

# Authors' Response to Referee Report 2

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We would like to thank the Referee for a thorough and constructive review. We are providing detailed responses to individual comments below.

## Summary

This paper provides a method to compute optimal interest rate rules in a dynamic general equilibrium model subject to a zero lower bound (ZLB) constraint. The contribution is methodological and, in my view, potentially significant.

The analysis is competently executed and the paper is generally well written. The paper needs a better collocation within the economic literature and to be more focused on its main contribution.

## Comments

1. Introduction: when describing the two alternative approaches to determining optimal rules under the ZLB, the main contributions in both strands of the literature should be cited.

We are not certain that we are interpreting this comment correctly. Nevertheless, there are three citations for the first strand and five for the second strand discussed in the Introduction. If the Referee's suggestion is to elaborate on each of the two strands we can certainly do that; however, we thought that the key points and citations provided cover the essence of the two basic approaches mentioned.

2. In the Introduction the approach taken in the paper should be related to the literature comparing Commitment (time inconsistent solution) versus discretion (time consistent solution) in economics. A good starting point to organize this discussion is Woodford (1999).

This is indeed an excellent point, as is the reference suggested (also cited in the paper).

The approach presented in the paper assumes a commitment policy. However, the same approach (namely parametric programming solution of constrained real-time optimization) could be applied to a discretion policy. What would be different between the two cases is the model describing the effect of interest rate adjustments on output gap and inflation. More specifically, expectations would be different depending on whether such expectations are based on anticipation of commitment or discretionary policies from the central bank. We will add this clarification to the revised manuscript and point out possibilities for future research.

3. Section 2: please provide more details concerning the simple model you spell out at the beginning of the section (equations 2 and 3). Is it the baseline New Keynesian model that you have in mind?

The model we used is along the lines of the discussion presented in references such as Michael Woodford's *Interest and Prices* (2003) chapter 4. The specific model we used is taken from L. Ball, "Efficient Rules for Monetary Policy," *International Finance*, vol. 2, pp. 63–83, 1999, as cited in the manuscript.

As a general comment, using a simple model such as the one we (and many others in literature) have used is clearly a choice justified on the need to be able to draw analytical conclusions and insight. This is a clear case of George Box's dictum "all models are wrong; some are useful". Clearly, far more elaborate models are available that can provide – at least in principle – more accurate predictions. However, such models are difficult to manage and would require a different approach in order to provide insight. (Such an approach would have to uncover reliable patterns that would result from extensive numerical work – a proposition that we believe does have merit, but is out of the scope of the work presented in our paper.)

4. Section 2.2: you first say that the model summarized by equations 2 and 3 is calibrated, then that it is estimated using a prediction error method. Please clarify.

What we mean is that least-squares calibration of the model is based on prediction error method. A corresponding clarification will be made in the manuscript.

5. Matrix A describing the evolution of the system has an eigenvalue larger than one. It is not fully clear to me whether instability of the system is necessary to apply the MPC optimization. Specifically, the reformulation of the problem in the form specified in the objective described in (11) requires instability of the original problem?

Let us clarify: MPC does not require instability of the original problem; in fact, MPC would prefer stability of the original system. However, the model we used (taken from standard literature) naturally includes this instability (eigenvalue at 1). Therefore, the feedback strategy used must be designed in such a way that stability is ensured.

6. You specify that minimizing (10) is equivalent to minimizing (11). Does this mean that the maximum level of welfare obtainable is independent of S and R in (11)?

Admittedly, the transition from eqn. (10) to eqn. (11) can be explained better.

Here is a better explanation: The form of the L term in eqn. (10) is quite flexible. A common form is quadratic, as indicated in eqn. (11). In addition, the infinite summation in eqn. (10) can be replaced by a finite summation plus a terminal cost, as indicated in eqn. (11). The advantage of eqn. (11) is that it is easier to manage, because of the finite number of terms.

7. Figure 2 is not very explicative. I would drop it and refer to figures 3 and 4 in the analysis.

We will follow the Referee's suggestion to remove Fig. 2 from the manuscript and rely on Figs. 3 and 4.

8. I do not find of great interest discussing which values of  $\lambda$  and R replicate the original coefficients of the interest rate rule chosen by Taylor. I would limit the discussion to the analysis of combinations of  $\lambda$  and R that lead to a unique stable equilibrium.

We thought that indicating what values of  $\lambda$  and R replicate the original Taylor rule would be of interest, since it would provide a good reality check for the proposed approach, and illustrate the fact that our approach extends rather than replaces the original Taylor rule. Therefore we are reluctant to remove it. However, in the spirit of the Referee's suggestion, we will remove most of the technicalities on the subject into an appendix, leaving the key finding (replication of Taylor's rule within our framework) in the main text.

Of course, we certainly agree with the Referee's assertion of the importance of stability.

9. Section 3.3: In this section, you show that reformulating the MPC problem including a term which penalizes the rate of change of the interest rate results in a Taylor rule where inertia is optimal. Woodford (1999) points out that including a smoothing motive in the welfare loss function of the CB would lead to an interest rate rule characterized by inertia. While this reverse-engineering exercise is interesting, including a term which penalizes interest rate changes in the MPC problem is not supported on welfare grounds. In other words, the welfare function you are maximizing would not be a correct second order approximation to the utility of the representative agent of the economy you are considering. For this reason I would remove section 3.3. In my view the aim of the paper should be that of providing a method to compute optimal interest rate rules under a ZLB constraint. The optimality of inertial, which is subject to the above critique, can be neglected in this contribution.

This, again, is an excellent point raised by the Referee. Indeed, an approximation of the welfare function would lead to a purely quadratic objective function. The Referee is also correct in pointing out that the issue of inertia is not central in our paper (ZLB is). However, we have two reasons for believing that the

discussion on inertia, albeit not central, may still be of considerable interest and can be retained in the paper:

- a. It appears that the US Fed does take rate-of-change issues into account when adjusting rates. This has been pointed out in references we cite and is also the outcome of our own calculations. The naïve justification that the Fed would like to avoid overly large shocks in rate changes is intuitive plausible, although, of course such justification is in no way rigorous or conclusive.
- b. We wanted to make the case that the proposed approach (parametric programming and MPC) is also applicable to cases more complicated than the standard quadratic-objective MPC case (which naturally leads to the standard Taylor rule). In fact, even more elaborate cases (objectives, models, constraints) can be considered, the key being what insight (as opposed to mere numbers) such cases can offer.

10. Is the method proposed in the paper applicable to larger (medium-scale) models? A discussion of this issue would be useful.

In principle, yes, although combinatorial complexity (combinations of active and inactive constraints and corresponding polytopes) would quickly become a non-trivial issue as model size would increase. What would help the analysis is the fact that the ZLB is a fairly simple constraint, and careful analysis would probably simplify the combinatorial complexity that would naturally emerge with larger models. We believe that this would be an excellent topic for future study, and want to thank the Referee once more for his insightful comment.

#### References

Woodford M. (1999), Optimal Monetary Policy inertia. Unpublished manuscript available at the web page of the author.