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Håkon Tretvoll

Statistics Norway

"**Welfare and Spending Effects of Consumption Stimulus Policies**" (QE Manuscript #2442-1).

Dear Håkon:

Thank you for submitting your manuscript to **Quantitative Economics**. I have now heard back from three referees, I have discussed your paper with an associate editor and with another editor, and on this basis, I have decided to invite your paper for a revision.

The three referees are all quite positive although they do have quite a lot of issues with the paper. I also like your paper, but I have somewhat more serious issues than the referees and my concerns are shared by the other members of the editorial board. It will take some work to address these concerns. Nonetheless, I wish to give you the chance to revise the paper because it contains some very interesting work on an important issue.

Let me summarize what I see as the main issues.

1. **Motivation**: I think the paper needs a stronger motivation in the sense that it is a bit unclear why I need to introduce splurge consumption and other features of your model. In particular, incomplete markets/HANK models with liquid and illiquid assets, and/or models with spreads between borrowing ans savings rates, can potentially account for both MPCs and for the relevant features of the (liquid) wealth distribution. So why go the way you choose in the paper? Do you have empirical evidence in favor of this alternative model or are there other reasons for exploring this?

Let me quote from the feedback I got from another editorial board member:

"Perhaps the authors want to argue that a HA model + fully constrained agents cannot match their data targets, so that one needs something else, and splurge consumption is a natural solution that also seems intuitive. But then they need to make this argument explicit: they need to compare a version of their model with a fraction of fully constrained agents (which would be the natural first place to go) to their benchmark model and argue how the data rejects this alternative model. They should also discuss why other alternatives to matching iMPCs that have been introduced in the literature are not good enough for what they want to do."

2. **Splurge consumption**: A key aspect of your paper is the introduction of splurge consumption. I find this an interesting idea. However, I have issues with your modeling on page 8 contained in equations (1)-(4). Here you assume that splurge consumption is a constant fraction of income which enters the budget con-

straint, but does not impact on the marginal utility of consumers' optimal choice of consumption. The latter is crucial since a moderate level of splurge otherwise would have no or little impact on the economy. In the other extreme, suppose you had assumed that consumers get utility from splurge goods in exactly the same way as $c_{opt,i,t}$ so preferences are given as $\sum_{t=0}^{\infty} \beta_i^t (1-D)^t \mathbb{E}_0 u(c_{opt,i,t} + c_{sp,i,t})$, which would seem a natural starting point. In this case, splurge would have no effects on total consumption unless $c_{opt,i,t} < 0$ (which you could rule out, I guess).

You present your assumptions without defending them, but I think you need to have a convincing story about this as it otherwise looks arbitrary. Your simplest defense, of course would be that preferences are given as $\sum_{t=0}^{\infty} \beta_i^t (1-D)^t \mathbb{E}_0 (u_1(c_{opt,i,t}) + u_2(c_{sp,i,t}))$, but in this case you would need to have a good "story" about what type of goods these splurge goods are and why you can treat preferences this way.

Moreover, as also pointed out by Referee 1, your current set-up is equivalent to a model in which there is a constant average tax rate, ξ, but you then count tax payments as consumption. Again, this seems inconsistent. Referee 2 points out that your assumptions alternatively can be thought of as each household having some buffer stock members and some hand-to-mouth members. This also seems arbitrary and it is hard to accept that these different branches of the family cannot insure amongst themselves.

In a footnote you mention that splurge might be close to rational in a model with small durables. However, this would seem to me to contradict your calibration (that 30 percent of net income is spent on splurges).

In summary, I think you need a convincing story about splurge consumption, otherwise this seems too arbitrary and also implies that your analysis cannot be used for welfare analyses.

3. Choice setting: You present your analysis as partial equilibrium, but it really is simply a choice setting (with some choices not modelled). I do not have a problem with this as such since such models can be used for many interesting purposes.

However, I do think that this means that some of your results may be questioned, and that you need to be very careful with your analysis and perhaps rethink parts of it. Here are my issues:

- Clearly, not all of the policies that you consider are equally affected by the lack of (general) equilibrium effects. Tax cuts work mainly through supply side effects which you exclude. I think it is important to point this out and be less dismissive about tax policy. Unemployment insurance is also sensitive to the lack of a supply side modeling as one usually would think of these as potentially hampering job creation. Stimulus checks are more direct demand policies. Hence, in the end, if you extended your analysis to GE, it is unclear to me whether there would be a clear winner.

- I find it very misleading to talk about multipliers in the choice setting. Your model leaves out equilibrium mechanisms that create the potential for such multipliers (the standard Keynesian cross mechanism for example). You do provide the consumption externality feature as an extension, but it was "unclear" to me that this really allows one to interpret results in terms of multipliers. Indeed, since multiplier effects come from general equilibrium effects, one could seriously question why you look at multipliers at all in your paper.

- In continuation of these points, showing impulse responses at long forecast horizons and calculating present value multipliers seems a bit odd to me (in the pure choice framework, all of the transfers will be spent sooner or later and the multiplier will go to one; anything you get different from that comes from the ad hoc externality). It is still interesting to see how fast this happens, but

why not focus on shorter horizons?

Note that both Referee 1 and 2 make comments about this issue as well.

4. Calibration: I found it odd that the calibration mixes up targets/parameters from the US and from Norway. Why not use targets from Norwegian data (or US data) as far as you can and then simply add to this insights from other papers/data (which might be US related of course). Referees 1 and 3 make comments amounting to the same concern.

5. Welfare Criterion: Referees 2 and 3 complian about the welfare measure that you use. I agree with them, it is somewhat murky. Moreover, how should one actually think about welfare in this choice set-up? I think it is fine to examine some measure of "bang for the buck" but is that really the same as welfare?

Given these comments, one option would be to leave the multiplier and welfare analyses to another (general equilibrium) paper and simply focus on how "splurge" consumption can help account for iMPCs and use the model for the analysis of the impact of some selected fiscal policies. If you choose not to go this way, you need to argue your case carefully.

Please respond also point-by-point to the referees' comments (some of those in Referee 1's comment have also been incorporated in the manuscript so you can just mention that).

I would also like you to keep in mind that the journal requires authors to share their replication data and code files (all tables and figures should be replicable). Prior to being accepted for publication, the software and data will be subjected to a replication check by a dedicated team. You will find here information here:

https://www.econometricsociety.org/publications/es-data-editor-website

Hence, it would be wise to make sure your paper can be replicated and prepare the files although these only will be required in case the paper is conditionally accepted for publication.

I should re-iterate the standard disclaimer: A revise and resubmit decision is not a guarantee that **QE** will publish your work and I can make no promises about the final outcome. But I think the paper has sufficient promise that it makes sense to invite a revision and that it could potentially make a very nice contribution to the journal.

Many thanks for letting us consider you work, I look very much forward to seeing the revision.

Sincerely,

Morten O. Ravn

Co-Editor