation and design of electronic markets. He is a pioneer in the fields of experimental finance and experimental macroeconomics. Sunder has published six books and more than 150 articles in the leading journals of accounting, economics, and finance, as well as in popular media. He is a past president of the American Accounting Association and a winner of many research awards.

## Notes

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