Editor

The revised manuscript has been seen by referee #1 once more. I concur with their assessment that the revisions insufficiently address the outstanding concerns. The assumption that crop technology parameters are (a) bounded to the most pessimistic / optimistic case of the deterministic model (which are not necessarily the maximum / minimum values from a biophysical perspective, and (b) not auto-correlated along the time axis (low crop yields at time t make it less likely that yields are high at time t+1) introduces artifacts that affect the core conclusions drawn in the papers abstract and conclusions.

The assumption (a) that crop technology parameters are bounded is necessary to avoid completely unrealistic cases when crop productivity becomes infinitely large or small and is consistent with biophysical modeling studies cited in our paper. As regards the assumption (b) the crop technology parameters are, in fact auto-correlated along the time axis in the stochastic FABLE model.

To remedy this problem, the authors should revise the underlying model. If there are fundamental reasons preventing you from adjusting your model, the interpretation of the results as well as abstracts and conclusions must be thoroughly revised.

In light of the above, we don't think the model itself needs any revisions. Instead, we have made further clarifications about the validity of assumptions (a) and (b) in the revised manuscript. As regards the assumption (a), we have made it clear in the abstract, introduction, the model description, and the conclusions part, that our model has a bounded solution space, which is a standard assumption in economic modeling of natural resources, and that our shocks are bounded to avoid unrealistic realizations of crop technology parameters that would prevent finding model solution. Please refer to our response to reviewer 1 for the list of specific changes made. As regards the assumption (b), please note the following sentence on line 293 of the revised manuscript "Observe that the serial correlation of random variables has been captured in their associated transition laws", and the appendix section B3 describing the discretization of autocorrelated shocks.

Reviewer 1

I thank again the authors for adjusting the manuscript based on the made recommendations. The text now contains two paragraphs explaining the linkage between the bound solution space and the reduce range of outcomes. However, the new manuscript still completely ignores that the key findings of the paper are basically invalidated by it. For example the abstract still says that "For the same model parameters, the range of land conversion is considerably smaller for the dynamic stochastic model as compared to deterministic scenario analysis. This highlights the importance of incorporating uncertainty in the model's optimization stage to determine optimal paths of natural resource uses." This is plainly misleading as a) the smaller range is not a consequence of running it with a stochastic model, but a consequence of the bounded solution space (in an unbounded stochastic case the range would be identical) and b) incorrectly deducts that this would highlight the necessity to do stochastic analysis (there are other good reasons for stochastic analysis, but this is not one of them).

We have revised the abstract, deleting the last sentence, and changing previous sentence to "For the same model parameters and bounded shocks, the range of land conversion is considerably smaller for the dynamic stochastic model than for deterministic scenario analysis." Indeed our findings will not necessary hold when shocks are unbounded.

In addition, to justify that the approach of a bound solution space by the explicit assumption of a bound solution space in the FABLE model is somehow questionable. It just means that the yield assumptions in the model are consistent to the other assumptions of the model, it does not mean that it is a sensible approach overall. The main question is whether a bound solution space is a reasonable approach to model reality or if it is not and there are good reasons to question this assumption.

We have addressed this concern by adding the following sentences on line 107 of the revised manuscript: "Similar to other models in this class, the FABLE model has a bounded solution space, as all these models are theoretically shown to have equilibrium paths (Stokey, 1989). This assumption is important because it is often impossible to prove that in the presence of unbounded solution space, the stochastic model has a finite solution. For example, Weitzman's dismal theorem (Weitzman, 2010) shows that a fat-tail damage function with an infinite upper bound leads to an infinite risk premium, but a numerical truncation to finite support will always have a finite risk premium. So, assuming the bounded solution space is necessary for avoiding the potential qualitative inconsistencies between their theoretical and numerical results. We further discuss the assumption of the bounded solution space in the FABLE model in section 5.

To bring this manuscript into a form acceptable for publication it would be absolutely important not only to mention the shortcomings of this methodology, but also to get rid of all these incorrect conclusions in abstract, discussion and results. At the moment it mentions the shortcomings at some point but downplays them and completely ignores them in the rest of the manuscript.

In addition to changing abstract we have added the following sentences in the introduction and conclusions sections:

Line 60: "To ensure consistency between theoretical and numerical model solutions, we assume the bounded solution space. As we show below, this assumption is well justified for economic models of large natural resource allocation problems, including the FABLE model."

Line 72: "We then compare and contrast them with the results of the dynamic stochastic model, where the global planner has rational expectations about crop yields subject to bounded autocorrelated climate shocks"

Line 78: "This result indicates that when the climate shocks are bounded, the scenario analysis may significantly overstate the expected agricultural land conversion magnitude under uncertain crop yields."

Line 464: "Similar to other dynamic economic models, it assumes a bounded solution space,

excluding unrealistic scenarios of infinitely low or high crop productivity."

Given that these concerns were already raised in the last two rounds of review I am a bit surprised and concerned that this still has not been properly addressed by now.

We hope this last iteration satisfactory addresses your remaining concerns.