

Comments on the paper

"Production Process Heterogeneity, Time to Build, and Macroeconomic Performance"

by M. Amendola, J. L. Gaffard and F. Saraceno

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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

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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.