Optimal Inflation Target: Insights from an Agent-Based Model
Answer to the referee comments

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I. GENERAL COMMENTS

We thank all the referees and commenters for their useful remarks. Prompted by their questions, we performed additional numerical simulations of the model, that led us to a better understanding of its qualitative behavior. As a consequence, the paper has been extensively revised and we belived that the new version has been greatly improved with respect to the initial submission.

In particular:

1. We found that in some region of parameter space, mainly controlled by the parameters R and ρ^*, the economy displays coexistence between several equilibrium states. This coexistence region was overlooked in the first version of this paper, and explains some of the strange effects that we initially reported. In the revised manuscript, we discuss the coexistence, but then we focus on a region of (R, ρ^*) in which there is no coexistence, which simplifies considerably the discussion.

2. We added a new "methodological" section where we explain our philosophy. We hope that this will clarify the approach we use in this work, addressing in particular the issue of the existence of a "welfare function".

In the following, we start by answering our three referees, and then turn to the invited reader assessments and the anonymous comments.

II. REFEREE REPORT 1

We thank the referee for carefully reading our paper and providing very interesting comments. Here are some answers:

1. It seems to us that here there are two logically independent questions: one is what should be the optimal inflation target, the second is what happens if the CB decides to change this target. For the first question, the assumption of a constant τ (i.e. how agents react to realized vs. target inflation) seems appropriate. For the second question, clearly dynamical adaptation might be a crucial effect. In our paper however we are mostly concerned with the first question. We agree that the second question is also important, but it is more difficult to address, so we believe that it is better to start understanding the phase diagram of the model with constant τ. Adding the dynamical adaptation of τ is of course a very important direction for future work. We added some remarks in the paper about what one could expect in a situation where the τ depend on ϕ_π and also on time, or on the difference $\pi^\star - \pi(t)$.

2. The referee is right that calling $\rho^\star$ the natural interest rate is misleading. In DSGE models, one assumes that there is a single equilibrium state of the economy, which fixes the natural interest rate. In our setting, the macroeconomic state is an emergent concept that derives from the microscopic rules. In this sense, $\rho^\star$ is just a free parameter of the model, and it has no "natural" value. We stressed this in the revision of the paper, just after Eq.(4).

3. We fully agree and this is related to the previous point. We revised the abstract.

4. We do not impose a zero lower bound on the CB interest rate in our model, but rather discuss the probability that the rate becomes negative. A zero lower bound could easily be imposed instead, but in view of the recent episode of negative interest rates after the 2008 crisis, we chose to model the CB policy without ZLB. The final results would not be drastically modified if we imposed a ZLB. We added a remark after Eq.(4).

5. For this point, see our reply to the invited reader comment 1, point B, and the new "Methodology" section. We choose not to have a precise measure of optimality, as we believe that this is a more pragmatic (although heterodox) approach. Our aim is to understand the macro behavior of the economy as a function of the free parameters, to provide a "dashboard", under the assumption that decision-makers will use this to decide about optimality. In the paper, we take low unemployment and moderate inflation (close to target), as a qualitative measure of the quality of the economy.

III. REFEREE REPORT 2

We thank the referee for carefully reading our paper and providing very interesting comments. Here are some answers:

1. We fully agree with the referee that Eq.11 (Eq.10 in the older manuscript) should read, more generally

$$\Gamma = \max\left\{\alpha_\Gamma(\rho^{\ell,\mathrm{ema}}(t) - \widehat{\pi}(t)), \Gamma_0\right\} \tag{1}$$

   in such a way than when the real interest rate is smaller than Γ_0/α_Γ, firms do not react to a change in the interest rate. However, this point has been already discussed in our previous paper [*Monetary Policy and Dark Corners in a stylized Agent-Based Model*, Journal of Economic Interaction and Coordination 12, 507 (2017)], see Eq.(13) of that paper and the discussion below it. We had checked at the time that the precise value of Γ_0 (within a reasonable range) did not affect qualitatively the behavior of the model.

2. We agree that the absolute values of interest rates and inflation rates are not directly meaningful, because the model has not been calibrated (in particular the elementary time scale, here chosen to be 3 months). However it is quite clear e.g. from our Fig. 4 that a symptom of a too low target is under-realisation of inflation. In other words, when $\pi < \pi^\star$ it is likely that $\pi^\star$ is too low. When the economy is doing well, we always find π slighly larger than $\pi^\star$. So, one can conclude that the value at which $\pi = \pi^\star$ sets a "lower limit" above which the target $\pi^\star$ is beneficial to the economy. The "upper limit" for $\pi^\star$ should then be dictated by the cost of inflation, which, we agree, is not taken into account by our model. Unfortunately, these inflation costs are hard to estimate; the DSGE assumption that these costs are dictated by the non-synchronicity of price updates looks to us unconvincing. As we explain in our new "Methodological" section, we still believe that the qualitative results provided by our model can be useful, and generate fruitful discussions.

3. Unfortunately, we do not yet have a full intuitive understanding of the mechanisms behind some of the non-trivial outcomes of the model. What we described in the original manuscript was our best current understanding, but we also added a new section V.C, "Economic interpretation", to the revised manuscript, where we provide additional intuition on the behavior of the model. Note also that the phase diagram has been considerably simplified compared to the first version of our manuscript, thanks to a proper identification of a bi-stable region. We refer to the other answers below and to the new "Methodology" section for more details.

Minor points:

- We added the specification that the increase of Y_i is bounded by the available workforce, in such a way that $\sum_i Y_i \leq N$. We also specified that the banking sector is described at an aggregate level and makes zero profit at each time step.
- We added a reference to Arifovic et al. (2010)
- The motivations for Equations 12 and 13 (previously 11 and 12) have been discussed in previous papers about the Mark 1 model (by Delli Gatti et al.) and about the Mark 0 model (by ourselves).
- We now consider several values of ϕ_π in Figs. 3 and 4, that cover all the range where no strong instabilities occur.
- We find very useful to provide some comparison with the benchmark DSGE model. This is a recurrent question that we are confronted with each time we present our results.
- We removed the statement that "the equilibrium state of the DSGE economy cannot be characterized" (by which we meant that DSGE models usually describe fluctuations around a given equilibrium, while in our model the macroecomic state is emergent from the micro-rules).
- We have removed all "theological" references.

IV. REFEREE REPORT 3

We thank the referee for carefully reading our paper and providing useful comments. We agree in particular that the *main strength of the paper is the methodological approach concerning the identification of the two native states of the economy*. Here are some answers to his/her queries:

1. While we agree that the "original" Taylor rule is the one of Taylor himself, it is now customary to write it the way we did, following for example the DSGE literature (see e.g. Gali's book, Chapter 3, Eq. (25)). In any case, the two ways of writing the Taylor rule just amount to a redefinition of the parameters $\rho^\star$ and ϕ_π, as we now make explicit in a footnote. Apart from a slight change of interpretation induced by these redefinitions, none of our results are affected by such a change.
2. The setting of ρ^d is indeed not detailed in the core of our paper, this to keep the presentation as simple as possible. It is spelled out and discussed in our previous paper [*Monetary Policy and Dark Corners in a stylized Agent-Based Model*, Journal of Economic Interaction and Coordination 12, 507 (2017)], which may be inconvenient, but it is also explicit in the pseudo-code given in the Appendix, so that the present paper is indeed self-contained as is.
3. The price and wage setting mechanism is indeed, to some extend, ad hoc. However, as explained in the paper and in the various replies to comments, we prefer at this stage to stick with the now well established Mark 0 and Mark 1 models, and use these as benchmarks to study monetary policy experiments. As we emphasized in our "Tipping point" paper, and again in the introduction of the present paper, we do not believe that time is yet ripe to engage in a fully realistic Agent Based Model, but rather to establish a series of methodological points. Still, as carefully explained in "Tipping point", we have found that many of these micro-rules can be substantially modified without affecting the qualitative conclusions, i.e. the emergent states in which the economy can land. The question of empirically-founded rules is of course crucial, and one of the main argument against Agent Based modelling. We think that this is a difficult question that will only be answered progressively, but this should not discourage us from trying to explore the general properties of macro-economic ABMs. This is the aim that we currently pursue.

4. The referee is totally right. We used "investment" inappropriately to mean investment in human capital. Real investment and its impact on productivity is sorely missing from Mark 0, and, as stated in the paper, this is one avenue we definitely want to explore in the near future. However, as we just emphasized, the aim of the present paper is more of a "proof of concept": using only labor and consumption as buiding blocks, in what macrostate can the economy end up and how far can we understand the role of inflation? We certainly share the referee's frustration that many interesting aspects of the economy are not included in Mark 0, but, again our modelling strategy is to progress brick by brick.

Minor points:

1. We have clarified the definition of the financial fragility Φ. If negative, firms are healthy. If strongly negative, firms are flush with cash and willing to increase wages faster.

2. The notation $X^{\rm ema}$ is now explained after Eq. (2). There is indeed a difference between ρ^d (instantaneous) and $\rho^{d,{\rm ema}}$ (time averaged).

3. Price/wage setting are sources of randomness, as is the consumers' choice of the firm from which they want to buy. Changing the amplitude of the noises has no influences on the qualitative features of our economy. Of course, the precise location of the phase boundaries, as well as the value of inflation and unemployment, depend on all the parameters of the model.

4. This is a very good remark; we have now changed the definition of the model to be consistent with this.

5. Physicists would rather call them "phases". We prefer the word "state" to "regime" as the latter suggests that there might be some temporal evolution. We believe that the expression "state of the economy" suits well what we want to describe.

6. A complete list of all the parameters of the model, with their actual values, was already given at the beginning of the pseudo-code in Appendix A, but the referee is right that a table is useful. We added Table I with the list of all parameters that are kept fixed, and a new section III.D where we discuss the remaining parameters that are varied in this work.

V. INVITED ASSESSMENT 1

We thank the referee for carefully reading our paper and providing very interesting comments. Here are some answers:

1. We thank the referee for pointing out the paper by De Grauwe, which is indeed relevant and we added a citation in the discussion section. We will certainly investigate the mechanism introduced in that paper in future investigations.

2. We agree that the behavioral rules are somehow rigid and that the 100% unemployment phase is obviously not realistic. In our mind, the behavioral rules are relatively reasonable in the good phase of the economy, and they correctly describe how such phase can become unstable. However, once the instability happens and the economy collapses, of course it is unreasonable to expect that agents will keep acting according to the same rules, in particular the Central Bank and other institutions. In a world with a deeply dysfunctional economy, and almost full unemployment, agents would certainly adapt very different strategies. This is why, indeed, the bad phase of the economy cannot be interpreted at face value. We added a remark on this point in the new "Methodology" section.

3. The good phase of the economy in our model is characterized by average negative profits and, correspondingly, negative interest rates. This is a consequence of both the absence of productivity growth and the presence of waste of production. Call $S(t)$ the savings at time t. We have $S(t+1) - S(t) = W(t) + \rho_d(t)S(t) - p(t)C(t)$, where $W(t) = w(t)Y(t)$ is the total wage, $\rho_d(t)$ the interest on deposits, $p(t)$ the average price and $C(t)$ the total consumption. Hence in stationary state, $S(t+1) = S(t)$, we have

$$p(t)C(t) - w(t)Y(t) = -\rho_d(t)S(t) \ . \tag{2}$$

The left hand side of the last equation is precisely the average firm profits. In presence of full market clearing, one would have $Y(t) = C(t)$ and $p(t) = w(t)$ (note that productivity is equal to 1), so the average profit would be zero and so would be the average interest rate. Instead, in our economy, markets do not clear. One always have

$C(t)$ less or equal to $Y(t)$, as a result of the fact that sometimes agents do not find enough products in the firm they have chosen, leading to involuntary saving. Since our manufactured products cannot be stored, firm profits are on average negative, and so is the average interest rate. Although we have not find such a specific discussion in the literature, we believe that the above would be true for any closed economy with constant productivity, non storable products and involuntary savings. In the presence of a positive growth rate, the stationary interest rate would indeed be positive. We now have added a comment on how to account for a constant growth rate of the economy within our simplified framework.

4. The commentator is right that it is somehow unavoidable that complex ABMs can lead to non-intuitive results, in particular concerning the shape of the phase diagrams, such as those in Fig.2 and 4(a). This is in fact one of the strength of ABMs: to be able to elicit scenarios that are hard to imagine for the unaided human mind, because they are the result of non-linearities and sometimes antagonist feedback loops. We added a remark on this point in the new "Methodology" section. Note in particular that the phase diagram has been considerably simplified compared to the first version of our manuscript, thanks to a proper identification of a bi-stable region. We have also added a new section V.C where we provide additional explaination on the behavior of the model.

5. We produced plots of the Phillips curve and added them to the paper in a new section. We find that in the model, unemployment is a decreasing function of increasing inflation, which is consistent with the Phillips curve. However, the correlation is small when the parameters of the model are fixed in time; it becomes appreciable when the parameters of the economy are themselves time dependent.

6. In our model all firms produce the same good, but sell it at different price, so we do not understand the remark on the Dixit-Stiglitz mechanism. Would it be possible for the author of these comments to clarify this point?

7. A discussion of the transmission mechanisms in our model is contained in the conclusions of the paper, where a comparison with DSGE models is also provided.

8. Concerning intuition, this is, as emphasized above, a general problem of ABMs. Again, we believe that this is an important and realistic feature of these models: indeed, reality is not always understandable in terms of simple rules of thumb. However, please see the new section V.C for additional elements of intuition on the model.

Minor comments:

- We prefer to keep ρ for consistency with our earlier papers.
- We made the acronyms explicit in the subsection headings, but for compactness we prefer to use them in the captions.
- We used larger axis and explicit labels in the figures.
- We prefer to keep "we" in the abstract, it seems to be a quite standard use in the literature.

VI. INVITED READER COMMENT 1

We thank the reader for carefully reading our paper and providing very interesting comments. Here are some answers:

(i) The quantity $S(t)$ is indeed the total savings at time t. The update equation was missing (we made reference to our previous paper) and we added it as Eq.(6) (see also point 2 of the previous reply where this equation is given).

(ii) Our rules are such that firms have to change the quantity they produce, $Y(t)$, at each step. Price is instead only changed in some situations, according to Eq. (12) of the revised manuscript. We agree that this is somehow arbitrary, but the same is true for many other rules in agent-based models. The hope is that these details do not matter too much for the qualitative behaviour of aggregate quantities, as we discuss in detail in our previous paper "Tipping points in Macroeconomic Agent Based Models". We added a new "Methodology" section to discuss this point more explicitly.

A) We agree that investments are an important ingredient that is missing. But this would require a substantial extension and complexification of the model, including capital, financial markets, etc. We are currently working in this direction but we do not yet have enough results to discuss these points in the current paper.

B) We agree that there is no utility function that is optimized in our model. Yet, there is a (somehow arbitrary) notion of a "good economy", i.e. one with low unemployment, moderate inflation, etc. It seems to us that this loose notion makes more sense than the optimisation of an even more arbitrary utility function. As Keynes said, it is better to be roughly right than exactly wrong. We added a paragraph in the new "Methodology" section to discuss this point.

VII. ANONYMOUS COMMENT 1

We thank the commentator for his/her useful remarks and for pointing out some relevant references that we added in the introduction of the paper. Here follow some answers to the comments:

- We agree that the suggested extensions are interesting and we will certainly do work in that direction. As suggested, we discussed a bit more in the manuscript the impact of the value of $\tau^\star$ (now called τ^{T}, "T" for target) in the revision of the paper. In our study, we set $\tau^{\mathrm{R}} = \tau^{\mathrm{T}} = \frac{1}{2}$. The results when agents fully trust the CB policy ($\tau^{\mathrm{R}} = 0$, $\tau^{\mathrm{T}} = 1$) are not radically different, although, as expected, realised inflation is closer to target. In the other extreme case ($\tau^{\mathrm{R}} = 1$, $\tau^{\mathrm{T}} = 0$), the economy is fraught with instabilities. A more detailed study of the phase diagram as a function of $\tau^{\mathrm{R}}, \tau^{\mathrm{T}}$ is left for future work, as is the extension using the mechanism proposed by De Grauwe in his paper.

- As already written in response to the comments of the previous referees, we do not have complete intuitive understanding of the behavior of the model (a feature that the ABMs share with the true economy!). We explained in the conclusion section what we understand, and unfortunately we do not have much to add to this discussion. To answer the last question: in the disfunctional economy phase fragile firms do not ask for credit but prefer firing as soon as demand drops. These firms therefore they do not react to interest rates and higher inflation. Hence, once the model goes into the collapsed phase, monetary policy becomes ineffective. This is discussed in more detail in our previous paper "Monetary Policy and Dark Corners in a stylized ABM". Note that, as stated in the new "Methodology" section, we do not expect our micro-rules to be reasonable in a fully collapsed economy, where agents would probably react in a very different way.

- Terminology: we agree with these remarks, some of which have been already stressed above. We changed these terminologies in the revision of the paper. In the revised manuscript, $\rho^\star$ is called the "baseline" CB interest rate, we stress (in the methodology section) that we do not define a welfare function to be optimised, and we clarify that in the native state the CB is inactive, but still fixes $\rho^\star$, while agents expectations are not affected by the (non-existent) CB target as τ^{T} (the old $\tau^\star$) is zero.

- Validation: our aim here is to study the properties of the model in a whole range of parameters, even if some regions are clearly absurd. We believe that such studies help understanding the model through a detailed investigation of the phase diagram, and of the dependencies of the different observables on the parameters. We restrain at this stage from making precise calibrations and validations of the model as we know that some potentially important features have been left out of the model. This is stressed in the new "Methodology" section.

Minor comments:

- When a firm defaults, it is replaced by a newly initialised firm. The details are given in the pseudo-code of Appendix A (under "Revivals"). This is also detailed in our previous papers on Mark 0.

- We would like to keep the manuscript as compact as possible, and since the determination of ρ^d and ρ^ℓ is exactly the same as in (Gualdi et al, 2015), we just made reference to that paper and to the pseudo-code of Appendix A, where all relevant information is given.

- We increased the size of all symbols in the figures.

- We replaced τ^{ema} by τ^{R} (for "realised" inflation) and $\tau^\star$ by τ^{T} (for "target").

VIII. ANONYMOUS COMMENT 2

We thank the commenter for his/her useful remarks. Here follow some answers to the comments:

- References [2,3] already provide an history of the 2% target. We gave references to these.
- We give a short description of the building blocks of the model in section III. Details can be found in our previous papers, that are cited in the present one. The pseudo-code of Appendix A gives all the information needed to understand and/or recode the model.
- We agree in principle, but the presentation order is fixed by the fact that to explain firms' behavior we need some quantities that are defined above. We do not see a simple way of fixing this.
- Some intuition is provided in the conclusions, but as already written above as reply to other commentators, we do not necessarily have a clear intuition to explain the rather complex behavior of the model. This is discussed in the new "Methodology" section.
- We fully agree and we apologize for this: there is indeed no investment in the model, and investment will be replaced by saving everywhere.
- By "in spirit" we mean that the equations are not strictly identical, but they are qualitatively similar. In both cases, consumption increases when the real interest rate decreases and vice-versa. We slightly rephrased the original sentence.
- This is an important point. We modified the model in such a way that in the "native" state τ^{T} (the old $\tau^\star$) is zero, in such a way that the inflation target $\pi^\star$ becomes completely irrelevant in that case. We thank the referee for pointing out this problem.
- In Eq. 6, we are not sure to understand the query of the commentator. Our equation means that the expected inflation is a linear combination of the target inflation and the past realized inflation, with coefficients that do not necessarily sum to unity. (A sum larger than 1 may describe over-reaction).
- We agree and we clarified this issue by fixing $\tau^{\mathrm{T}} = 0$ in the native state, in such a way that now $\pi^\star$ is irrelevant, and the only control parameter is $\rho^\star$.
- We agree that the precise value of 2% has no special meaning in our model, since we did not try to calibrate the elementary time scale of the model. This was already stressed in the paper, but we stressed it even more (see the new "Methodology" section).