Adaptation and Networks

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DISCUSSION: ADAPTATION AND NETWORKS

M. HEANEY, The University of Chicago, Moderator

Michael Heaney: I'd like to open this discussion by saying that, after attending the panel, one thing about the presentations deeply disappointed me, and that is that Jonathan Bendor is not here in person to serve as the lightning rod of criticism and argumentation, as he has done so effectively in years past. Perhaps there will be some contention for the new lightning rod of criticism, but please, Professor Diermeier, if you would, tell Professor Bendor that he was certainly missed at today's panel. I'd like to make a few comments, allow time for the panelists to respond, and finally open the discussion to everyone for comments and questions.

First, this is an interesting panel to start with, because in a sense this set of scholars is not a traditional agent-based modeling group. They didn't show us any continuous run-time movies. In a sense, they haven't fully bought into the agent-based modeling framework, and that's part of what makes their work so interesting. We're seeing the beginning of the incorporation of some insights from agent-based modeling into ongoing fields of study. So throughout the conference, we should be asking ourselves, how can some of the partial insights that we gain from our models be incorporated or used in certain ongoing areas of theoretical investigation or empirical study?

Each of these authors' goals is to take something from agent-based modeling, or from related fields, and incorporate it into micro-economic theory, game theory, and political economics. The Diermeier paper looks at variations in the process of rationality — different ways of thinking about adaptation. The Hanaki and Peterhansl paper looks at evolving structures, and the Ahn paper looks at heterogeneity of types and builds in those ideas.

Looking at the Diermeier paper — and this is a meta-theoretical paper — I'd first like to ask a meta-theoretical question. Could you defend the way in which your paper approaches the question of empirical validity? As I understand it, you are seeking the kind of empirical validity that is a matching between the final result or the equilibrium result of the model and in a sense a final result or an equilibrium result in the empirical world. Is this necessarily a fruitful question to ask, given the difficulty that we have in doing this matching? Should we focus instead on whether there's empirical content or empirical validity to the kinds of social mechanisms that are at work or simply to the local interaction structures? It wouldn't necessarily have to just be a question of macro- or micro-level matching empirical validity, but also an issue of mezo-level empirical validity.

My second comment is that this paper takes the idea of assumptions very seriously, and there was one assumption in particular that I would be very interested in seeing you work with, and that is the assumption of social structure. Part of the methodology of this paper, and of other papers by some of these scholars, has been to ask what assumptions do we need in order to get a certain kind of result? With regard to the part of the paper that deals with social structure, in order to eliminate the possibility that, as you say, "anything can happen," my question would be, what kinds of social structure do you need? How much social structure do you need to get a certain kind of result?

For example, perhaps you don't need a completely connected social network. Perhaps all you need is the existence of certain opinion leaders in the network or the existence of certain brokers in the network. So you may have more social structure in your model than you already need in order to get the result. That kind of investigation would seem to be consistent with the spirit of some of the other aspects of the paper.

I have a few different comments, a couple of minor ones, for the Hanaki and Peterhansl paper. First of all, with regard to the partner-changing aspect to the paper, I think that you should make partner changing costly, and that will help to give you some interesting path dependencies in the model and also make the dynamics a bit more realistic. Second of all, your paper stimulated some interesting questions that I think you could investigate with just some slight variations in the model. For example, I'm interested in knowing how the model might deal with gossip; that is, third, not just individuals exchanging information within dyads, but also exchanging information about what may be going on in other dyads in the network.

Your paper also seems to stimulate the question of whether information can become a local public good. That is, if you start to see local regions within the network that are developing rich information — pools of information — and whether that begins to take on public-good characteristics and whether it leads to certain regions of the network to start performing in a superior fashion.

My major comment on this paper, though, is that I'd like to see you explore the notion of "over-embeddedness;" that is, the extent to which social structure can have these positive effects on cooperation and interaction, but also negative effects, which may lead to inferior results. Your paper begins to suggest that. I'm also interested seeing if you could take a little time to discuss the tension between Figure 3 and Figure 5 in your paper. Figure 3 shows a positive, monotonic effect of increasing clustering, of increasing social structure, but Figure 5 shows that there is some limit to the benefits of social structure, because clustering begins to go down over time. So could you think about, or could you incorporate within your model, not only the beneficial effects of social structure, but also the cost of social structure?

As to the Ahn paper, I would be interested in your thoughts on the value of the following distinction: is there a conceptual value in distinguishing between the types of actors who react differently under different contexts as you take various types of actors and you give them a certain context and see how they react differently? Is there a value in making the distinction between that and saying that actors can change their type in different contexts?

For example, is it that I am an egoist, and in contexts A, B, and C, I behave differently, or is it that I think about my world differently as it is framed to me in different ways? Do I think and act like an altruist in my family, but with regard to publication of my research, do I think and act like an egoist? Perhaps with respect to, say, participation in professional associations, I act like a reciprocator. Is it a matter of me changing my type in these different contexts under these different frames, or is it a matter of playing different strategies in different contexts? Is there a value in making a distinction between these two ideas?

Also, in your model, are there changes in the population of the agents? If I understand the model correctly, changes in the composition of the population occur only through death, and I wonder if it would be possible to build a model through which agents change their type within their own lifetimes. Also, what do you think the value or lack of value of that would be? It would be interesting to see you incorporate some second-order effects; that is, that agents start learning locally from what's going on among others in their local network.

If I could have just one effect on the research of the scholars in this panel, I'd like to see each project investigate social structure with a little more nuance. As I understand these models, the authors are conceptualizing social structure as being the kind of thing that is either more or less. So we have either a more structured network or a less structured network. However, there are different kinds of structures, and we might not be interested in more or less. Rather, we might be interested in certain characteristics of the structure and how those characteristics of the structure feed into the behavior of the actors.

For example, it might not just be a question of more or less connection within the network, but also more or less hierarchy, more or less cohesiveness, and also the existence of brokers and where those brokers are in the network. Investigating these types of questions would give the discussion of social structure more nuance, and it would avoid conclusions that are somewhat — or what I find to be somewhat — unhelpful, such as structure; well, this shows that structure matters. Personally, I believe that structure matters. And so how and under what conditions does structure matter? In order to begin to answer that question, we need to have more differentiated ideas about structure.

These papers seem to interact with one another and speak to one another in interesting ways. I'd like to propose that Diermeier and Hanaki and Peterhansl take the Ahn issue seriously and think about — or at least muse upon — how your models would be different if you incorporated Ahn's idea of multiple types of actors. Dr. Ahn, I propose that you take up the Diermeier idea of thinking about variations in adaptive rationality and how that might affect the results of your model. Finally, not for the discussion here, but perhaps for the drive or the flight home, it seems that each of these research programs could write at least one paper that takes these ideas and applies them within the context of prospect theory. How appropriate would that be, given the awarding of the Nobel Prize in economics this week? With that, I'll give each of the authors a chance to comment. Professor Diermeier.

Daniel Diermeier: [inaudible on tape]…of working in some very real social features into the particular games that have been studied very well, but it seemed like there was a lot of room for introducing embeddedness and a number of other features that we've tried to introduce in looking at these games and studying their outcomes. But who knows where we can take it from here? Maybe looking at empirical data eventually would be something very exciting to do, although, given the paper as it is now, it would be very difficult to make that link. I think you're correct.

David Sallach: David Sallach, University of Chicago. Professor Diermeier, regarding this tendency to try to begin to focus some of the models so that they move away from agents that are thoroughly abstract and manifest socially interesting phenomena, I suggest that, in addition to classical and mixed preferences, it would very interesting to see a similar kind of analysis of agents that have sociation effects, which are not only present in various physiological kinds of needs, but also in marginal utility.

Diermeier: Yes, absolutely. The way one could model that in this context would be, in a certain sense, to have balance on the aspiration levels — an upper bound, for example. From that point on, I'm satisfied; I'm not going to update anymore. Actually, it's fairly straightforward then to incorporate it into the theorems. Right now, for every possible payoff, we have an aspiration level, but you can restrict that. You would only need to reformulate the theorems and then basically … as well if the aspiration level is above or below in that wide range, and then you get the result again, or corresponding result, I should say.

Michael North: Michael North from Argonne National Laboratory. This comment is for Ahn's paper. It was very interesting that you said that you're obviously developing a simple agent-based model, but then you have another mathematician who is trying to find an analytic solution for the same problem. This comment may apply to the rest of the panel as well. I think it is important to do that type of docking, either between agent-based models or between, say, a computational technique and an analytical technique. In particular, perhaps each of you would comment on the rapprochement in the sense of how you would take what you have and try to compare it to some other type of technique, particularly if you're analytical or computational, if you take computational for analytical.

T.K. Ahn: If any computation model can, for example, be properly modified without sacrificing the substantive assets such that it can solve analytically, there's no reason why you shouldn't do that, and maybe for every agent-based model, especially in relation to an evolutionary one, it's better advice to check if that's addressed as it is or with proper modification analytically.

I've seen some papers or presentations, which, for me, it's definitely do-able analytically, and it can be solved. Probably, it's not just to show off math muscles, but you have a more general, complete understanding of the situation.

Alexander Peterhansl: I have a quick comment on this subject. Even though ultimately your ambition might be to model something analytically, one of the things that we've learned in putting our model together and writing our paper is that the computational method can serve as an incredibly powerful test bed to test out features, to play and get a feel for what's driving the model. In that way, it serves as an incredible filter for things that you might eventually want to put into a small analytical toy model that you can go around and present. I think ultimately, though, your lessons are learned on the computer, exploring parameter spaces, putting in new features, taking them out again, and so on. In terms of a formal coupling, though, I think it is an exciting area. I don't know how much has been done in this area, but I think it is very promising indeed.

Diermeir: You're running into open doors. I'm very much in favor of that. I think it's important in two respects. We've seen the general trade-off between analytical versus computational approaches. I think that it's pretty clear what that is, just as T.K. and then Alexander have pointed out.

There's something else, I think, which is unfortunately not done nearly enough, and people may not even be quite aware of it, but a couple of questions could be asked. What am I really doing when I'm doing a simulation or when I'm doing agent-based modeling? What properties am I implicitly assuming, or what type of solution concept am I really working with? That motivated our paper mainly because there's a large, very well respected literature in sociology that starts out, writes down the models, and then has 5 or 10 different starting values.

But what you want — and I think increasingly it will be important, particularly for this field — is to be taken seriously by people that work in economic theory or formal modeling and others, in a game theory. … Whenever somebody develops, say, a new solution concept, they show existence, and that's an important part of it. For example, what is the solution concept? Does it exist? What are its uniqueness properties; what are its robustness properties? Then they connect it to some mathematical theory that is appropriate in that case, whether it's a mark … theory or whether it's some kind of computer science approach that shows us that what's going on is not in any sense empty or too dependent on the details of the particular simulation. I think that is not done enough. And so, in a sense, that's what we're trying to do in our paper, at least for one class of models.