

## Comments on the paper "Can Artificial Economies Help us Understand Real Economies?" by A. Kirman

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Alan Kirman's paper is a remarkable piece, a must read for any scholar interested in understanding the reasons behind the success of ABM modelling. The paper has a *pars destruens*, in which the many limitations of standard representative agent analysis are discussed; and a *pars construens*, in which, starting from those limitations, he presents the features and shows the potential of the alternative modelling strategy, based on heterogeneous interacting agents with limited rationality. The question of the paper is therefore straightforward, and crucial: Can ABM provide insights both on micro and macro or emergent behaviour, that standard representative agent models do not provide?

The answer is of course yes, and it is hard to disagree.

I found the paper particularly convincing in its description of the serious limitations of standard representative agents theory. Homo Oeconomicus simply does not exist, and social systems exhibit emergent behaviour that no aggregation of representative agents can replicate and/or explain. I found interesting, in this discussion, the attempt to disconnect the notion of rationality, that is not the monopoly of mainstream theory, from a very particular incarnation of this rationality, rational expectations. Agents can be rational even (actually, especially) if they do not know the true model of the economy.

I also share the emphasis on the necessity of more meaningful analyses of economic dynamics than the simple comparative dynamics or saddle path adjustments that constitute the bulk of mainstream analysis.

Coming at the *pars construens*, Kirman puts at the centre of a new paradigm two elements: a) heterogeneous "boundedly rational" agents; b) interaction and the analysis of emergent properties of

economic systems. The paper gives a number of interesting examples, both macro and micro of such a strategy, arguing for their capacity to better replicate stylized facts than the mainstream models.

I will conclude this short discussion with just a few remarks:

First, the case for ABM as a superior *descriptive* tool is flagrant. Even simple and stylized AB models have fare greater performance than representative agent models in replicating stylized facts. What is less clear, or at least what the paper is less successful in doing, is to make the case for the superiority of AB models in what concerns generalization. Reading Kirman's paper and more generally ABM literature, one has the impression that the capacity to remarkably replicate reality, *ex post*, comes at the price of the specificity of models, and hence of the incapacity to apply them to similar but not equal situations. This tension between data fitting and generalization is far from being surprising. Scholars in the neural network field, for example have long dealt with the issue of overfitting. I need not to convince anyone of the importance of generalization (how could we otherwise be able to respond to Trichet's request?). I am therefore surprised at how little discussion about this one can find in the ABM community, and in Alan's paper.

The other issue that I have with ABMs (and in general computational) models in economics is that too often robustness and model consistency seem to be optional. Disciplines that made use of computational techniques from their early steps would never accept model results based on a single set of parameters, and would ask the author to assess the robustness of her results to parameter changes. Likewise, the emphasis on out-of-equilibrium dynamics should not exempt the author from showing the internal consistency of their models (I think for example, in macro, of the respect of the resources constraint). Too often the ABM community does not require from its members the same intellectual discipline that is standard in other subjects. I would have expected the issue of robustness (that of course in this setting is much harder to check than in mainstream analysis) to be discussed in a methodological paper like Alan Kirman's.

Finally, a (minor) remark on the paper itself. I would have liked to see maybe less examples, but with a more detailed explanation of both the modelling strategies and the results. This paper is meant to be pedagogical, but in the end ABM models remain mysterious object, and a more thorough analysis of a selected number of examples could probably have been more effective in avoiding this.

## Reply to Comments

**Alan Kirman**

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It is very hard to disagree with the comments on my paper and I would like to thank the author for them. I do think that it is, perhaps, worth taking up a couple of issues, robustness and internal consistency.

To take the last point first, rationality in standard models means consistency with a certain number of axioms. As soon as we abandon that criterion we are told that we are giving up “sound microfoundations”, yet in a genuinely dynamic model in which people follow simple rules it is unlikely that their choices would be consistent in this sense. The axioms used are derived from the introspection of economists not from careful examination of the actual behaviour of individuals. This is the very basis of behavioural economics and agent based models use behavioural rules which are often more in line with that field than the more traditional axiomatic approach. Of course, the choice of rules seems ad hoc, but in my view no more ad hoc than our axioms.

The other point, robustness, is a serious one and the criticism that one should not accept the results of simulations with one set of values for the parameters for the model is perfectly correct. Indeed, any serious agent based modeller inspects the parameter space to see how large is the set of values for which his results hold. Too often the results of one set of values are given, but this is by way of illustration, and the author should also include or make available the robustness tests. I am guilty of this in presenting some examples.

As a last observation, I would point out that in many standard economic papers authors happily assume a very specific functional form for utility or production functions and the question as to how dependent their results are on that choice is simply not raised. I suspect that this is simply a question of familiarity and that we are so familiar with homothetic functions with all their implicit assumptions that we are not troubled by them. Yet when faced with alternatives which are less familiar we are much more exacting.

