

Comments on “Unemployment Benefits and Financial Factors in an Agent-Based Macroeconomic Model”, by Riccetti, Russo and Gallegati

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Taking advantage of the format of this journal this contribution is less a referee report and more a discussion of a few issues I found particularly interesting in the paper. I hope that the readers and the authors will excuse me for shamefully exploiting the opportunity provided by the paper to discuss issues that I consider personally more relevant, however arbitrary this choice may appear in relation to the authors' intention. Given my own personal specialization, my comments will focus on the (too rare) excellent methodological use of agent-based simulation made in the paper.

In a recent message to a mailing list for social scientists interested in agent-based simulations appeared a request for a “Exciting, simple, practical ABM paper” to be used for teaching agent-based modeling to students in social sciences. This paper would be a very good candidate for students in economics wishing to approach ABM because of its balance between simplicity of content and richness of results. Moreover, instead of being one of many technically elegant, but content-empty models that economists too frequently present in simulation exercises, the authors manage the even rarer accomplishment of providing a very pragmatic paper with a clear and robust contribution to an issue of high relevance both in the theoretical debate and in the current policy debate. I will start my comments by firstly discuss the methodology adopted and then moving to comment on the economic content of the paper.

Many economists are convinced that the advantage of agent-based simulations lies in the possibility to provide detailed and realistic representations of real systems without the constraints imposed by the necessity to solve analytically the model. As a consequence, many of these papers try to imitate as many details as possible of the targeted systems, aiming at validating the model according to a measure of distance between simulated data and to available empirical evidence. This approach is fundamentally wrong. A model is necessarily simpler than the reality it represents, and the less distracting details are present in the model the easier is its use. The problem, of course, is to correctly distinguish irrelevant details from necessary components. As geographical maps, what is relevant and redundant is not objectively given, but depends on each specific application

demanding a specific choice. In any case, the criterion “as close to reality as possible” is not a sensible one: the most detailed possible map formed overlapping all the others is surely “closer” to reality than any other one, but also totally useless.

A model, like a map, should not be assessed by its absolute similitude to reality, but starting from the goal it is built for, and its distance from reality can be adequately measure only considering the goal it is meant to pursue. In this respect, the paper is crystal clear. The goal the authors pose themselves can be summarized as follows: do unemployment benefits help or hinder a country macro-economic conditions? The question is part of an old, and never fully settled, debate among economists. Those closer to the interventionist tradition praise the automatic stabilizing effects of unemployed benefits, able to trigger counter-cyclical policies without the need of explicit decisions, or even to be aware of the need intervention. On the contrary, the current mainstream school of thought criticizes, from a micro perspective, the effects of the benefits on the individuals’ incentive to work, and, on the macro side, the effects on public debt and the inflationary consequences of excessive government spending. This debate dates from the counter-revolution that debased the Keynesian orthodoxy unable to account for the contradictory (from a narrow Keynesian perspective) condition of high inflation and lack of growth. After three decades of policies based on the Chicago orthodoxy, that is, that small government and low inflation push growth by means of low interest rates, the debate gets a new life because (again...) of an apparently inexplicable crisis. The currently dominant economic consensus would solve any problem by reducing government debt and loosening monetary policy to stimulate growth via lower interest rates. However, interest rates are at all time lows, hitting the floor represented by free money, and the cuts in spending have been shown to further depress the economic landscape, obtaining the pervert result of worsening, rather than improving, the ratio of debt over GDP. Hence, the insurgency of the few Keynesian thinkers left in the profession against those P.Krugman calls “deficit scolds”, which re-ignited the debate on the role of government. However implicitly, this paper is clearly a contribution to this debate with an un-conventional approach and unusually open-minded and non-partisan policy implications.

The debate is politically hot and conducted with almost religious fervor, leaving little room for a logical argumentation, besides the listing of carefully selected historical cases vaguely supporting one’s argument and against the opponents’. The problem is that no empirical evidence will ever settle a debate like this, since any historical record is, by definition, a special case because of the huge and increasing diversity of economic systems across geopolitical and historical contexts. The only, narrow, path to provide fresh contribution is to investigate how a given policy may affect specific aspects of a system. That is, a purely speculative analysis that under strict ‘ceteris paribus’ assumptions, describes step by step the chain of events that may take place as consequence of a policy initiative.

Agent-based models are an ideal instrument for this purpose, since they are the only methodological tool able to generate a truly dynamic pattern to be compared with the sequence of events of reality, or of alternative thought experiments. Though the paper fails to take full advantage of this feature (more on this below), it still manages to build a very strong support for a novel and convincing result. The question tackled by the authors can be summarized as follows: do unemployment benefits discourage work and produce an unsustainable burden for public finances? Or do they produce a stabilizing effect countering the vagaries aggregate demand? The answer, as frequently happens, is: it depends. Among the many factors likely to influence the results the authors focus on the level of the benefits. They show that increasing the level of benefits starting from very low levels reduces volatility at the cost of only a moderate increase in inflation and public debt. Moreover, the increased amount of government backed securities necessary to finance the increased spending acts like an increase of liquidity in the financial system, so that the benefits generate automatic stabilizing effect not only for fiscal, but also for monetary policy.

According to the paper results, the positive effects of unemployment benefits are, however, non monotonic. The authors sustain that, in effect, excessively high benefits do push real wages beyond a threshold after which a different, pervert, mechanism is activated. In fact, too high wages, pushed by the low opportunity costs of unemployment, would force down profits and, by increasing the fragility of the productive sector to defaults, would generate higher volatility and, ultimately, a less productive system.

In a sense, the results presented in the paper vindicate both sides of the debate. On the one hand, unemployment benefits are shown to be a policy instrument justified by economic efficiency reasons, besides the generic justification based on social fairness. On the other hand, it is also true that an excess in the dosing turns the medicine into poison, since too high benefits drain resources that cannot be put to productive use, inducing a recessive and inflationary spiral. Not only these conclusions appear sensible, but they also are supported by a formal method that, at least in principle, could be assessed both in general and for specific cases, besides extended to encompass additional elements not included in the present version of the model.

Without entering in the details of the model structure, it is worth to make some critical comments on a few modeling choices adopted by the authors. Firstly, the authors fail to provide a complete account of their model. Though the text is fairly accurate in describing the economically relevant content of the model, the implementation is likely to include some additional details not presented in the paper because too technical to deserve space in the main body of the paper. However, they may possibly be highly relevant for the reproducibility of the results. It would be useful to have in appendix, even only on request, the very code used for the model; however rare is the case of simulation modelers reproducing others' results, it should not be precluded by not sharing the whole content

of the program implementing the simulation.

As said, the model is purposefully simple, with few actors only whose decisions are represented by straightforward algorithm. This is at the base of the strength of the paper, since it makes possible to not only generate, but also interpret the results. And it is also a difficult task that deserves to be praised. In particular, it is worth to highlight the simplicity and elegance of the market-matching mechanism adopted, able to replicate both the variety required by heterogeneous bounded rational agents and the requirement that more attractive options get higher rewards than lesser peers.

There are however two modeling choices that are not only questionable (as any one, after all) but particularly harmful, and un-necessary so, in the context of the model. The first is the choice of using a single universal time clock for each and every dynamics of the model. That is, the time span used, e.g., by firms to change their price is the same time required to decide how much to produce, which is also the same frequency used by consumers to decide their consumption level, change bank account, for the central bank to decide interest rate, etc. One of the core features of real world systems is that different decisions have different time horizons, and the irreversibility of past decisions is a major constraint looming large over any decision maker. The same criticisms brought against the use of representative agents in favor of heterogeneous ones apply also, and possibly more, to the timing of decisions. An entire population whose members move all at the same synchronized beat is closer to a parading marching army, while the loosely-coordinated, diverse decision makers assumed by proponents of agent-based models is far better represented by a differentiated time scaling, as well as differentiated decisions.

Similarly, the fixing of the number of firms (and banks) so that new entrants appear only to replace defaulted incumbents is an unjustified modeling choice. Nothing would prevent modelers to allow the number of firms to increase and decrease along an endogenous dynamics. It needs to be said that both these assumptions are widely diffused among simulation modelers. Their only justification lies in the technical difficulty caused by having differentiated time clocks and a dynamic, rather than static, allocation of data structures. While this may have been a sufficient justification at the dawn of economic simulations, this is no more so now with the improvements of both programming tools and expertise available today.

The problem for those two assumptions is that not only they impose un-necessary and un-realistic constraints, but it may be sustained that the model would greatly gain in detail and power of representation by removing both those arbitrary assumptions. In fact, a varying number of actors (e.g. firms) and diversity of decisional timings is more likely to generate endogenously “slow” cycles, rather than the very sketchy volatility presented in the paper. Though we sustained that similitude to reality should not be the primary concern of a model, a richer range of real features produced in the results is an obvious advantage that is worth to pursue, particularly when it does not add too much in terms

of additional complexity.

Concluding, though the paper may improve by a little bit of editing, my opinion is that it deserves to be praised as an innovative and convincing piece of research. It both delivers a sensible contribution to a highly relevant issue in the current policy debate, and provides a convincing and solid starting point for the development of a promising research project.