

Reviewer 1

*I thank the authors for their effort in revising the paper. All specific comments have been adequately addressed. Also the SCEQ algorithm is now explained in the manuscript. Concerning code availability I welcome that the authors made their code available via GitHub. However, I would like to point out that the chosen format to upload it as a zip archive to GitHub is unusual. As it fulfills my initial request to make the code available it could stay as it is now, but I would nevertheless recommend to convert it to a more conventional format of having the model uploaded in an uncompressed form and have the model archived in a public archiving service (e.g. Zenodo) to ensure that the code is also available for download in the future.*

**We have uploaded the model solution code to Zenodo (see <https://zenodo.org/records/10014997>).**

*The remaining issue where I still feel that the authors did not address my concerns appropriately is the discussion of the robustness of the main finding of the paper (overestimation of land conversion ranges in deterministic models). Instead of adding a discussion of the robustness of finding the authors added a footnote claiming that a unbounded solution space only would make sense if the deterministic scenarios would be unbounded as well.*

**We have removed this footnote in the revised manuscript.**

*Firstly, I am not sure how an unbounded deterministic scenario should look like given that due to its deterministic nature all conditions are prescribed. Hence, the authors seem to claim that every comparison with a deterministic scenario requires a bounded solution space for the corresponding stochastic scenario. It is not clear to me where this conclusion comes from or how it could be justified.*

*Secondly, I think that the authors have not fully grasped the point I was trying to make in my last response so I will try to rephrase and repeat it: For simplicity, let's just have a look at the comparison in Figure 3a) between stochastic pessimistic and deterministic pessimistic where we can clearly see the difference between the two with the stochastic pessimistic scenario showing less agricultural land expansion than the deterministic scenario. In combination with the optimistic scenarios that leads to the claim in the paper that the deterministic analysis leads to an overestimation in the range. So the question is, why does the stochastic approach leads to a smaller range and the answer in the paper is that this happens because it is stochastic. I clearly disagree here. The reason is not that it is stochastic, but that the stochastic scenario works with a bound solution space.*

*Comparing both pessimistic scenarios we have on one hand a deterministic scenario which computes a world in which everything is bad and we know for certain that it stays that bad. At the same time the stochastic scenario computes a world in which we start in a pretty bad state but we know that it will either stay that bad or get better but under no circumstances get worse! That means that this scenario is by definition (due to the bound solution space) a better world than the one computed in the deterministic pessimistic scenarios. Hence, we also end up with less agricultural area in use. In case of an unbounded solution space the model could not make this assumption and would need to prepare for shifts in either direction (for better or worse). How such a scenario behaves we can see quite well looking at the average. Here the probability to get better or worse is identical and the findings of the stochastic and deterministic scenarios more or less agree.*

Thanks for this clarification. We have added the following sentences on p. 17, line 375 of the revised manuscript to clarify this point further: “Compared with the deterministic model under the pessimistic (or optimistic) scenario, the social optimum in the stochastic model requires a smaller (or greater) conversion of other types of land to cropland. This is because when the current state of the crop technology index is the worst (best), its future states cannot be worse (better) and have a nonzero probability of being better (worse). The expected future yields will then be better (worse) than the deterministic-pessimistic (optimistic) scenario. As the size of expected crop yields affects the magnitude of the land conversion decisions, the range of stochastic model solutions for agricultural land will be smaller than the range between the most extreme deterministic model solutions.”.

*Hence, one can argue that a bound solution space will lead to a smaller range in a stochastic approach compared to the range we get if we run the bounds in a deterministic model, but this is only true for a bound solution space. The question is whether a bound solution space is a reasonable assumption here. I would not think so. Given that this is a more conceptual analysis it is okay to use a potentially unrealistic assumption but it is absolutely critical to properly report and discuss it. In particular the required conditions under which this result holds true need to be clearly mentioned as the results are otherwise misleading.*

Thanks for bringing this issue to our attention again. We have added the following sentences on p. 17, line 375 of the revised manuscript to address this concern: “Note this result may not hold if the model solution space is unbounded. This concern doesn't apply to the stochastic FABLE model because (i) the model's time horizon is finite; (ii) the crop technology shocks are discrete and finite (hence bounded) based on scientific projections used for the model's calibration; (iii) all of the model's state variables (land and fossil fuel resources) are bounded because the total land and the total fossil fuel resources are finite; and (iv) we impose bounds on model decision variables based on the theory of economic dynamics (Barro and Sala-i-Martin, 2004), such as strictly positive and finite consumption and output of land-based goods and services; land conversion cannot exceed the total supply of land). The solution space, therefore, must also be bounded because the extent of movement of optimal land uses in any direction is limited by the constraints mentioned above.”

Reviewer 2

*1. About climate impact scenarios from ISIMIP (fast-track) or Roseweig et al. (2014), was there a reason for not using more recent scenarios? I believe Jonas Jaegermeyr, a coauthor of the paper, has led studies/experiments of the more recent ISIMIP rounds (e.g., ISIMIP 3b). I think it would be great to provide some clarifications. Using fast-track data seems fine for testing the model, but I think audiences would like to know to what extent more recent data could affect the results.*

**Unfortunately, these data were unavailable when the manuscript was submitted to GSM (it took a long time for the journal editors to find reviewers). Given the methodological scope of the paper and very lengthy revisions associated with revising model baseline and climate yield shocks, we chose to leave this exercise for future, more policy-oriented research.**

*2. In your response, you mentioned: “Since this is a social planner's problem all prices are effective shadow prices, which are determined endogenously by the model.” I am confused by the shadow prices here. The model has both demand and supply. Isn't that prices are variable solved? I would appreciate more clarifications as to why market price results cannot be provided.*

The model solves for optimal quantities (i.e., different allocation of land uses in GHa) under the supply (e.g., total land and fuel resources) and demand (final consumption of land-based goods and services entering utility function) constraints, both in quantities. The prices don't explicitly enter the model and are calculated as Lagrange multipliers (or shadow prices) for respective resource constraints.