SUPPLEMENT

AGENT-BASED MODELS AND ECONOMIC POLICY

edited by

Jean-Luc Gaffard and Mauro Napoletano

Comments and replies

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COMMENTS AND REPLIES

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.

Comments on the paper
"Macroeconomics in a self-organizing economy" by Q. Ashraf, B. Gershman and P. Howitt

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This paper starts by putting its finger on one of the most important problems in economics that of how economic activities come to be coordinated given the limited and local knowledge of the participants. This is a theme which is recurrent in economics and was discussed by Jevons, Walras and many of their successors and underlay the debate over the relative merits of socialism and market economies in which Hayek played a prominent role. What this paper does is to propose an agent based version of the Clower-Howitt model which aims to show how the network of firms, banks and consumers self can self organise into a coordinated state.

Whilst one cannot disagree with the criticisms that the paper makes of standard macroeconomic models such as DSGE, one is left wondering whether the criticisms are fully answered by the model proposed.

In this short comment I shall first take a brief look at the nature of self organisation and its properties and then go on to look at the model that the paper proposes in the light of this. Early in the paper the authors make the appealing analogy between an economy and an anthill. As they say, appealing to the father of the idea, Adam Smith,

"It is capable of "spontaneous order," in the sense that a globally coherent pattern of transactions can result from purely local interactions, without the intervention of a central coordinator. Indeed, like an anthill, a free market economy can organize transactions into patterns that are beyond the comprehension of any of its individual participants. "

This reflects the views of entomologists such as Deborah Gordon who is worth quoting on the subject.

> "The basic mystery about ant colonies is that there is no management. A functioning organization with no one in charge is so

> unlike the way humans operate as to be virtually inconceivable. No insect issues commands to another or instructs it to do things in a certain way. No individual is aware what must be done to complete any colony task. Each ant scratches and prods its way through the tiny world of its immediate surroundings. Ants meet each other, separate, go about their business. Somehow these small events create a pattern that drives the coordinated behavior of colonies."
>
> Deborah Gordon Ants at Work

The analogy seems to be apposite but a little closer examination shows that this is less true than it might seem. What the paper argues is that self organization achieves in the economy is a "globally coherent" pattern. By this is meant the idea that individuals driven by their own self-interest, manage to achieve, in general something close to a "socially optimal" situation. Ants have no self-interest and although their activity is coordinated there is little to suggest that it is, in any sense, optimal. This is, of course, in contradiction with the usual simplistic evolutionary analogies, which are used by economists to suggest that whatever survives must be optimal in some sense.

The message that one might try to take from the paper is that the economy somehow self-organises into an efficient or optimal state. However, the authors are careful to avoid falling into this trap. As they say,

"At the heart of all our work is a parable concerning the spontaneous emergence of a more-or-less self-regulating network of markets operated by profit-seeking business firms ".

Why then do I have any quarrel with the model ? The only objection is that the firms are perhaps too " rational " and that their rationality is too homogeneous. Given the tools that the authors propose it would be possible to be more adventurous in their modelling of the behaviour of the agents and to make them less uniformly purposeful and more like ants. To see what I mean it is worth taking a look at the basic model.

A model must necessarily simplify as John Kay (2012) observes in his paper "The Map is not the Territory" one should therefore see to what extent the model captures the essence of the phenomenon it is treating. Now, one can only wholeheartedly endorse the idea that how trading self organizes and its impact on allocations is an essential feature of economic life. In the light of this how does the model presented in the paper stand up? In the model shops trade the endowment good and consumption good of the owning household and open when there is a random opportunity. Is this a good simplification of the way in which trade occurs? Do trade networks and in particular retail shops develop in this way? There are few shops that trade goods which they hold or which alternatively they produce. We examined the kiosks which sprung up in Moscow at the time of the collapse of the Soviet Union and found that they sold widely demanded consumption goods such as cigarettes and coca-cola and that these were sold at prices which were set according to different rules by different kiosk holders. The holders, as in the Clower Howitt model had no experience of shop owning previously. But the important difference was the lack of specialization. The question that arises is how important is the association of owners and households to specific goods in the model?

Although the Clower Howitt model had the great merit of being a pioneer in explaining the organization of trade perhaps it would be worth considering a model with more heterogeneous rules for the agents. For example, in the Moscow case, kiosk holders told us that they used rules such as a simple mark-up over cost, or they tried to match the average, (or lowest in some cases) of the group of kiosks around them. One could then observe to what extent a common pricing rule evolved whereas in the model presented here the rule used is uniform and one might wonder why shops should wind up with break-even prices.

Again the authors rightly insist on the self organizing nature of the economy and use their ACE model to capture this. This means moving away from the standard assumption of equilibrium at each point in time. The usual way to achieve this, in standard models, is to introduce some sort of friction, but the sort of evolution described in the model seems more convincing than some arbitrary stickiness of prices and arises out of endogenous self organisation. But to come back to the origin of shops, Guriev et al. (1996) in an early paper pointed out that as soon as the infrastructure necessary to get goods from suppliers to consumers was inadequate many individuals would become intermediaries (or shops in the terminology of the paper) with consequent costs for the economy, since these individuals were no longer directly productive. In his model a small change in the cost of transporting goods drastically diminished the number of intermediaries and significantly increased production. Such an aspect is absent in the model described here.

The model presented incorporates an analysis of the role of inflation and of the role of banks both aspects which are lacking in more standard macroeconomic models and this is a very positive feature of the paper. One might however, quibble with the argument in favour of less regulation since the banks are, by assumption, respecting the most severe form of regulation, they are serving their basic function of reallocating the capital of others and not indulging in proprietary trading. Were they to be allowed to do so they might have a less laudable impact on the economy.

Thus the model presented is, of necessity, simplistic but there is nothing intrinsic in its construction and the tools used that would prevent its being used to investigate more realistic situations and this is the great benefit of the approach taken. ACE models move into territory which is unexplored because of the lack of analytical tractability but by so doing they allow economists to explore as Peter Howitt, in particular, has shown in a number of previous papers, the self organizing properties of economic systems which are surely more important than the sterile equilibrium assumptions usually adopted in standard economic models.

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Reply to Comments

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We are grateful to Alan Kirman for his comments, which suggest a number of ways forward as we continue to explore the issue of self-organization from a macroeconomic point of view. It is certainly true that we have picked a very particular and stylistic representation of the way trading networks form. Our setup is intended to embody in a straightforward way some of the basic features of actual economies that we find particularly salient for the issue of self-organization, especially the fact that exchange intermediaries tend to arise when there are unexploited gains from trade, that their operations use up a large fraction of any economy's resources, and that the process of establishing oneself in business is a hazardous one. The tight connection we assumed between the goods traded in a shop and the tastes and endowments of the shop's owner is, of course, not empirically plausible. However, it is not clear to us why our results should be particularly sensitive to the details of this connection; this is a question that certainly needs to be investigated further and one that we intend to explore in future research.

The fact that our shops are highly specialized captures another aspect of reality that we think is quite salient, namely that almost all trading facilities in a modern economy deal in a sparse subset of all traded objects in the broader economy. Even Wal-Mart does not sell industrial machine parts, legal services, funerals, golf course architecture, and a myriad of other items. But clearly our model of extreme specialization is a long way from what one sees in most real trading facilities, and it should not be hard for us to allow for a broader variety in the extent of specialization across shops, and perhaps also to recognize the multiple layers of middlemen that deal in increasingly broad categories of goods as we move up the chain from producers through wholesalers, brokers, distributors, and ultimately retailers.

There is no doubt that such details are of first-order importance when exploring issues in a microeconomic context. Our explanation for ignoring them in our work thus far is that we see our work as contributing to a discipline (macroeconomic theory) in which there has been almost no representation whatsoever of the formation of trading networks until now. Having come across one representation that seems capable, at least under some ideal circumstances, of producing an orderly pattern of transactions, we have been keen to put that representation to work in addressing some of the questions that have proven particularly intractable in more conventional equilibrium approaches. Perhaps it is now time to explore the extent to which our results are sensitive to allowing for the kind of heterogeneity that Alan Kirman and others have discovered empirically in the formation of actual trading networks.

Finally, we agree completely that our model of financial regulation should not be taken seriously as making a broad case for less financial regulation, especially since we have assumed that banks already obey a "Volcker rule" - that is, they make commercial loans but do not engage in proprietary trading. Not only does this assumption limit the scope for moral hazard, it also limits the extent to which fire sales can destabilize the process of deleveraging by causing a downward spiral in asset prices, because the assets unloaded by these banks are durable commercial goods with stable market prices rather than financial assets with highly flexible prices. Nevertheless, we find it interesting that this imposing this particular regulation seems enough to make other dimensions of prudential regulation (that is, limits on loan-to-value and capital-adequacy ratios) redundant or even destabilizing (as it does in "bad times"). The result underlines a point that is easy to forget in the aftermath of a disaster created by a poorly regulated financial system, which is that what we need in order to get the most out of our financial system is not tighter regulation in general but rather more intelligent regulation - regulation that limits the behaviors of financial institutions that tend to destabilize the real economy while loosening constraints on their stabilizing behaviors. There is so much more to do.

Comments on the paper

"Macroeconomic Policy in DSGE and Agent-Based Models" by G. Fagiolo and A. Roventini

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From the very beginning (from the title, I should say) the authors (F&R hereafter) pursue the goal of comparing and contrasting the relative merits of the DSGE and ABM approaches, with reference, in particular, to policy implications. The comparison between DSGE and ABM is carried out almost everywhere in the text—sometimes ...between lines—and is made explicit especially in the introduction and in the concluding remarks. I think that F&R have brilliantly exposed the weaknesses of DSGE models (sections 2 and 3), have been successful in providing an overview of ABM (interpreted as a way out of the strictures of the DSGE approach, see section 4) but their comparative assessment of DSGE and AB models is not convincing. T*he authors' evaluation is that ABM beats DSGE hands down* but this assessment is clearly unbalanced. Let me reveal my priors before proceeding: I am aware of the limitations of DSGE models—which have been spelled out by many authors, especially since the onset of the Global Financial Crisis (GFC), and are thoroughly surveyed in the paper—and I am very much in favor (to say the least!) of ABM but I think that such an unbalanced assessment of the two streams of literature is not only unrealistic but also not useful, especially in terms of future developments of the AB literature. It may be overly optimistic and slow down the pace of development and refinement of ABM.

New Keynesian DSGE modeling has a honored and by now quite long history. This body of literature has grown over a span of more than two decades in the usual manner, *i.e.* by addition of missing elements (with respect to the three-equations model sketched in section 2 by F&R) and by twists and turns dictated by new macroeconomic evidence. For instance financial factors have been introduced in this literature since the end of the '90s (even if they have gained center stage only after the onset of the GFC)[1]. It is true, as stated over and again by F&R, that these models cannot capture, almost by construction, some of the basic features of the GFC and therefore cannot be used to forecast the advent of a financial crisis. In many instances, well known proponents of this approach have recognized this limitation: DSGE models are useful in macro- economic forecasting "in normal times" but almost useless in the proximity (or during) a financial crisis and the ensuing recession. Therefore, if we want to capture at least some of the features of the GFC we have to go beyond DSGE macro models.

Are ABMs an alternative? F&R's answer to this question is a resounding yes! Mine is a more cautious: not yet.

Contrary to the DSGE literature, AB macroeconomics is still in its infancy.[2] It is true that, by construction, form a specific point of view ABM are better than DSGE models: There are research tasks, in fact, that can easily be carried out in ABM and are by construction out of the reach of DSGE models. In particular, one can generate artificial cross sectional evidence (through simulations) and compare the simulated evidence with the empirical one. For instance most of the ABMs mentioned in the references generate a power law distribution of the firms' size. This unique capability, however, is of limited use in assessing the emergence of a financial crisis.

As to the aggregate evidence, it is indeed true that all the ingredients which you may dream of to capture stylized facts of the crisis are already part and parcel of AB models (bounded rationality, nonlinearities, bankruptcies and so on) as F&R correctly point out. But these models have been so far able to re- produce these stylized facts *only qualitatively*: instead of the "well behaved"—but terribly unrealistic—impulse-response plots of the DSGE approach, ABMs can reproduce the irregularly oscillating time series of GDP, generated from the bottom up, with ample room for booms and sudden busts of economic activity. This is all fine but there is a long way to go before implementing empirically these models for forecasting purposes: ABM can reproduce the "stylized facts" both at the cross sectional and at the

1. Bernanke, B., Gertler, M. and S. Gilchrist (1999), "The Financial Accelerator in a Quantitative Business Cycle Framework", in J.B. Taylor and M. Woodford (eds.), *Handbook of Macroeconomics,* vol. 1, Elsevier, Amsterdam. Curiously, this seminal paper does not appear in the references of F&R.

2. AB models have been applied in a number of fields and have been around for decades now but applications to macroeconomics are only few and most recent, as one can infer from the list of references in F&R.

aggregate level but at the present stage of development they are not implementable for forecasting purposes; on the other hand empirically implemented NK-DSGE models are indeed used for macroeconomic forecasting (but they are reliable only in "normal times"). My impression is that so far ABMs and NK-DSGE models have been built and analyzed for different purposes, as answers to different research questions. Therefore they are not really comparable (and this is indeed the impression that one gets from the paper).

Potentially, once empirically implemented with the specific needs of macroeconomic forecasting in mind, ABM will, in my view, be used to generate macroeconomic forecasts (and therefore they will be truly comparable with NK-DSGE models). Moreover, *potentially*, ABMs can do much more than NK-DSGE, *i.e.* they can be used to generate early warning signals of an incoming crisis (because ABMs can "accommodate" domino effects and therefore systemic risk, issues that cannot be dealt with in standard NK-DSGE models). I'll make a bet: it will take years, not decades. But this is only an educated guess, it is not reality yet. We have to wait (and work) before verifying the guess.

Reply to Comments

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We thank the discussant for the very insightful and stimulating comments to our paper. About the current state of agent-based models (ABMs) vis-à-vis DSGE ones, we are a little bit more optimistic than him, because of three related reasons. *First,* we believe that macro ABMs such as the K+S (Dosi e*t al.*, 2010, 2012) and CATS (Delli Gatti *et al.*, 2011) models largely beat DSGE ones on the empirical validation side. *Second,* as empirical validation is a necessary condition to perform policy analysis and we have shown in the paper that DSGE models do not meet this criterion, we believe that policy implications drawn from DSGE models are logically inconsistent and should not be used by practitioners and policy makers. *Third,* ABMs allow for much more flexibility in the design of policy experiments than DSGE models, which are typically developed by patching them with ad-hoc fixes every time they receive incoherent feedbacks from empirical results. Having said that, we think that the discussant is right about the fact that ABMs especially in macro still miss some important features before being able to replace DSGE as "the" tool for economic policy. In particular, in addition to those described in the concluding section of the paper, we single out *five* of them here.

1. Expectation formation. ABMs in macroeconomics should pay more attention to the way agents form their expectations. More specifically, a lot of work is needed to endow agents with more sophisticated expectation formation procedures which allow them to learn from their past mistakes.

2. Prediction. As the discussant correctly notices, ABMs are mostly employed from positive and normative perspectives (*i.e.* to explain or reproduce, and to under stand what kind of policy measures could lead to certain desired outcomes). What is still missing is prediction. However, prediction requires to take seriously the issue of calibration,

which is again an issue that in the ABM literature requires more discussion.

3. Estimation. In principle, ABM models parameter can be estimated with the data, possibly with Bayesian techniques, thus leading to fully calibrated models that can challenge the predictive capability of DSGE ones. Again, a lot of work is required to fill this gap.

4. Welfare. More attention must be put in designing ABMs where one can easily evaluate the outcome of any policy measure in terms of social welfare. So far, in absence of a well developed theory of consumer choices, the outcome of policies is only evaluated through aggregate measures like output growth or volatility.

5. Comparability. Different DSGE models can be easily compared in their structure and in the results they produce because they are built following standard proce- dures. On the contrary, the extreme freedom one faces in developing an ABM from the bottom-up reduces the comparability among different ABMs. The ABM community should make additional efforts to develop some standard procedures which could allow different ABMs to "speak" to each other. An interesting effort in developing a common documentation guidelines is Wolf *et al.* (2011).

Despite all this room for future works, we still believe that ABMs are already a very good alternative to standard DGSE models. They are based, instead of DSGE, on relatively more realistic assumptions, whereas DSGE are built upon building blocks that are rejected by both experimental and empirical evidence. No one believes anymore in Friedman's instrumentalist tenets: if one wants to build models that explain reality, it is imperative to start by models that use approximations to reality as their assumptions, not false ones. In our view, this suffices to decree the winner of the contest: agent-based models.

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Comments on the paper

"Reconstructing Aggregate Dynamics in Heterogeneous Agents Models" by D. Delli Gatti *et al.*

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The main aim of the paper is to apply the image processing interpretation of the Maximum Entropy (MaxEnt) method to the Kirman (1993) model and the Abrams and Strogatz (20003) voter model as implemented by Stauffer *et al*. (2007). This follows the initial work in Barde 2012 which showed that the Schelling (1969) model of segregation can be predicted with the methodology. The discussant does point out some of the major issues that are associated with the methodology, many of which I agree with. The most important comment is probably the fact that more exploratory work is needed to establish a taxonomy of valid assumptions for corresponding statistical properties. Having said this, I feel that two important clarifications are needed.

My first comment relates to the claim that the assumptions or simplifications required to obtain the MaxEnt solution are arbitrary. Given some data d (the initial condition in agent-based models), the basic formulation for obtaining the prediction μ the maximum entropy problem is given by:

$$\max_{\mu}\left[\alpha S\left(\mu \mid m\right)+\ell\left(d \mid \mu\right)\right]$$

The first part of the expression, $S(\mu \mid m)$ is the relative entropy of with respect to a model m and $\ell(d \mid \mu)$ is the likelihood that the initial condition d is a noisy version of the prediction μ. For any given problem, two terms need to be specified: the model term m and log likelihood $\ell(d \mid \mu)$. While there is an element of 'educated guessing' in specifying these terms, this is not as arbitrary as the discussant claims.

— The model term m is a diffusion term which specifies how far the prediction can stray from initial condition, and this is the term that controls for time in the system. Intuitively, if very little time has elapsed, one should used a very peaked m, as μ will be very close to d. Conversely, long time horizons are represented with a flatter m. It is also important to note that m can have several dimensions, depending on the nature of the problem: one dimensional for the ants model, two dimensions for the Schelling and voter models.

— The likelihood term ℓ depends on the nature of the path linking the initial condition to the predicted state of the system. The image-reconstruction algorithm treats μ as the true image to be discovered and d as a noisy version of μ. This time-reversed path is conditioned on the fact that if the sequence of actions taking the system from its initial condition to its equilibrium distribution is best-response (a common assumption in economics), then the reverse path is effectively a noise process. The likelihood term is therefore determined by knowledge of the updating process, which determines the implicit noise process in the reversed path.

Both these terms are determined from the updating rules of the system, and are therefore not as arbitrary as it may seem. It is true that if little information is available (for instance if the exact transition probabilities are unknown), they must be approximated. For instance, in the generic version used for the voter model, both a gaussian likelihood $\ell(d \mid \mu)$, *i.e.* a gaussian noise process, and gaussian correlations over two-dimensional space for the model term m are assumed as an approximation. However this can be refined if more information is available from the updating process. This is the case in the ants model, where the transition probabilities are well known. In this case the model term is the diffusion of a stopped random walk rather than a gaussian diffusion and the likelihood is designed directly from a path integral of the transition probabilities.

Clearly, MaxEnt is no miracle solution: if the researcher has no information about the dynamic updating process of a system, then there is no way that knowledge of the initial condition alone can lead to a decent prediction of future states. In the Kirman ant model, for instance, the initial condition at $t = 0$ is simply a value $x \in [0,1]$ representing the share of ants of a certain colour. If the researcher is ignorant of the recruitment mechanisms, then x alone does not provide much information on the stable distribution of the system at a later time $t = n$. The central argument for using MaxEnt in the context of agent-based models is precisely that the updating rules of the system are known *ex ante*, as they are provided by the researcher.

My second comment is would be that the aim of the methodology is not to replace the traditional Monte-Carlo methods used in agent-based models but instead to provide a complement. The methodology is analytical in so far as the derivation of the maximum entropy problem is obtained from a rigourous Bayesian approach however, as mentioned by the discussant, in most cases a numerical methodology is required to solve for the solution of the problem. Furthermore, as pointed out by the discussant, the three simple models analysed so far with MaxEnt are a far cry from the complex systems routinely used in the agent-based literature. So given this, what is the usefulness or purpose of the proposed methodology?

An important application in my opinion is to provide a tool for categorising types of agent-based models according to the strength of their convergence to a stable distribution. A key finding of the paper, as well as the companion work on the Schelling model is that while the three models are clearly stochastic, the fact that they are amenable to MaxEnt prediction reveals that they are much more predictable that one might think. In technical terms, this is related to the fact that the image reconstruction MaxEnt algorithm works only if one is able to treat the reversed time-evolution of the system as a noise process, indicating that the time-evolution is in fact a finite improvement path. I agree with the discussant that more work is needed

In the future, rather than providing a direct solution tool for large agent-based model, a potentially important application for MaxEnt is the prediction of those component modules of the larger model that are amenable to MaxEnt. In interesting possibility in this regard is to take advantage of the faster execution speed of the methodology compared to Monte-Carlo to directly provide agents in the model with expectations, by using MaxEnt on the current state to obtain predicted future values for key state variables. Similarly, it could be used to speed-up large agent-based models by using the faster MaxEnt method on those components that are known to be amenable to the methodology.

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Reply to Comments

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In our view, the aggregation problem does not boil down to simple "averaging" in such a way as to resurrect the Representative Agent. [1] In order to elaborate on this, we should start from the following notion: In a (macro) system there can be elementary (micro) and composite (meso) constituents. Micro constituents are units which agglomerate into within-homogeneous but between-heterogeneous sub-systems (meso constituents). Accordingly, a (macro) system can be seen as made of (meso) sub-systems composed by (micro) elementary units, that is a statistical ensemble which represents all the significant configurations the system can assume.

At any level of observation, a quantity is a functional, whose realised values are measurement outcomes. For instance, micro-functions implemented in an ABM are micro-stochastic processes constituting a statistical ensemble. In a single run of simulations, an ABM will generate a sample of numbers which is the realisation of the collection of their outcomes at each point in time: in a sense, an ABM is a space-time random field.

The numeric outcome of each micro-functional can be thought of as the outcome of an experiment, hence it is the measurement of a certain quantity on an observation unit.

A transferable quantity is a variable whose aggregate value is given by the summation of the constituents' values. Only transferable quantities admit an exact/algebraic aggregation. Non transferable quantities are system specific: being realised by the superimposition of

1. The RA is not the average agent, technically it is more properly an estimator for the system as a collective body characterised either by transferable and not transferable quantities.

underlying micro-level quantities they are emergent information. For instance it is not possible to algebraically aggregate individual prices (they are non transferable quantities): their mean is not the market price but the average price in the market. The market price pertains to the market as a collective entity, a system by itself. The inflation rate does not make sense at the individual level but it depends in some way on individual behaviours.

It is possible to associate a stochastic process to each kind of quantity in order to have aggregation in terms of expected values. The expected value of a given observable variable is a functional and does not coincide with the average. The expected value is the estimator of the first moment of a stochastic process, the average is a particular realization of that estimator given a set of experimental outcomes.

From the algebraic point of view aggregation is not a problem if quantities are transferable. A collection of numbers characterising the same property of the system's constituents can always be added up to generate the aggregate value. This makes the aggregation problem somehow misleading. Indeed, if a collection of realised numbers $\{y_{i,t}\}$ from a transferable quantity is available, then $Y_t = \Sigma_i y_{i,t}$ solves the problem. But what if $y_{i,t} = f(x_{i,t})$? Is it still true that $Y_t = f(X_t)$? Moreover, what if we know Y_t and X_t but cannot observe the micro-data? Given a set of micro-data from a transferable quantity it will always be possible to determine an exact system level number by means of algebraic aggregation. This is not possible in the other two cases. Therefore, the problem is inferential as concerning the macro-functional. In terms of micro-foundation things are even more complicated. The correct question in this case is: given the macro (X_t, Y_t) what is the micro $(\{y_{i,t}\},\{x_{i,t}\})$ which is consistent with it? The answer is: the most probable one. Therefore, the expected value functional is needed before an average estimate.

As stated above, the system's constituents can be thought to agglomerate into sub-systems which are within-homogeneous and between-heterogeneous with respect to some criterion. This aspect leads to a mean-field approach to aggregation.

Mean-field can be seen as a method to determine aggregate functionals at sub-system level taking into account the phenomenology of micro-functionals and of their realisations. If mean-field were made explicit in terms of expected values, averages would of course be given at sub-system level, but in no way this implies that these values are representative of a collective agent in the same way as the representative agent does. The representative agent is a simplifying assumption which allows to manage heterogeneity and interaction for practical purposes when dealing with problems of aggregation in a micro-founded context. It is (or behaves) as a collective body on a smaller scale: in its extreme version, the representative agent is associated with the system as a whole, annihilating every kind of heterogeneity for system's constituents Hartley (1997). The representative agent can be thought of as the estimator of a (sub-)system, but it still remains a description of a collective body on a reduced scale level: a per-capita value is not a property of the individual, it is still a system property. With per-capita values we are used to compare (sub-)systems not individuals. The fact that one can think of the average as a numerical *aggregator* of micro values can therefore be misleading because it might be thought that a mean-field approximation of the system is equivalent to the representative agent.

These statements can be made specific by considering the mean-field approach in the master equation framework for the dynamics of a probability density for a given observable on a certain state space. The density $P(N_j(t),t)$ is the probability distribution of micro-constituents over a state space of sub-systems, which are shaping the configuration of the macro system. Therefore, in the master equation framework, the density $P(.)$ is to be conceived as a control-functional. On the other hand, the mean-field observable $N_j(t)$ plays the role of a state-functional.

In mean-field terms, one can specify a model for $N_j(t)$ which links its realisations to some other quantities at macro level to take care of the environment feedbacks, as if they were some force-fields acting on system constituents and inducing their agglomeration into sub-system as an externality effect. It is also possible to specify these effects in terms of *effective interactions* among sub-systems, which is what Aoki (Aoki, 1996; Aoki, 2002; Aoki and Yoshikawa, 2002) calls mean-field interaction by means of transition rates. Moreover, there can be also some emerging characteristic which drives the most probable path trajectory of the state-functional, which is what Aoki calls the macroeconomic equation, best know as macroscopic equation and which can be associated to the notion of pilot-quantity, at least according to the pilot-wave theory in the Bohminan interpretation of quantum mechanics (see Bohm, 1952a; Bohm, 1952b).

Among the methodologies to solve master equations (see Kubo *et al.*, 1973; Gardiner, 1985; Risken, 1989; van Kampen, 1992; Aoki, 1996; Aoki, 2002; Aoki and Yoshikawa, 2002) when the state-functional is known to be distributed as unimodal and peaked about its expected value, $N_i(t)$ can be expressed by means of the van Kampen ansatz: $N_j(t) = N\phi(t) + \sqrt{N}\varepsilon(t)$. In this representation, the macroscopic equation drives the expected value of the share of agents occupying the *j*-th state and is itself a function of transition rates, $\dot{\phi}(t) = \phi(\beta(t), \delta(t))$, each of which—in Aoki's interpretation—includes the effect of the environment on $N_j(t)$ by means of the so called externality functions $\psi_j(t)$ depending on system quantities. Therefore, being a fully functional development of the system, and allowing for heterogeneity and interaction, the mean-field/master equation approach cannot be confused with a representative agent, unless the representative agent were specified as an estimator for the system as a collection of collective bodies (sub-systems) each of which takes a place on the state space and obeying an exclusion-like principle, which is not a very reliable assumption. Differently said, two subsystems in the same state are almost the same sub-system and they can be lumped into a larger body because their elementary constituents belong to the same micro-state.

Finally there is one more technical aspect which needs to be dealt with: a master equation, in general, does not admit a closed form solution but requires an approximation method to be solved. Basically there are three methods of approximation, each of which has been described by Aoki (Aoki, 1996): Kubo method (Kubo *et al.*, 1973), Kramers-Moyal expansion (see Gardiner, 1985; Risken, 1989) and van Kampen system size expansion (see van Kampen, 1992). A fourth method is also available, it is the one developed in our paper and it can be called Aoki method: in essence it is a variant of van Kampen's, even though more natural and easy to deal with. All these methods share a common feature: they are grounded on approximation techniques. In the van Kampen/Aoki perspective, by using the ansatz $N_j(t) = N\phi(t) + \sqrt{N}\varepsilon(t)$ into an explicit definition of transition rates, the master equation for $P(N_j(t), t)$ is transformed into a master equation with respect to the spreading fluctuations term $\varepsilon(t)$, that is concerning the density $Q(\varepsilon(t),t)$. This new master equation is perfectly equivalent to the original one and its approximation is as follows: transition rates are Taylor approximated about the drift $\phi(t)$, the density $Q(\varepsilon(t),t)$ is Taylor approximated about the spread $\varepsilon(t)$. Due to the transformation $P(N_j(t),t) \equiv Q(\varepsilon(t),t)$ and a time rescaling, a system size parameter N enters the new master equation and its approximation. Hence, by applying the polynomial identity principle, two differential equations can be asymptotically isolated: the first one for the dynamics of the most probable drifting path trajectory, $\phi(t)$, and the second one for the dynamics of the probability density of spreading fluctuations $\partial_t Q(\varepsilon(t),t)$. The first one is the macroscopic equation, and it depends on transition rates, even though it reads as an ordinary differential equation. The second one asymptotically converges to a Fokker-Planck equation as the system size increases. The macroscopic equation can be solved separately from the Fokker-Planck, its solution can therefore be used to solve the latter. Very often, the Fokker-Planck can be analytically solved with standard methods but, if the transition rates are too complicated, the solution can also be found *systematically*, in van Kampen's terminology. Indeed, by setting the stationarity condition $\partial_t Q(\varepsilon(t),t)=0$, the Fokker-Planck equation boils down to a continuity equation obeying Liouville theorem, and it reads as an Hamilton-Jacobi equation. Since it asymptotically concerns a second order approximation, the stationary distribution is found to belong to the family of exponential distributions of Gaussian type. Therefore, what one really needs is a set of coupled equations (called the mean-field system) for the first and the second moments to get the dynamic functionals for the expected value and the variance driving the density $Q(\varepsilon(t),t)$ through time.

Two remarks are in order at this point. First, the differential equations for the expected value and variance functionals of the spreading fluctuations distribution about the drift depend on the transition rates and the macroscopic equation (here it comes its pilot role), and this shows that fluctuations about the drifting path trajectory have an endogenous specification in terms of mean-field or *effective interaction*. Secondly, what has been found to be Gaussian is not the solution of the master equation itself, but the distribution of fluctuations: van Kampen/Aoki methods do not provide a properly said Gaussian approximation to the model.

This method is not less valid than the Kramers-Moyal or Kubo methods just because of approximation. Indeed it does not properly allow for Gaussian approximation of the master equation, while Kubo method guesses a-priori an exponential probability kernel of Gaussian type and, if the Pawula's theorem (see Risken, 1989) conditions for the second order approximation are not fulfilled, Kramers-Moyal method is by definition approximate without any asymptotic behaviour. Moreover, as it can be done either with Kubo and Kramers-Moyal methods, the van Kampen/Aoki method can deal with higher order moments, which usually characterise asymmetric distributions. The weakness of van Kampen/Aoki methods is that they provide a local approximation about the drift for the state-functional $N_j(t)$, while

Kramers-Moyal and Kubo methods provide a global approximation for the probability density $P(N_j(t), t)$ control-functional. In our model this is almost irrelevant because the markovian nature of the model allows quite naturally for a second order approximation, and because the state space is trivial being made of two states only. Therefore, the state functional $N_1(t)$ is necessarily unimodal, and this allows for the shown ansatz. In general, with more complex state spaces, non-unimodal distributions and asymmetries, Kramers-Moyal method gives better results provided some reasonable order of approximation.

In our opinion, Aoki's interpretation of the Master Equation Approach (MEA) combined with Mean-Field Approximation (MFA) leads to three main theoretical results with promising applications to socioeconomic disciplines, mainly developed in macroeconomics:

1. *stochastic aggregation* of complex systems made of interacting and heterogeneous constituents;
2. *inferential identification* of drift and spread stochastic functionals as dynamic components of time series at the system's level;
3. *endogenous modelling* of interaction and spreading fluctuations about cyclical drift as the macroscopic emergent phenomenon due to the superimposition of microscopic behaviours.

Of course the MEA-MFA does not solve all the methodological and technical problems of macroeconomic modelling but it makes some steps beyond theoretical and practical problems the standard model is facing in micro-foundation of macro-models and aggregation of micro-behaviours. One of the most intriguing suggestions that have emerged from the issues dealt with in the paper and its discussion is to take in account local and global interaction by means of a nested structure consisting of groups made of sub-groups which can be partitioned into even smaller agglomerations over a finite *hierarchical structure of concentric levels*. In principle it might be possible to specify several master equations one nested into the others from the higher to the lower level of description. Each equation should be used to distinguish different interactive environments, from the very global to the most local one. The deeper one goes through this structure the more the interactions become less global, or more local, and the nested combination should take care of field-effects exerted by the level above or outside on the level below or inside. It is our opinion that this structure could be promising for two related purposes: it can describe transitions between different areas of a state space by considering dynamic transitions though partitions within each areas, and it can be a starting point to develop a phase-transition and self-organised-criticality analysis for complex socioeconomic systems.

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Comments on the paper

"Of Ants and Voters: Maximum entropy prediction of agent-based models with recruitment" by S. Barde

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The paper by Sylvain Barde presents the explorations into the powers of a novel technique (to economics) called Maximum Entropy (MaxEnt hereafter). The methodology was introduced to economics by Foley (1994), but to the present day its potential is largely untapped. MaxEnt allows predicting solutions to agent-based models analytically. The previous use of methodology has been in image reconstruction, where predictions are made about the original image based on the noisy signal at hand. The approach has a great potential on reducing computational time required to run full-fledged agent-based models that are very often NP-difficult.

A particularly intriguing feature of the methodology is that time is implicitly embedded in it. This might not be important in image reconstruction but it is very important in economics as it allows to predict not only the time invariant/equilibrium solution to the model but also to describe the transitional path to it.

In previous paper (Barde 2012) the sufficient conditions for the applicability of the methodology have been derived. The same paper has applied the MaxEnt methodology to Schelling's (1969, 1971) model of segregation. It has been demonstrated that MaxEnt is powerful with respect to the models with fixed proportion of distinct populations.

In current paper the methodology is applied to two models with recruitment. These are the models of ant behavior by Kirman (1993) and that of language competition by Abrams and Strogatz (2003). The distinction with respect to the previous application is that recruitment allows the proportion between the (competing) populations to vary. The properties of the two models discussed are well known. In light of this, the performance of the methodology is tested on different time horizons. Using rigorous computational approach it is demonstrated that, similar to the previous application to Schelling's model of segregation in Barde (2012), MaxEnt performs very well in case of present two models with recruitment. This is true especially for the short-term predictions where initial conditions influence the outcome greatly (which is equivalent to noisy signal containing large chunk of undistorted information).

Let me outline a methodology to assess the powers of MaxEnt that the author follows closely with one exception on which I will concentrate below.

A researcher starts from the theoretical model which we can solve numerically using ABM. She uses general Monte-Carlo approach to generate the development paths implied by the theoretical model from numerous random initial conditions. These development paths are traced all the way to the relevant time-invariant/equilibrium distribution. This is the problem that is computationally expensive for virtually every relevant economic or social model.

In parallel to this, a researcher writes down the statistical model that is based on underlying theoretical model. Further, this statistical model is solved for the transitional path and equilibrium distribution. The solution can be analytic, however this is usually not feasible. Therefore, numerical methods are involved in solution. The distinction from the ABM approach, however, is that this does not require Monte-Carlo simulations over large set of initial conditions (that is already taken care of by the statistical model). Hence it substantially cuts down the computational time.

Further, the two equilibrium paths and resulting equilibria can be compared in order to judge upon the accuracy of MaxEnt predictions.

As mentioned earlier the author in current paper follows the methodology closely. The transitional dynamics and equilibria are derived properly though ABM. He also succeeds writing down the corresponding statistical models in case of both models. However, for solving statistical models arbitrary simplifications are made. In articular, in Kirman's (1993) model the author uses the limit density derived by Alfarano and Milakovic (2009). But, in process of solution he replaces the diffusion term in the statistical model by simple random walk. In Abrams and Strogatz's (2003) model he assumes that the probability of two agents speaking the same language is normally distributed over the distance between the agents in order to model special correlations statistically.

Both of these simplifications are necessary for numerical tractability of statistical model. However, none of them stem from respective theoretical models and, therefore, are arbitrary. In both cases the author shows that despite these simplifications the predictions derived from MaxEnt methodology are accurate. But, arbitrariness of these simplifications casts doubt on the applicability of the methodology on larger scale.

The merit of the methodology is that it allows a researcher to derive the approximation of the solution in considerable shorter time. This is only useful in cases where ABM formulation of the problem is NP-difficult and solving it in real time is not feasible. In contrast to the evaluation exercises that the author has performed in present paper, when a researcher really needs to use MaxEnt she will not have the actual ABM solution to check the accuracy of MaxEnt.[1] Then if she would have to make arbitrary simplifications in the statistical model in order to derive MaxEnt predictions she will have absolutely no guarantee that the prediction at hand has theoretical validity.[2]

In light of this shortcoming it would be very useful if we would have some kind of taxonomy that would match each class of models with types of simplifications that a researcher can make in the process of solving a statistical model without undermining the validity of the MaxEnt predictions. This clearly involves immense amount of work and the methodology of creating such taxonomy is not clear for me at the present moment. However, I am afraid, without such a reference the applicability of the MaxEnt methodology is restricted to the class of models for whom the statistical models can be solved at least numerically *without* simplifications. And, again, based on the simplicity of the two models that we have seen MaxEnt applied to in current paper, I believe this class does not include all that many models.

1. If she did, she would have no need to run MaxEnt in first place.
2. On the other hand, if the statistical model can be solved without simplifications the researcher is on the safe side. However, given the extreme simplicity of two models discussed in this paper, I doubt that any relevant theoretical model would generate the statistical model that would be (at least numerically) tractable.

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Reply to Comments

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The main aim of the paper is to apply the image processing interpretation of the Maximum Entropy (MaxEnt) method to the Kirman (1993) model and the Abrams and Strogatz (20003) voter model as implemented by Stauffer *et al.* (2007). This follows the initial work in Barde 2012 which showed that the Schelling (1969) model of segregation can be predicted with the methodology. The discussant does point out some of the major issues that are associated with the methodology, many of which I agree with. The most important comment is probably the fact that more exploratory work is needed to establish a taxonomy of valid assumptions for corresponding statistical properties. Having said this, I feel that two important clarifications are needed.

My first comment relates to the claim that the assumptions or simplifications required to obtain the MaxEnt solution are arbitrary. Given some data d (the initial condition in agent-based models), the basic formulation for obtaining the prediction μ the maximum entropy problem is given by:

$$\max_{\mu}\left[\alpha S\left(\mu \mid m\right)+\ell\left(d \mid \mu\right)\right]$$

The first part of the expression, $S(\mu \mid m)$ is the relative entropy of with respect to a model m and $\ell(d \mid \mu)$ is the likelihood that the initial condition d is a noisy version of the prediction μ. For any given problem, two terms need to be specified: the model term m and log likelihood $\ell(d \mid \mu)$. While there is an element of 'educated guessing' in specifying these terms, this is not as arbitrary as the discussant claims.

— The model term m is a diffusion term which specifies how far the prediction can stray from initial condition, and this is the term that controls for time in the system. Intuitively, if very little time has elapsed, one should used a very peaked m, as μ will be very close to d. Conversely, long time horizons are represented with a flatter m. It is also important to note that m can have several dimensions, depending on the nature of the problem: one dimensional for the ants model, two dimensions for the Schelling and voter models.

— The likelihood term ℓ depends on the nature of the path linking the initial condition to the predicted state of the system. The image-reconstruction algorithm treats μ as the true image to be discovered and d as a noisy version of μ. This time-reversed path is conditioned on the fact that if the sequence of actions taking the system from its initial condition to its equilibrium distribution is best-response (a common assumption in economics), then the reverse path is effectively a noise process. The likelihood term is therefore determined by knowledge of the updating process, which determines the implicit noise process in the reversed path.

Both these terms are determined from the updating rules of the system, and are therefore not as arbitrary as it may seem. It is true that if little information is available (for instance if the exact transition probabilities are unknown), they must be approximated. For instance, in the generic version used for the voter model, both a gaussian likelihood $\ell(d \mid \mu)$, *i.e.* a gaussian noise process, and gaussian correlations over two-dimensional space for the model term m are assumed as an approximation. However this can be refined if more information is available from the updating process. This is the case in the ants model, where the transition probabilities are well known. In this case the model term is the diffusion of a stopped random walk rather than a gaussian diffusion and the likelihood is designed directly from a path integral of the transition probabilities.

Clearly, MaxEnt is no miracle solution: if the researcher has no information about the dynamic updating process of a system, then there is no way that knowledge of the initial condition alone can lead to a decent prediction of future states. In the Kirman ant model, for instance, the initial condition at $t = 0$ is simply a value $x \in [0,1]$ representing the share of ants of a certain colour. If the researcher is ignorant of the recruitment mechanisms, then x alone does not provide much information on the stable distribution of the system at a later time $t = n$. The central argument for using MaxEnt in the context of agent-based models is precisely that the updating rules of the system are known *ex ante*, as they are provided by the researcher.

My second comment is would be that the aim of the methodology is not to replace the traditional Monte-Carlo methods used in agent-based models but instead to provide a complement. The methodology is analytical in so far as the derivation of the maximum entropy problem is obtained from a rigourous Bayesian approach however, as mentioned by the discussant, in most cases a numerical methodology is required to solve for the solution of the problem. Furthermore, as pointed out by the discussant, the three simple models analysed so far with MaxEnt are a far cry from the complex systems routinely used in the agent-based literature. So given this, what is the usefulness or purpose of the proposed methodology?

An important application in my opinion is to provide a tool for categorising types of agent-based models according to the strength of their convergence to a stable distribution. A key finding of the paper, as well as the companion work on the Schelling model is that while the three models are clearly stochastic, the fact that they are amenable to MaxEnt prediction reveals that they are much more predictable that one might think. In technical terms, this is related to the fact that the image reconstruction MaxEnt algorithm works only if one is able to treat the reversed time-evolution of the system as a noise process, indicating that the time-evolution is in fact a finite improvement path. I agree with the discussant that more work is needed

In the future, rather than providing a direct solution tool for large agent-based model, a potentially important application for MaxEnt is the prediction of those component modules of the larger model that are amenable to MaxEnt. In interesting possibility in this regard is to take advantage of the faster execution speed of the methodology compared to Monte-Carlo to directly provide agents in the model with expectations, by using MaxEnt on the current state to obtain predicted future values for key state variables. Similarly, it could be used to speed-up large agent-based models by using the faster MaxEnt method on those components that are known to be amenable to the methodology.

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Comments on the paper

"Asymmetric (S,s) pricing: Implications for monetary policy" by Z. Babutsidze

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1. Summary of the paper

Firms' pricing behavior determine aggregate prices and therefore affect movements in aggregate prices and in aggregate output. Moreover the propagation of money supply shocks depends on pricing patterns, in general depending on the assumptions about financial markets monetary policy may have or not real effects. The present paper deviate from the presence of imperfections in financial market while the author concentrates on the assumption that firms in setting prices may deviate from the optimal level with non negligible consequences in terms of effectiveness of monetary policy not only during equilibrium periods in the business cycle but also during booms and busts.

The author develops, within a Dixit-Stiglitz framework, a structure os (*S, s*) pricing introducing an inaction interval around the price's optimal level. In particular, if the price is inside the interval the optimal behavior for the firm consists in not adjusting the price and it is well documented in the literature that monetary policy in this framework is neutral. However, once asymmetry in the inaction band above and below the optimal price is introduced money is non neutral.

Even if asymmetries at the micro and macro level are well documented by the empirical evidence it is well known by the profession that to study the link and interaction between micro and macro asymmetries is not an easy task. Therefore the aim of the paper consists in modeling and analyze the link and interaction between micro and macroeconomic asymmetries and in studying monetary policy effectiveness during different phases of the business cycle. In order to achieve this goal the author starts from a Dixit-Stiglitz monopolistic competition framework, adopts a model of firms' asymmetric pricing and builds an Agent-Based-Model (ABM) to perform monetary policy exercises. The author achieves two main findings:

— If shocks are sufficiently high the model reproduces significant asymmetries in the reaction of output to positive and negative macro-shocks.

— The economy under scrutiny responds differently to similar shocks across different periods of the business cycle. In other words the author finds an asymmetry in response to similar shocks during a boom and during a recession.

I think this is a crucial issue and very worth studying with new tools such as the ABM approach.

2. Comments

The author adopts a Dixit-Stiglitz (DS hereafter) monopolistic competition set up where the individual demand function faced by the firms is represented by:

$$Y = \left(\frac{P}{\overline{P}}\right)^{-\eta} \frac{M}{\overline{P}} \qquad (1)$$

where $\eta > 1$ is the price elasticity of demand, M is individual money supply, P the price set by the firm and $\overline{P}$ represents the aggregate price. The author seems to interpret M as an idiosyncratic shock but it is not clear from the paper which is the role of this shock and how it exactly works. It is not immediately clear to me what the author means by "idiosyncratic, mean zero shocks in money supply". A change in money supply is by definition an aggregate shock. I think it is necessary to explain in more details what is the idiosyncratic shock in the model and how it is eventually related to individual money supply.

On p.   lines   the author goes on stating that "idiosyncratic, mean zero shocks in money supply would call for no aggregate price changes". Why? Is there a typo in the paper? In the Dixit-Stiglitz model it is exactly the opposite, in fact a money supply shock does not have real effects since it completely translates into a change in the aggregate price.

In my opinion notation can be misleading I would rewrite individual demand following the specification below:

$$Y_j = \left(\frac{P_j}{\overline{P}}\right)^{-\eta} \frac{1}{n} \frac{M}{\overline{P}} \qquad (1bis)$$

where M represents total money supply, n is the number of firms and P_j is individual price.

The author defines the aggregate price as a simple average of individual prices in the economy:

$$\overline{P} = \frac{1}{n}\sum_{j=1}^{n} P_j \tag{2}$$

The definition of the aggregate price introduced into the DS model is more complicated than the simple average and the author does not explain why he is using the simple average instead of the aggregate price defined by DS:

$$\overline{P} = \left(\frac{1}{n}\sum_{j=1}^{n} P_j^{1-\eta}\right)^{\frac{1}{1-\eta}} \tag{2bis}$$

The implementation of the simple average to define the aggregate price does not seem to be coherent with the DS framework where the aggregate price level is derived from the household's minimization problem, therefore I would keep the original one specified into Equation (2bis).

Using the original definition of the aggregate price, taking the logarithms and totally differentiating you should end up with the following relation:

$$d\overline{P} = \frac{\frac{1}{n}\sum_{j=1}^{n} P_j^{1-\eta} dP_j}{\overline{P}^{1-\eta}} \tag{3}$$

Therefore, equation (14) in the paper will be slightly different and the effect of price elasticity on demand (η) does not disappear in the aggregate.

It seems to me that using a simple average to define the aggregate price you are ruling out by definition the effects of the parameter η at the aggregate level. What are the consequences of your assumption on the results? Will your results be different once you take into account relation defined into Equation (3) instead of Equation (14) in the paper?

To conclude I think that the paper deals with a really interesting and crucial issue and I am under the impression that it can make a non negligible contribution. However, it is hastily written so that only the insiders of this literature can retrieve the full line of reasoning behind few lines which briefly touch upon crucial developments. My suggestion is that if you want the "general economist" to be able to read, understand and take the message home you should try to fine-tune better the structure and the exposition of the paper and clarify the points mentioned above.

Reply to Comments

Zakaria Babutsidze
SKEMA Business School and OFCE

I am pleased to find that the discussant finds the paper interesting and I am grateful for her thoughtful discussion, which has raised few important points. In this note I want to follow up on three of them.

I want to start with the clarification on why idiosyncratic mean-zero shocks call for no aggregate price changes and why aggregate shocks with non-zero mean affect real economy. This is indeed at odds with Dixit and Stiglitz (1977). The reason is that in Dixit and Stiglitz (1977) prices of all producers at all times are in optimum. In contrast, our paper adds (S,s) pricing to the original framework. Therefore we deviate from this optimality feature. In our case price deviations from optimum have non-trivial distribution given by equation (9) in the paper. This feature introduces effective monetary policy in the setup.

The problem with modeling the shock process in the setup is duly noted. Indeed, I have not modeled the shock explicitly and I see why the process is hard to understand/interpret in the framework of the paper. Let me take this opportunity to clarify the issue.

The idiosyncratic shock is not M. What I had in mind instead is an idiosyncratic shock that hits the firm and calls for change in the optimal price. It is indeed very hard to think about the shock in per-firm money supply that can be idiosyncratic and discussant's proposal to rewrite the variable M/n would indeed expose this impossibility. It would have been a significant improvement on the current state of paper if I'd modeled the shock process explicitly and had made necessary adjustments so that the equation (3) in the paper read

$$p^* = g + m + \varepsilon,$$

where ε would be interpreted as an idiosyncratic shock *not related to money supply*. For instance, it could be a shock coming from production process, supply chain or some other entirely unrelated place.

Then it would be necessary to distinguish this shock from the monetary shock that is controlled by the government. We can do this by further changing the above equation to

$$p^* = g + m + \mu + \varepsilon,$$

where μ is the instrument of the monetary policy. It is government that decides on the size of μ. As the consequence the complete shock process that we are describing in the paper without modeling explicitly is $\mu + \varepsilon$. This process is distributed normally with the mean μ controlled by monetary policy.

The third issue that I want to discuss is the issue of the simple average as opposed to the weighted average used by Dixit and Stiglitz (1977). Using weighted average would change the results qualitatively. More precisely, the results of the paper are just the subset of more general results that would be generated by not eliminating η. After all, we would have an additional parameter to take into account. This would further increase the complexity in relationships that paper investigates. However, qualitative results would stay the same for the reasonable values of η. Different values of η would simply call for different definition of how large the shock should be in order for producers to adjust prices. As I am not calibrating the model, I believe the abstraction from the effects of η is justified.

This problem would, however, become more acute if I had proceeded to provide exact bounds for aggregate shock sizes that would call for price adjustment by all producers and thus make monetary policy (relatively!) ineffective. Despite this problem let me still elaborate on this issue in the framework of the model as presented in the paper.

Indeed, in this setup such bounds are calculable.[1] Recall the shock process is normally distributed in the paper. For further simplification of these results in this short format it is convenient to change the distribution of the chock process to the one that has a bounded support. Therefore, let's assume that shock process has a uniform distribution on support *[-u;u]*. In this case the size of the shock after which every producer has to adjust the price is $a+b+u$. If the size of the aggregate shock exceeds this bound it becomes ineffective, as every producer will adjust prices.

However, there is a small caveat in the reasoning.[2] In the framework of the model the pre-shock average price is always above the average optimal price. This relationship is given by equation (11) in the paper. In contrast, if every firm resets their price to the new optimum in response of a shock, new average price will be equal to

1. I thank the discussant and Domenico Delli Gatti for pointing this out to me in a private discussion.
2. I am grateful to Peter Howitt for pointing this out to me.

new average optimal price. This characteristic still leaves the room for the monetary policy. For demonstration of this implication consider the expansionary monetary policy on a large scale (such that aggregate shock accedes $a+b+u$). Even though in this case every firm adjusts the price the monetary shock will not be entirely absorbed by the price, as average price will increase less then proportionately to the size of the shock. This is exactly due to the fact that average firm was holding the price higher than its optimal price before the policy became effective.

What is peculiar in this mechanism is the response of the economy to contractionary monetary policy. In this respect the model produces somewhat counter-intuitive results. Because of positive average deviation from the optimal price, large negative monetary shock induces the fall of prices more then proportionally with respect to the contraction of the monetary mass. Then, it follows that large contractionary policy can stimulate real economy. However, this effect is not persistent as it goes to zero relative to the size of the monetary shock as size of the monetary shock increases.

Comments on the paper

"Macroprudential policies in an agent-based artificial economy" by S. Cincotti, M. Raberto and A. Teglio

Augusto Hasman
SKEMA Business School and OFCE

The recent financial crisis has shown that something was going wrong with the banking system and many researchers and policymakers agree that capital requirements should focus on the contribution of each institution to systemic risk more than on the specific risk of each institution in isolation (Brunnermeier *et al.* 2009, Squam Lake Working Group 2009, and Adrian and Brunnermeier 2008). This new macroprudential perspective tries also to reduce the procyclicality of the previous banking regulation.

Macroprudential policies in a Eurace-Model

The paper tries to replicate in an artificial economy some of the measures proposed by the Basel Committee in order to analyze their impact on economic performance. Those measures include the creation of a capital buffer during upturns to be use during critical periods. The authors use as the "conditioning variables", those that would determine the level of capital requirements, the distance between the actual level of unemployment and its threshold and also the distance between the actual level of credit growth and its threshold. If I understood well, the mechanics of capital requirements works as follows: when the level of unemployment is higher than its threshold, capital requirements are set at its minimum level (k_{min}), for lower levels of unemployment, capital requirements increase smoothly up to k_{max} (when the unemployment rate is 0). On the other side, when the rate of growth of the aggregate credit is higher than its threshold, the level of capital requirements is set at its maximum (k_{max}) while when it is below that threshold, it decrease smoothly up to its minimum level of k_{min} as a function of the level of unemployment. The authors choose 25% as a threshold for the unemployment rate, 5% for the case of credit growth, 8% for $k_{min}$and 12% for k_{max}. Since the paper tries to replicate the economy using real values, a natural question that arises is whether a 25% level of unemployment is a good threshold. Neither Spain nor Greece has attained such level of unemployment in the actual crisis and their banking system is already in a critical situation. Similarly, should all countries use the same threshold or not? This point has policy implications: should capital requirements be focused only on local economic conditions or should it consider conditions on partner countries? Probably, in a more and more integrated financial system, foreign conditions should also matter.

This work obtains very good results in terms of macroeconomic performance for the artificial economy. The dynamic regulation of capital requirements stabilizes the economy in the long run and improves the main economic indicators. However, in this model bank default risk is zero since the central bank is eager to inject money in order to prevent such event. Consequently, the difference between microprudential and macroprudential regulation for systemic risk disappear. The concept of systemic risk as the failure of a significant part of the financial sector vanished (Acharya, 2009). I believe that the introduction of an interbank market is necessary in this context to test the implications of different capital requirements configurations on financial stability and consequently on the real activity. Additionally, the simulations provide very high variability in the macroeconomic aggregates (for example, unemployment is higher than 40% four times in 30 years). A natural extension should include analyzing the sensitivity of the results to modifying the limits for capital requirements while keeping its dynamic configuration.

Future extensions should also consider the possibility of banking crises due to bank runs or bankruptcies. In line with previous comments, it would be interesting to analyze how different configurations for capital requirements affect the economic performance of differently concentrated banking systems.

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Reply to Comments

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Andrea Teglio
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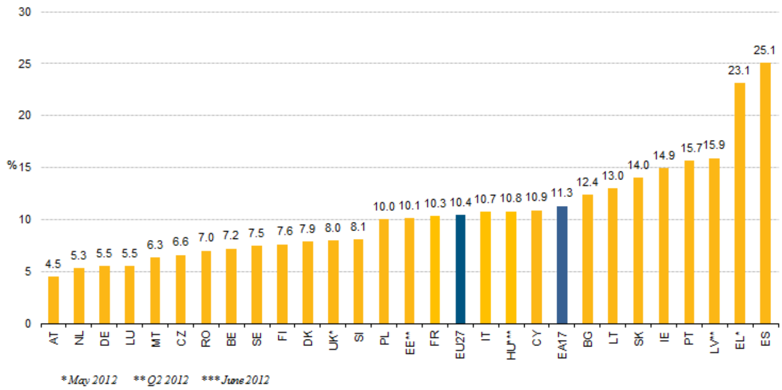
First of all, we would like to express our appreciation to Augusto Hasman, who carefully read our paper and indicated some important points that need to be discussed. This is of course an opportunity for us to clarify some aspect of the paper.

The first issue raised by the discussant concerns our choice of conditioning variables, *i.e.*, the economic indicators that should allow one to distinguish between good times and bad times. In particular, it is argued that a 25% level of employment is not a realistic threshold because neither Spain nor Greece has attained such a level, being their banking system already in a critical situation.

In this respect, it is worth noting that the threshold simply means that when the unemployment level is higher than 25%, banks are allowed to follow a looser regulation, with capital requirements at the minimum level (8%), in order to release the capital buffer that had been built-up during good times, i.e., when unemployment was lower. The macro-prudential rule changes therefore in the range of unemployment rate between 0 and 25%, thus considering a rate of 25% higher enough to be assumed as a threshold. Unfortunately, according to the last Eurostat unemployment statistics (see Figure), such unemployment level seems realistic.

Furthermore, we agree with the discussant that different countries should probably use different thresholds (according to their historical levels of unemployment and to the structural characteristic of the economy) and that foreign conditions should also matter. However, stated that the current version of the model only considers a single country context, we have been inspired by the current range of unemployment levels in the European Union in order to set the 25% threshold.

Figure. Unemployment rates, seasonally adjusted, June 2012



Source: http://epp.eurostat.ec.europa.eu/statistics_explained/index.php/Unemployment_statistics.

Later in his discussion, Augusto Hasman suggested to introduce an interbank market in the model, along with the possibility for banks to go bankruptcy. He argues that, with banks always fueled by Central Bank liquidity, the difference between micro and macro prudential rules for systemic risk could disappear.

As regarding the bankruptcy of banks, we agree with the discussant. Modeling this aspect is in our research agenda as it would be useful so to further improve the model and to understand and test the fiscal effects of bailing out policies. Nevertheless, systemic risk is already a key factor in the current model, but on firms' side. In this respect, Figure 5 in the paper shows the financial fragility indicator and this is clear example of the evolution of systemic risk in the model. When it is too high, an economic crisis is probably around the corner.

As regarding the modelling of bank runs and banks' bankruptcies these are also in our research agenda and we think that investigating their effects on government debt and fiscal policy would be systemically relevant and worth to be considered.

Conversely, concerning the interbank market, although it would certainly enrich the model we think it would not constitute a fundamental improvement. This because during the peak of a crisis interbank markets cease to function and the central bank is always available to provide the necessary liquidity to guarantee the functioning of the banking system, as the recent events have clearly shown. Furthermore, because solvency and not liquidity is the key issue in a banking crisis, in particular for what concerns their systemic effects and the sovereignty.

Finally, we would like to thank the organizers of the Workshop on "New advances in agent based modeling: economic analysis and policy" held in Paris June 19 and 20, 2012 at OFCE, Skema Business School for framing such a precious and stimulating event.

Comments on the paper

"Wage Formation, Investment Behavior and Growth Regimes: An Agent-Based Approach" by M. Napoletano, G. Dosi, G. Fagiolo and A. Roventini

Peter Howitt
Brown University

This paper is part of a series that uses the authors' Keynes+Schumpeter (K+S) model to address both short-run and long-run macroeconomic issues in an agent-based computational economics (ACE) setting. There are two features of the model that I find particularly appealing. First, it deals with the long-run growth consequences of factors which more conventional approaches have regarded as being strictly in the domain of short-run macro theory. In particular, the model is well suited to studying the long-run effects of wage flexibility, a factor that previous writers have taken as affecting only short-run deviations of unemployment from its natural rate. In the K+S model, as in reality, factors that prolong and exacerbate deviations from full employment can impede long-run growth. The paper does a nice job of laying out conditions under which this is more or less likely to happen.

The second aspect of the K+S model that I find appealing is that, like other ACE models, it is capable of dealing with possibly unstable adjustment dynamics that can contribute materially to short-run fluctuations. Unstable adjustment dynamics are ruled out by assumption in the more conventional rational-expectations-equilibrium approach, which assumes the economy is always brought into equilibrium by some unspecified, costless mechanism that uses no time and never fails to converge. By contrast, instability is always a possibility in an ACE model, depending on parameter values, and hence the ACE approach is capable, at least in principle, of shedding light on the circumstances under which, and the extent to which, the economy's adjustment process is likely to affect its macroeconomic performance.

These two aspects are interconnected. A central reason why short-run considerations have long-run consequences is that short-run deviations from full employment reflect coordination problems. When unemployment rises as an economy enters a recession, there are clearly gains from trade that are going unexploited. In that sense, recessions imply a kind of coordination failure; the economic system is failing to coordinate the beliefs and actions of the various actors within the system. But at the same time, such coordination problems have important long-run consequences, as we have understood at least since Harrod's demonstration that long-run growth can be affected by the difficulty of coordinating firms' investment plans with households' saving decisions. And to deal with these coordination problems for either short-run or long-run analysis, we need a non-equilibrium framework such as ACE.

The central message of the paper is threefold: (1) the functional distribution of income matters for long-run macroeconomic outcomes, (2) wage flexibility can affect these outcomes, but (3) the strength and direction of these effects depend critically on whether investment and production decisions are driven by profitability (the Classical view) or by expectations of aggregate demand (the Keynesian view). In all cases, it seems that low unemployment and stable high growth require an intermediate distribution, with not too large a share going to either labor or capital. But wage flexibility is helpful mainly under the Classical view. These conclusions are all drawn from repeated simulations of the model under alternative parameter values.

The paper does a nice job of explaining its results. I have some quibbles, however, about some of the authors' modeling choices. In particular, their model as presently constituted allows for a very limited subset of all the possible channels through which wage flexibility can affect macroeconomic performance. Thus, the paper has no discussion of the unstable debt-deflation dynamics that could be triggered by excessive flexibility, no zero lower bound on nominal interest rates that might get hit more often if wages were more flexible, no inflation uncertainty that might get exacerbated by more flexibility, and no inflation expectations that could be destabilized if wages were flexible enough. All of these channels tend to make increased wage flexibility a force for worsened macroeconomic performance; all of them have been discussed in the literature at one time or another, going back through Patinkin and Tobin to Keynes and Fisher; and all of them would tend to undermine the particular conclusion reached in the present paper to the effect that wage flexibility generally helps under the Classical view and has little effect on macro performance under the Keynesian view.

Moreover, the channel that is operative in the present model is one whose effects I think are probably overstated. That channel is the one that works through the functional distribution of income - when wages fall in response to unemployment, labor income falls and capital income rises, relative to what they would have been with less flexibility, and this in turn causes a further drop in aggregate demand, and hence in output, because of the assumption that the marginal propensity to consume out of capital income is lower than out of labor income.

Now it may indeed be true that in most countries the MPC out of capital income is less than out of labor income, although I don't know of a paper that has carefully estimated the difference. Nevertheless, I doubt if that difference is anywhere near as large as is assumed in this paper, where capital's MPC is set to zero, and labor's MPC is set to unity.

This is not to say that the paper's main results are wrong. Even though the paper is missing many of the channels through which wage flexibility might impede the restoration of full employment, it could end up with the right overall effect because it exaggerates the one channel that it does include. But errors in opposite directions cancel only by coincidence. I would like to see the authors incorporate these other channels and to attempt a more realistic analysis of the differential MPC channel. This should be quite doable, because one of the virtues of their ACE approach is that there is in principle no limit to how many channels of influence one can consider, since analytical tractability is no impediment to the generation of numerical results.

In summary, the paper makes it clear that the K+S model has a great potential. I look forward to seeing more of that potential realized in the authors' future work.

Reply to Comments

Giovanni Dosi and Giorgio Fagiolo
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Mauro Napoletano
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First of all, we thank very much Peter Howitt for all comments on our paper. They are very insightful and provide a roadmap for future extensions and exercises with the K+S model.

One of the main issues raised by Howitt concerns the channels through which *nominal wage flexibility* can affect macroeconomic performance. Howitt rightly points out that in this model wage flexibility can affect aggregate dynamics only through the *income distribution channel*. We do it on purpose, motivated by the attempt to understand how income distribution can affect the aggregate dynamics of the economy, and in particular its self-recovery capabilities after an adverse shock. More precisely, in our previous work (Dosi *et al.*, 2010), we studied whether in presence of nominal wage flexibility the labor market converges to the full-employment equilibrium. In line with the intuition of Keynes (1936), we found that wage flexibility does not reduce unemployment. In the current work we tested the robustness of the aforementioned result under different income distribution scenarios and different rules of firm investment behavior. We find that nominal wage-flexibility to unemployment is either ineffective or counter-productive in the scenario that is probably the more realistic nowadays, *i.e.* a "demand-led" regime. In turn, this result complements those in Section 2.1 of the paper showing that both the short- and long-run performance of the economy are lower when the income distribution is too unbalanced. Some advanced economies (and the US in particular) have experienced a significant increase in inequality in the recent decades. Our analysis contributes to show that in some circumstances the increase in inequality can yield higher volatility and lower growth, which are not curbed by wage flexibility.

We certainly agree with Howitt that we should explore all the other possible channels through which wage flexibility impact on macroeconomic performance, such as debt-deflation dynamics, zero lower bound on the interest rate, inflation uncertainty, inflation expectations. Indeed, we also conjecture that the presence of these mechanisms is likely to reinforce the adverse effects of income inequality and nominal wage flexibility observed in the demand-led regime, therefore strengthening some of the basic messages of our paper.

Another important point raised by Howitt, relates to the difference in average and marginal propensities to consume (MPC) between workers and capitalists, which in our model is assumed to be very large. Howitt is definitely right in pointing out that this assumption plays a central role in generating most results of the paper. However, our results hold as far as that the marginal propensity to consume of capitalists is lower than that of workers (see Kaldor, 1955, for the original formulation of this argument). In any case, we plan to extend the model allowing for households savings and indebtedness (which indeed played an important role in the recent crisis). Again, we conjecture that by introducing this extension, many of the results of the K+S model about the effects of fiscal, monetary and credit policies (Dosi *et al.*, 2012) will turn out to be reinforced.

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Comments on the paper "Production Process Heterogeneity, Time to Build, and Macroeconomic Performance" by M. Amendola, J. L. Gaffard and F. Saraceno

Pietro Peretto
Duke University

This paper contains two parts. The first one summarizes most of the previous work of the authors on the out-of-equilibrium approach in macroeconomics. The second one discusses the implications of this approach for several current issues. I enjoyed very much reading both parts of the paper. What I liked in the first part of it is the attempt to offer a different framework for the analysis of macroeconomic phenomena. The approach proposed by the authors is different, because it tries to incorporate important observations, often ignored in other approaches (both mainstream and non-mainstream). Moreover, I found that it takes seriously into consideration the issue of conceptual foundations in macroeconomic analysis. Finally, I think that, in the second part of their paper, the authors propose very detailed arguments to justify the relevance of their approach for the analysis of current issues. In addition, this section of the paper really forces one to think (hard!) about current events and the ability (or lack thereof) of mainstream economics to explain them.

To sum up, I think the paper can provide a useful roadmap to understand how and why different theoretical approaches deliver different results. Accordingly, it can potentially serve as a useful guide to map specific results to data. At the same time, I also think that the paper needs some improvements. First, since the paper aims at clarifying the general structure of an approach (the out-of-equilibrium one), it is important to be extremely precise about what the approach does relative to the frequently criticized mainstream. I have some quibbles with some of the characterizations of the model discussed in the paper. This is also because the main components of the core model (and their interactions) are not discussed in detail. Moreover, I think the link to empirics is often left to reader's imagination. Instead, I think it would have been better to show different examples of how specifically the approach outperforms alternative. Finally, let me conclude with a remark that can be generalized to several other papers in this special issue that, like this one, propose interesting alternative approaches in macroeconomics. I think that a relevant question is about who is supposed to be target audience of those papers. Those who already share the message of them? Or those who do not, and should be won over? If the target is of the second type than my candid opinion is that this paper, as many others in the heterodox and orthodox literatures, try to put too much effort into showing that the proposed model is definitely better than the alternatives. As a researcher working in the mainstream but also very skeptical about it, I think instead that a more effective approach would rather consist into acknowledging not only the evident limits but also the potentialities of the mainstream approach, and try to understand how both mainstream and non-mainstream economists can learn from each other and do better macroeconomics!

Reply to Comments

Mario Amendola
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First of all let us thank Pietro Peretto for his useful and "candid ", in his own words, remarks. Leaving aside the positive part of his review, we see two main criticisms. The first is that we build our argument too much "against" mainstream, and too little based on its own merits. This is a remark that we take on board, as we always saw our work as complementing, not substituting standard equilibrium analysis. We claim that out-of-equilibrium sequential analysis is best fit to analyze processes of qualitative change (like technical progress), but we'd never advocate it, for example in the field of consumer choice. We try to make clear in our papers that we aim at adding to the mainstream, not at substituting; Pietro's remarks show that we still need to make a communication effort.

We are also sensitive to the second remark, which is the necessity to bring the model to the data. Here we have two "comments to the comments". The first is that the framework, as it is now, is yet very difficult to be put up to empirical validation. To make an example, the interaction of adaptive behavior and irreversibilities in investment typically involves, in the model, "dented" fluctuations that are nowhere to be seen in real data. Does it mean that the model is not good? We do not believe so. At the price of further additions to an already complicated model (for example introducing some sort of inertia in consumption or in expectation formation, or longer time to build periods), we could smooth these dents, and obtain time series that are more realistic. It will have to be done, but for the moment we preferred to focus on the qualitative properties of the model, and assess whether they allow to make sense of phenomena of change. In some cases, for example in the case of the productivity paradox mentioned in the 2005 paper, we argue that these qualitative features help to make sense of stylized facts that are paradoxical in equilibrium theory.

Finally, we took on board Pietro's comment about the somewhat cryptic formal analysis of the paper. The reader is still encouraged to go to the original papers, but we expanded the appendix in order to make the paper more self-contained.

Comments on the paper

"Structural Interactions and Long Run Growth: An Application of Experimental Design to Agent-Based Models" by T. Ciarli

Maurizio Iacopetta

OFCE

The wealth of nations depends on a great number of factors. Classical economists emphasized the role of accumulation of physical capital, as well as the constraints of natural resources. Solow also shared the view of classical economists, except that he pointed to the importance of technological change. Romer (1986) however noticed that the emphasis on capital accumulation would lead to a conclusion of accelerating growth in the earlier phases of development, and at a dismal growth in more advanced stages of development. Such a conclusion, he argued, is patently against any historical records. If Romer pushed the line of the market mechanism that favors the appearance of new technologies, as a way to explain the modern acceleration of growth, Lucas (1988) hypothesized that human capital formation has an even more fundamental role. Arrow (1966), in an article so much ahead of his time, thought that the accumulation of capital and the adoption of a new technology are indeed just two faces of the same coin, for usually investments carry also new technologies. Contrary to many neoclassical economists, he preferred a fixed coefficient production function, underplaying the role of substitution of labor and capital as a key mechanism in the process of growth. Ciarli (2012) follows somehow Arrow's approach, in the production of goods, by giving an explicit vintage structure to the production process, but enriches it with further elements to account for institutional aspects of the labor market, where there are three types of individuals – managers, engineers, and production workers –, and to incorporate modern developments of the innovation literature. Indeed not only both horizontal and vertical innovations are modeled, but also the introduction of a new product proceeds in two separate phases – invention and development of a prototype.

Since the literature has emphasized, investments, innovation, structure of the labor market, composition of consumption, inequality, initial endowments, as the key elements of the long run fortune of advanced countries, Ciarli sets up an exercise that delivers us a ranking of these factors. This is indeed what policy makers ask for whey they turn to economists, only to get frustrated when they discover that every economist has a strong prior of what matter most for long run growth.

Ciarli goes some way towards remedying this defect of the profession, by proposing a model that encompasses a great number of elements found in different literatures of macro and growth. Since the research question he sets up is a quantitative exercise of evaluating which features matter more in the explanation of modern growth acceleration, inevitably he has to face the problem of assigning sensible parameters values to technological, institutional, and preferences parameters. Once such set of parameters are found, he runs the numerous difference equations of the model several times, and uses the statistical properties of the calculated patterns to infer the likely behavior of the economy. If the values of some parameters are drawn from an extensive empirical literature, such as the capital depreciation rate or the wage premium for engineers, others are kept on the side in order to earn degrees of freedom in running the simulations. The list of free parameters include the arrival rate of new goods, the rate of improvement of capital goods, the ratio between supervisors and supervisees, and the wage premia. It also includes preference parameters for basic as opposed to sophisticated consumption goods, and of low-quality as opposed to high-quality goods.

To introduce some discipline into the experiment, Ciarli wisely picks up a lower and a higher bound of the free parameters in light of values estimated in the literature – or in absence of such estimates according to values suggested by the structure of the model itself.

A second discipline device he uses is the fact that the productivity series and output series generated by the model are to be verified against the historical numerical data. Ciarli chooses not to commit to any particular country as far as data are concerned, for, presumably, all developed countries followed at a similar path, at least at low frequency, in the last two thousand years – the length of the experiment.

Ciarli studies the importance of a given parameter, by running simulations under each of the extreme values of the parameters and verifying the extent to which each parameter affects the patterns of the simulated time series, for all possible combinations of the remaining nine parameters, and for a given set of the fixed parameters. ANOVA techniques are used to assess the statistical significance of each factor.

The main outcome of the analysis is that technological change and the conditions of production are relatively more important than the organization of the labor market in explaining differences in growth rate. Ciarli also found little evidence that the preferences on different types of goods are important for long run growth. Interestingly, the ranking of the factors essential for growth seems to reflect the priority the literature has set into it. Indeed, as I said at the beginning, modern growth theory developed mostly around the right incentives to introduce innovation. What is perhaps not in line with the emphasis given by scholars of growth is the high significance of income distribution. This is a welcome suggestion for current and future research.

I should note, however, that the model does not allow to important forces to play any role. In particular, there is no human capital, no trade, and there are no financial constraints. Furthermore, it is not clear whether the size of the market or of the population could be tested. These elements have been widely cited as a possible source or a barrier to progress. A policy maker would probably like to know at least the importance of education and the role of financial institutions.

I also have a reservation about the amount of thrust we can have on the ranking of the factors essential for progress, even conditional on having some of them excluded. A scholar of the field would be tempted to have a lot of thrust in it, for it echoes the emphasis of the theoretical and empirical literature on the subject. But the dynamics are brought into light only through simulations, depriving the reader of the power of judging the main mechanisms of the model.

As for the simulations themselves, I would have liked to read a more elaborated explanation of why simulating the economy under extreme and unlikely parameters generates good information for producing realistic time series.

Reply to Comments

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First, I would like to thank Maurizio for a very positive and open comment on the paper, which is the outcome of a few years of intermittent collaborative work with a number of friends and colleagues: André Lorentz, currently at the Université de Technologie de Belfort-Montbéliard, Maria Savona, at SPRU, University of Sussex, and Marco Valente, based at the University of L'Aquila. Over the years we have combined our common interest in (long run) economic change as an outcome of technological change, to investigate different dimensions of structural change. We have inclined towards interpreting structural change as an intrinsic aspect of economic evolution and the steady—albeit cyclical—economic growth experienced by many world areas in different epochs. Each of us had a different research background (within the evolutionary/Schumpeterian tradition), covering Keynesian-inspired approaches to growth theory, the sectoral and organisational transformations that lie behind the service industry, consumption behavior, and the structuralist school of economic development. All these ingredients are reflected in various aspects of the modeling strategy in the paper. An additional aim of the paper written for this special issue, as clearly discussed by Maurizio, was identifying which of these approaches to economic thinking points to aspects with the greatest fit to explain long term economic development when we include a number of causes of structural change in the model.

My reading of the comments implies that the paper does not put sufficient stress on some important aspects and motivations of this paper, particularly with reference to the method used and the centrality of (different aspects of) structural change. I briefly comment on those before moving to Maurizio's reservations. On methods, we adopted simulation modeling (essentially agent-based) for two main reasons. The first is related to the complexity of the subject studied. This Special Issue more than adequately shows how much we can learn by constructing macro economic models of agent properties and their interactions. This is particularly true to investigate the changing structure of these interactions in the economy. Structural change implies that the relations between the agents in an economy vary through time (e.g. due to an increase in income, satiation in consumption, change in preferences, change in the composition of goods or in the working relations). As it is summarised in a quote already used in the paper, "What does it mean for a system to be in equilibrium when its composition keeps changing due to the emergence of qualitatively different entities?" (Saviotti and Gaffard, 2008, p. 116). Simulation modeling (agent-based) allows to model these changes as emerging properties of the model, *i.e.* occurring only under given conditions—that can be endogenous or exogenous, depending on the degree of complexity introduced in the model.

On the second aspect I would like to stress, while the paper assesses a number of classical and evolutionary forces of economic growth and (structural) change highlighted in the literature—as commented on brilliantly by Maurizio—there is more to this paper: which is the interaction between different aspects of structural change. For example, the model combines the relevance of physical capital (and its vintage structure), of technological change in the production of new vintages, of their relation with the labour structure, of how this in turn affects the level and composition of demand, and on the effects of product innovation leading to the emergence of new sectors supplying goods to consumers with different characteristics. In other words, this is not only a comparative dynamics exercise to assess the relative importance of each parameter proxying a factor of growth; it is first of all an analysis of how different aspects of structural change interact with one another.

In this respect I then would like to put more emphasis on the results showing how the effect of single determinants of structural change is significantly modified when other aspects of structural change vary. For instance, it is one thing to say that capital accumulation is relevant, especially when capital brings technological change, but quite another to stress under which conditions capital accumulation is more or less relevant. With respect to the pace of embedded technological change, for example, the model shows that a fast pace has a negative effect on output in the presence of a strongly unequal organisation of labour. This is explained by lower total demand: very large productivity gains reduce the price of goods, but also the number of vacancies. Also, large wage differences concentrate demand in the high income classes with preferences insensitive to prices, not inducing firm selection based on price differences. As I briefly mention in the paper, firms' heterogeneity and concentration of production—both linked to demand—are fundamental sources of growth in this model.

This brings me to the second reason for our adopting simulation modeling in this long term project: we think that this method allows us to capture the mechanisms behind aggregate behavior. Unfortunately, as rightly noted by Maurizio, for space and time reasons I was unable to include a description of the mechanisms behind each individual result in this paper, something that, as suggested, would have improved the credibility of my results. The paper describes only the main mechanism driving the transition from linear to exponential growth (beginning of section 4). This mechanism turns out to explain many of the results in this paper: given that we run the model for a limited number of periods, the main differences in output level and average growth lie in the timing of take-off. In essence, under most of the thousand combinations of conditions examined in this paper economies reach take-off within the first 2000 periods. However, the sooner an economy makes the transition from linear to exponential growth, the larger are the output levels and average growth rates. To return to the main point of the explanation of mechanisms and economic phenomena, the advantage of simulation modeling is that it allows long study of the reasons behind each result, and of the reaction of agents to changes in other agents and in the system. Clearly, there is the need for another paper or, better, a separate paper devoted to each parameter analysed here.

When building complex models there is always a trade off between maintaining a simple structure leading to intuitive results, and adding more sources known to affect the studied phenomenon. As Maurizio suggests there are many other factors that are known to affect growth, at least in the medium term ?, (e.g. Durlauf *et al.*, 2008). Education and institutions are definitely factors with a strong effect on long run growth, as illustrated by numerous modern theories of economic growth—respectively, e.g., Acemoglu *et al.* (2005), Adam and Dercon (2009), Rodrik (2007) and Galor (2010). But stochastic effects and 'initial' conditions also have an influence (Diamond, 1997). Access to finance and trade technologies have been major structural changes in the organisation of the economy. The latter is the focus in, for example, Hausmann and Hidalgo (2011) and Galor and Mountford (2006). The first of these papers links the diversification of production (and exports) with initial endowment and further development of skills, showing that only high capability goods sustain high income growth. The second paper assumes that the returns from trade in manufacturing are invested in human and physical capital while returns from agricultural exports are used to sustain population growth. Both provide very useful insights into one possible determinant of long run growth. Compared to our model these models assume a relation observed in the data, and show one mechanism at work. Rarely are more than two mechanisms (trade and skills, or rule of law and investment) considered together and their interaction analysed rather than assumed from the outset. Finally, in our model the size of the market is a crucial determinant of growth (Ciarli *et al.*, 2010).

I agree that it would be extremely interesting to study how introducing access to finance, education, and trade, would change the model results. For those willing to pick up from this project, I would also suggest focussing on the organisation of production, introducing explicit outsourcing and vertical integration. However, as mentioned, there is a trade off between the ability to explain each result and the number of experiments required to analyse all combinations of the parameters. The number of simulations needed to study five aspects of structural change (5 determinants of growth) is already extremely large; adding more factors increases the number of combinations exponentially. But there is one further aspect that is important to note here. All the determinants studied in this paper, and those suggested by Maurizio, are studied as proximate causes of growth—in the sense of Abramovitz (1986). What would be more useful for policy makers would be to know which type of education—with which incentives, disciplines, quality, methods, and so on—which type of finance, and especially which types of institutions—from rules of law to individual beliefs. But these are elements that, for the time being, cannot be modeled as deep sources of growth.

I then come to the final reservation in the discussion, on the use of limit values of the parameters. Choosing a 2^k full factorial design to assess the relative effect of all unknown parameters determining structural change required the selection of two extreme values. The choice of values above or below those observed is due to the choice to consider the whole parameter space. It might be rare for a firm to employ one manager at each tier $n - 1$ to supervise three employees working in the tier , or for a firm to invest all of its profits in R&D. But it is not impossible. The idea behind this paper is that one first explores what are the most relevant parameters in looking at the extremes in the distribution, that is, possible although very unlikely, and then focusses on those most relevant parameters to analyse non-monotonic effects between the two extreme values.

I want to end by commenting on whether this exercise is really useful for policy makers. Qualitatively, I would say, it suggests aspects of societal transformation that are more relevant for explaining long run growth than others. These aspects are probably worth exploring in more detail, moving from what are still proximate causes to the causes underlying the economic mechanisms. Quantitatively, I would not suggest that policy makers should believe in these numbers. What should be of use to policy makers is the availability of more powerful tools and methods to analyse economic change than standard equilibrium models.

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Comments on the paper
"On the Co-Evolution of Innovation and Demand" by P. P. Saviotti and A. Pyka

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The paper presents an extension of the TEVECON model in Saviotti and Pyka (2004) and following papers. This model exploits the Schumpeterian approach to growth conceived as a process of sectoral life cycles and sequential structural changes. In this version, the demand function takes into account two dimensions of sectoral production: differentiation and quality. Thus, besides the quantity path along which new sectors spread over the economy, the sectoral life cycle is also characterized by a path of differentiation and qualitative change occurring within each sector. Such process of sectoral change is driven by the search activities induced by the "accumulated demand". The goal of the paper is to derive some policy implications from the analysis of the interactions between demand and innovation along a process of growth having the correspondent three dimensions (defined as the "the three trajectories"): productivity, variety and quality. The dynamics analyzed consider different scenarios concerning the three trajectories. First, on the supply side, different effectiveness of the process of qualitative change (Low-quality versus High-quality scenarios) are considered, then, on the demand side, the differences concern the consumers' propensity to novelties.

Consumer propensity to novelties is shown to have a non linear relationship with growth. Indeed, while conservative preferences harms the emersion of new sectors, highly progressive preferences harm the complete expansion of more mature sectors. Also the LQ and HQ scenarios analysis confirms such trade-off between the rate of new sectors creation and the duration of the industry life cycle. Furthermore, the LQ scenarios is better performing in the short term but brings to a worse result on the long term. All such effects are reinforced in case of very dynamic economies, that in TEVECON are related to higher wages and faster human capital accumulation. A deeper insight or a further extension stems from a less intuitive trade-off emerging from the simulations: the one between quality and employment. Indeed, lower quality scenarios display higher employment growth rates. A similar dualism between alternative models of growth with slow sectoral cycles, high labour intensity on one side, high pace of innovations and growth on the other side, may bring to complex policy issues, in particular when, as in present times, the quest for growth recovery cannot be detached by the need to tackle high unemployment rates.

The paper also attempts to position TEVECON into the wide growth models literature. The authors state that one of the main differences with "orthodox" models is the lack of "complete closure conditions such as general equilibrium". This feature, together with the strong non-linearities of the equations, is also used to justify the use of simulation methods. At the same time, I would suggest the authors to couple simulation analyses with a deeper anaylitical description of the properties of the model. For sure, the model can't be solved since it is impossible to find a set of dynamic equations fully characterizing the dynamic of the main variables. However, it is probably possible to check whether the model is compatible with stable dynamic configurations such as steady states, steady growth trends, cycles or other well detectable although more complex underlying dynamics. This type of study can be very helpful when, as in this case, the main conclusions of the papers are based on the analysis of the long run dynamics and not (as instead in the cited "out-of-equilibrium" literature) on the short run traverse issues. As an example, in the present paper, the dynamic paths that emerge in the medium and long-run from by simulating the model do not seem to be significantly different from paths characterized by stable growth trends, with a cyclical steady increase in the number of sectors and cyclical components corresponding to the life cycle of the youngest sector. In this perspective, if equilibrium is defined as both the partial equilibrium in each market and the attainment of a stable configuration at aggregate level, the dynamics displayed in the long-run mainly result into "equilibrium" configurations. Such "compatibility" features of the model could have been partially investigated by looking at the structure of the equations (among which, in particular, the bounded, symmetric and convergent nature of most functions) before running the simulations.

Another argument on computational models methodology in general concerns the role of initial conditions. I see a tendency to run simulations and analyze the results of the model without a preliminary theoretical analysis of the hypotheses underlying the specific initial conditions and of their heuristic implications (a feature which is shared by many computational models which are "self-initialized"). In my opinion, this approach is not suitable when the dynamics is characterized by path dependency.

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Reply to Comments

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For what concerns the relationship between the Low Quality (LQ) and High Quality (HQ) scenarios, it must be pointed out that the LQ scenario leads (i) always to a higher rate of growth of employment, but at the price of stagnant wages, demand and human capital, and (ii) to a higher rate of growth of income only in the early part of the development process. These results can be compared to the observed real development paths which show that for successful economic systems the HQ scenario started to dominate at times variable between the early and the late 20th century for different countries. Thus, it seems that a transition occurred between an LQ scenario, which dominated during the 19th century, and the HQ scenario which emerged during the 20th century and subsequently became dominant. Our model predicts that such a transition had to take place if the economic system was mainly driven by income generation rather than by employment generation.

The policy implications can be complex because the patterns detected for the long run do not automatically provide us with the best policy guidance for the short run. Fig. 8 shows that the timing of the transition between the LQ and the HQ scenarios depends on the combination of different model parameters. This implies that a pattern which applied generally to the relationship between some variables, such as wages and growth, can take different forms in each short run period. For example, while growing wages were an important component of the observed economic development path, we cannot assume that raising wages at any given time will affect positively growth.

The suggestion to provide a deeper analytical description of the properties of the model is welcome. We are working on it. TEVECON is constituted by a general core, common to all extensions, and by extensions which explore particular aspects of the economic system. We have given a complete description of the core in Pyka and Saviotti (2011) and refrain from it in journal papers mostly for reasons of space. However, we accept the referee's suggestion and we are working on a 'compact' as well as graphical description which can be used in different papers.

To test the stability of TEVECON we have carried out several explorations of parameter space, in addition to those which have been published to make sure that TEVECON's results were not too sensitive to small changes of parameter values. These explorations showed that a) in general TEVECON is not unduly sensitive to such variables and that b) depending on the region of parameter space TEVECON can give rise to self-sustaining development or to the collapse of the economic system, which would then loose the capacity to create new sectors and to support the existing ones. For the stability and robustness tests we compile so-called corridors which describe parameter spaces with stable qualitative development paths (e.g. Saviotti and Pyka, 2004, Appendix). Furthermore, we differentiate the steady states that we can find from a general equilibrium. A general equilibrium is not compatible with an economic system characterized by endogenous innovation and changing composition. We have local equilibrium between demand and supply at the sector level.

TEVECON has some parameters based on initial conditions and we explored their impact on economic development. In some cases their impact on predicted growth patterns is limited, in others more noticeable, but never as large as to completely change economic development patterns. A sensitivity of development paths in dependence of small deviations from starting values cannot be observed. However, we agree with the referee that further work in this direction would be useful.

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Comments on the paper
"Environmental taxes, inequality, and technical change" by F. Patriarca and F. Vona

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Assessing the effects of environmental policies is a highly valuable enterprise, for a number of reasons. Scientific evidence is piling up about the relationship between global warming and human-induced emissions of greenhouse gases (IPCC 2007). With their countries stuck in the deepest recession since the 1930s, governments can reignite growth by stimulating improvements in energy efficiency. Fighting pollution can save the lives of many, while it can keep public health spending under control.

Patriarca and Vona perform this assessment by means of a theoretical model built around a well-defined causal mechanism: a fall in the relative price of the green good fuels adoption, which in turn feeds back into further price decrease via learning economies, much in the spirit of Cantono and Silverberg (2009) (see also Vona and Patriarca 2011). An environmental tax can act as a trigger for this causal mechanism, as it affects the relative price of green goods. The effects of the tax are mediated by the average income level, income inequality, and the rate of technological learning. Hence, the paper is essentially an investigation on a causation process in which the environmental tax is the cause, and diffusion of the green good is the effect. Income inequality, which plays a prominent role in the paper, can be seen as a moderating factor.

I see a lot of potential in exploring the relationship between income inequality and the environment. One of the main results of the paper is that, in a high income country with sufficiently fast learning, income inequality slows down the diffusion of the green technology, because the distance between pioneer consumers and the remaining population is too high. Far-reaching implications can be drawn from this finding. It has been shown that the increase in income inequality witnessed in recent years is at least partly an outcome of financialisation, which has caused an explosion in executive compensations (Finnov 2012). The inequality-diffusion relationship found by Patriarca and Vona implies that, by achieving a more equal income distribution, financial reforms that overturn the financialisation trend can improve the effectiveness of carbon taxes on green technology diffusion, in a sort of institutional complementarity (Aoki 1992) between financial and environmental regulation. The ensuing improvement in energy efficiency would make firms more competitive, throwing light on an interesting transmission channel between the financial and the real sectors of the economy. Hence, the insights provided by the paper go well beyond the mere assessment of an environmental policy measure. Policy-makers should definitely pay more attention to the inequality-environment nexus. Nevertheless, in what follows I shall discuss a number of issues, in hope that the authors could further improve along their highly promising research path.

Let me tackle the issue of consumer behavior first. The model depicts consumers as endowed with full, substantive rationality, who maximize utility and interact only through the price system. Income is the only source of heterogeneity among consumers. These are highly stylized assumptions that the authors are going to relax in future research, as I understand from their concluding section. In their exhaustive review paper, Gsottbauer and van den Bergh (2011) give important hints on the implications of bounded rationality and social interactions for environmental policy. As a take-home message, it is increasingly recognized that policy design needs to take the behavioral evidence into account. For instance, Janssen and Jager (2002), Schwoon (2006), and Cantono and Silverberg (2009) postulate that consumer choice depends not only on the level of personal need satisfaction, but also on social needs. In other words, individuals are placed on a social network, and their choices are affected by the choices made by their neighbors. Status considerations and the quest for information on new goods justify this. This given, maximizing utility in isolation is considered as just one possible cognitive processing mode. Consumers in Cantono and Silverberg (2009) are pure imitators. In Janssen and Jager (2002), consumers can alternatively engage in repetition, if their satisfaction is maximized; in social comparison if changes in the surrounding environment cause the satisfaction level to drop (i.e. comparison between past choice and the best choice made by the neighbors); in imitative behaviors after increases in the variability of the satisfaction level. Consistently, the pro-environmental impact of price differentials can be overestimated if the consumer is represented as a pure *homo oeconomicus* responding to only monetary incentives, neglecting behavioral biases (see the results in Janssen and Jager 2002, as well as the default option and endowment effects in Pichert and Katsikopoulos 2008, who performed laboratory experiments on the switch to green energy). Another issue of potential interest for environmental policy assessments is that, by neglecting other-regarding preferences, policy analysis could place too much weight on efficiency goals and too little on equity and fairness, that may be highly valued by agents embedded in a network of social relationships.

Concerning the supply side of the economy, in the Patriarca-Vona model exogenous improvements in technology and deliberate firm-level innovative outcomes are collapsed into a single learning parameter. Explicit modeling of firm-level green technology decisions, however, would allow to separate the two learning determinants and analyze how the effects of a carbon tax interact with innovation-related policies. Results from Janssen and Jager (2002) show that the effectiveness of a tax policy depends on the balance between imitators and innovators (the former slowing down diffusion), as well as on whether firms adapt their products at all (e.g. if they perform R&D to either innovate or imitate). One may also suppose that investments by innovation-oriented firms give rise to a positive externality on the willingness to pay (WTP) for green products: R&D investments stimulate the returns to education, and a more educated workforce is more concerned with environmental issues.

Finally, I would suggest that a fully dynamic policy analysis, cast in a coevolutionary framework, would provide further hints as to the long-term effects of environmental taxation. For one, focusing on certain tax rates in simulation scenarios hides the presumption that such measures are politically viable. Whether this is the case, it depends on pressures on policy-makers by interest groups, including those representing firms, that are not modeled in the paper. Studying how policy-making and firms strategies (including lobbying) coevolve could be a fruitful area for future research. As a further issue, the model includes no assumption on how environmental tax revenues are spent. This common modeling choice is safe if the use of tax revenues is neutral, which is hardly the case. Tax revenues can be used to invest in (green) public infrastructures and R&D subsidies, as well as in public education which may determine an increase in the WTP for green products and better preferences against income inequality. Conversely, tax revenues may be wasted by bad politicians, possibly hindering green technical change. It may be interesting to explore to what extent the results of the model hold even after considering such scenarios: agent-based modeling is particularly well suited for dynamic policy exercises.

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Reply to Comments

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We wish to thank Alessandro Sapio for the very useful and stimulating comments. Two interesting points are raised by the analysis of Sapio.

First, He suggests that the assumption of fully rational, autarkic agents can be misleading in view of growing experimental evidence on the role played by social norms and reciprocity in human behavior. In particular, it is likely that, as, e.g., in Cantono and Silverberg (2009), consumers' willingness to pay (WTP) for green products is affected by the WTPs of their neighbors. Including peer effects in consumption would certainly have relevant implications for our analysis giving an active role both to local (*i.e.* spatial) and aggregate income inequality. The spatial distribution of agents endowed with different income levels would affect the distribution of preferences for a given aggregate level of inequality.

In spite of its relevance, this extension would deliver quite intuitive implications. Consider, for sake of simplicity, two populations characterized by the same level of aggregate inequality and different levels of segregation by incomes, which is here a sufficient statistics for broader socio-economic conditions. It is clear then that the first population would display a stronger pioneer consumer effect, while the second a larger mass of potential adopters, i.e. larger market size effect. From a purely theoretical perspective, all our results for technology diffusion apply to this more general case. Relevant implications would emerge, instead, by allowing the government to intervene in both the sorting process and in the determination of the income distribution. However, such analysis would lack realism: policies explicitly affecting sorting are not feasible in market economies where the house market determines the level of segregation.

Finally, the empirical evidence in support of peer effects in consumption is still scant, see footnote 4 in the paper, so this assumption would be difficult to justify. In turn, including other behavioral assumptions that are empirically observable, i.e. altruistic agents are more willing to buy green goods, would add realism without adding further insights. Indeed, binding income constraints prevent the consumption of the green good of those 'environmentalists' with low incomes.

The second comment regards the way in which we model the supply side that, we agree, is over-simplified. Including heterogeneous firm in our analysis would allow to study the joint effect of environmental and industrial policies. This extension will also be more in the spirit of recent theoretical analysis of the effect of environmental policies (*i.e.* Acemoglu et al. 2010, Fisher and Newell 2009). We found particularly interesting the possibility of including firm dynamics in a context of (truly dynamic) endogenous policy determination as the one suggested by Sapio. Such structure will allow us to analyze the crucial question of the co-evolution of technology and policy, as we believe ABM models would be the most suitable tool to analyse this interesting issue.

Moreover, considering the policy game behind the determination of environmental policies has also a strong empirical motivation. In the energy sector, for instance, there is a large and growing case study and empirical evidence showing the opposition of exiting incumbents against the approval of ambitious renewable energy policies (e.g. Jacobsson and Bergek 2004, Nilsson et al. 2004, Lauber and Mez 2004, Nicolli and Vona 2012), that stimulate innovation (Johnstone *et al.* 2010). The reason of this opposition is that renewable energy production is partially decentralized and hence destructive for the centralized model of energy production that ensures high profit to electric utility. The same argument applies for the link between the large distribution of food and the intensive, very polluting, methods of food production. A possible extension with heterogeneous firms can consider the complementarities between entry barriers and environmental policies. For instance, reducing the entry will reinforce green innovators and increases the lobbying effort in favour of ambitious environmental policies.

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Comments on the paper
"High wind penetration in an agent-based model of the electricity market: the case of Italy" by E. Guerci and A. Sapio

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The paper presents a very detailed agent-based model of the day-ahead Italian electricity market. The model accounts in particular for:

— physical components: structure of the power transmission grid, partition of the country into zones, location and capacity of each thermal and wind power plant ;

— industrial components: oligopolistic power generating companies (gencos hereafter), repartition of power plants among gencos, production technique in each power plant ;

— institutional components: feed-in tariffs for wind-generated electricity, market clearing mechanism for thermal-generated electricity (total cost minimization). In this setting, the authors focus on price formation. Each genco is a player in a game, which corresponds to one hour of operation of the electricity market. Nature first chooses electricity demand and wind- generated electricity supply. Gencos then strategically choose their bids: supply prices (or equivalently markup over the marginal production cost) for each of the thermal power plant they control. The market operator then determines zonal prices for thermal- generated electricity and gencos receive the corresponding profits. In my view, the authors make a very interesting usage of agent-based model as a bridge between game-theoretic and empirical analysis of the electricity market. On the one hand, as in the game-theoretical literature (see references within the paper), the situation is framed in terms of strategies and payoffs (mark-ups and profits respectively). On the other hand, the authors take advantage of the flexibility of the agent-based approach to describe precisely the industrial and business operations of gencos whereas the standard literature usually contents itself with the arbitrary choice of a functional form for the payoff function. The authors also aim at introducing some form of bounded rationality by letting gencos compute their strategies (mark-ups) using a genetic algorithm?[1]

(GA, hereafter) with a fixed number of iterations rather than a best response. One might however argue that GA is used here as a numerical approximation of best-response rather than as a model of actual behavior. As a matter of fact, it is not straightforward to me that the Nash equilibrium of the model could be computed exactly and that another algorithm could do better than the authors' GA in this respect. Another problem with the use of GA is that it turns the determination of pricing behavior, which is at the core of the model, into a kind of black-box . This impression is reinforced by the fact that the genetic-algorithm is re-ran every period (after the demand and wind supply have been announced) as if agents had no learning skills or memory whatsoever. It seems to me that what the authors shall in fact put forward as a model of strategic behavior is, for each genco, a mapping which associates a mark-up strategy to each demand and wind supply profiles. This mapping might be determined using a genetic algorithm, reinforcement learning or be given by a simple rule of thumb, the issue is anyway to make apparent the decision making process of gencos.

Nevertheless, when it comes to empirical relevance, the kind of agent-based model developed by the authors certainly outperforms existing game-theoretic models. Details of the calibration of the model are reported in a previous paper (Guerci and Sapio 2011, reference in the paper). The focus in the present paper is on the evaluation of the impacts on electricity price of an increase in installed wind capacity. The authors find out that as wind supply reaches its potential, electricity prices decrease but as congestion becomes more frequent (in lines that connect wind capacity zones), the price reduction effects of wind are partly offset by increased market power. The authors then point out that "the main policy implication of our results is that transmission investments in the southern zones would we worthwhile, since they would bring further electricity price reductions, to the benefit of consumers." I am not sure the model looks at the right time horizon or is comprehensive enough to make this kind of conclusion. Indeed, the results are obtained in a framework where wind- generated electricity is bought at a feed-in tariff. Feed-in tariffs first of all have a cost which is eventually bared by consumers and this cost is not represented in the model. More importantly, these tariffs shall to be in place during a

1. The description of the GA could also be made more precise.

transitory regime only. Wind-generated producers might become strategic as their market share grow, what would definitively modify the price formation mechanism. Given the uncertainty about the transformations of the electricity market induced by the growth of renewable energy sources, it might be that simple indicators like production costs remain more reliable indicators of final electricity prices. In my opinion a more actual question raised by the authors' analysis is: can additional investment in the grid be partial substitute for feed—in tariffs in the promotion of renewable energy sources ?

Reply to Comments

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In his interesting discussion, which we are grateful for, Antoine Mandel raises two important issues, concerning the way we represent learning by power generating companies, and the assumption of a stable and durable institutional setting for wind power support.

Concerning the first issue, the discussant wonders what behavioral interpretation can be adopted for the genetic algorithm. The discussant's impression is of a black-box tool for approximating Nash solutions.

Our starting premise is that gencos need high computational capacity. Indeed, our boundedly rational model focuses on the behavior of portfolio power-plants oligopolists facing complex decisions due to production and transmission constraints. This decision making process implies handling of a huge amount of information concerning network characteristics such as lines capacities that may vary daily, opponents' past strategies, fuel costs, power-plants planned outages, demand and wind supply forecast at a zonal level and so forth. Information retrieval and processing are costly both in monetary and time-consuming terms. Given the large number of interacting actors and engineering systems in the market, it is hardly the case that a globally optimal choice exists, and even if the optimum is there, even a sophisticated software may not be powerful enough to find it in a timely manner. Herbert Simon was the first to point this out: his real-world examples of procedural rationality involved the use of computers by business companies and inspired discussions of how companies solve these decision-making problems. As stated in the paper, we had the chance to talk with practitioners in more than one occasion (e.g. Italian gencos and the consulting company REF-E), and we learned that gencos and consultants do use sophisticated software to make decisions and forecasts. Further, it is worth mentioning that the existence of an optimum is not always granted even in explicitly optimizing models that represent a drastically simplified decision problem, as discussed e.g. in Hobbs *et al.* (2000) and Sapio *et al.* (2009) in the context of SFE models.

The implemented decision making process assumes that gencos handle information and make decision on an hourly basis. We can figure out that gencos learn only once the hourly mapping between the mark-up strategy and the hourly market configuration characterized by specific demand and wind supply forecasts, line capacities, gencos' planned outages, by repeatedly launching their software applications. This is the rationale why "the genetic-algorithm is re-ran every period (after the demand and wind supply have been announced [and we may say, after also other information is provided]) as if agents had no learning skills or memory whatsoever". This "extreme mapping" is a technical opportunity for a genco deliberating its optimal strategy, since they have all this information. The simulator embeds this mapping by handling all the mentioned information, having retrieved it by means of internet or institutional sources.

By the way, we agree with discussant's comment that we might have adopted a learning classifier system based on a condition-action mechanisms by restricting the way the conditioning is performed, for instance, considering only demand and wind supply forecasts and accepting gencos to be blind with respect to other factors. We might have probably obtained "to make [more] apparent the decision making process of gencos". But the way gencos operate in the market might be not so apparent to non-practitioners. Obviously, our learning algorithm based on genetic operators is just one possible way implementing a boundedly rational behavior. We could have sought to look for Nash solutions by exploring the parameter space in "extreme regions". This would have increased the computational burden, *i.e.* more rounds and a larger population of candidate solutions. Indeed, we have not performed yet such deep parameter selection based on goodness of fit. But our modeling strategy so far has been quite satisfactory in replicating the observed price dynamics, as showed in Guerci and Fontini (2011) and Guerci and Sapio (2011).

A second issue raised by the discussant is a fascinating one, as it poses a challenge not only for our future research, but for agent-based modeling in general. To be true, the institutional frame wherein the electricity market dynamics unfolds is itself a function, at least in the long term, of the diffusion of renewables and, more generally, of new technology. Wind power investments one day will not need the kind of subsidies we now observe, and gencos will revise their strategies accordingly. Similarly, predictions about the impact of gas-fired power plants on electricity prices made in the Sixties may not have foreseen the emergence of a deregulation movement enabled by the introduction of small-scale combined-cycle gas turbines. Analyzing how support measures are revised is response to market outcomes can be demanding, since it involves modeling the political process behind the design of green policies. As suggested by the discussant, investments in the grid and feed-in tariffs can be seen as substitutes, therefore in the case of wind power, what needs to be modeled is the policy-makers' way to solve the trade-off between grid investments and wind power subsidies. Exercise of this kind would create interesting links between agent-based modeling and the broader approach of coevolution between institutions and technologies (see Nelson 1994 and Von Tunzelmann 2004 for aggregate history-based theorizing, Künneke 2008 for sector-specific analyses).

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