

***Interactive comment on “Assessing uncertainties in global cropland futures using a conditional probabilistic modelling framework” by K. Engström et al.***

**Anonymous Referee #1**

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Dear authors,

you present an interesting manuscript deriving probability distributions for global cropland areas from the new narratives describing the IPCC SSP scenarios. I think this is a valuable contribution as it tries to associate numbers with the narratives, helps imagining a bit more vividly how the world could look in these scenarios and what implications this could have for land use, and also serve as an example for other researchers that work with these scenarios. At the same time it exposes the uncertainties left open by the scenarios and the difficulties in working with them.

In general, I find the methodology you used and the results that you produced defensible, although I miss a more explicit discussion of some of the limitations of the analysis

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and its relationship to more structured land use models. Some thoughts on this:

a) You write that PLUM is consistent with other, more complex models of global land use change. (Quotation still under review) Besides the fact that this may just mean that it inherited their problems, what is more important here is the question in how far the results you obtain in this article are representative of the results you'd obtain with these models. Does the fact that PLUM has a simpler structure mean that the outcome distributions are wider than with other models, because there is less structure constraining them? Or are there locations in the outcome distribution that may not be produced by PLUM (but by other models), because it would require interaction effects between input variables/processes that are not incorporated in PLUM? In the end, are other models likely to produce the same outcome distributions (or wider or narrower ones) if they could be used in the same experiment?

b) That PLUM reproduces land use change between 1990-2010 counts in its favor, but cannot really be judged without the specific assumptions made in the validation runs, and does not guarantee much for land use change on longer time scales in a potentially very different environment in which fundamental parameters may change. What needs to be considered here is whether the structure assumed within PLUM is likely to remain unchanged and potential structural changes can explicitly captured within the input parameters. If that is the case, the comparatively reduced structure of PLUM may be an asset that may be exposed more clearly.

My other concern is mainly with the exposition of your research in the manuscript. It lacks a bit of detail on some important assumptions and is unclear on others. My specific remarks in this respect:

1. section 1,2: I think the understandability and readability of your article would benefit from restructuring. You should mention earlier and consistently from the beginning that you are going to compare five future scenarios which correspond to the five SSPs. The probability distributions/ranges assumed for uncertain pa-

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rameters - including the RCPs - are depending on the scenario and differ between scenarios. You might also want to consider reordering subsections in the Methodology section (2), starting with a discussion of the scenario/uncertainty approach (currently 2.2) and only then give details on the PLUM model (currently 2.1). This would proceed from the more general to the more specific and follow the structuring of the introduction. In section 2.2 (original numbering), you should then start with something like. "We construct five conditional probabilistic futures (F1-F5), each constructed based on the qualitative and quantitative information in the corresponding SSP (SSP1-5). ... The extent of climatic change within each future was determined by assigning a SSP-specific probability of each RCP ... " or something similar. I think this would help readers grasp what you are doing at the first reading and right from the start and not having to re-read a few times until that connection becomes clear. There is a clear hierarchy between scenario and other input (and RCPs are just one of them) in your scenario construction and your description should reflect that.

2. p.2, l.26: I think this sentence is misleading, because basically you use the five SSP scenarios to construct futures. The RCPs are modeled as dependent on SSP, in the same way as many other uncertain input factors. I think it would be helpful for the understanding of the article to make that clear right from the start and introduce the F1-F5 already here.
3. p.3, l.9: A constant cropland-cereal land ratio seems a plausible assumption for 1990-2010, but is there any indication that this will hold for longer time-scales and everywhere around the world?
4. p.3, l. 22: It is not clear to me, how actual and potential yields were calculated: You write that the potential yield is the yearly maximum in each grid cell. Does that mean the maximum yield simulated in any year between 2000 and 2100? Or, for each year, the maximum that was obtained for any cell in the country?

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(Be clear what is per cell and what is per country here and elsewhere.) Likewise, what is the difference to the actual yield? Is the actual yield also simulated or is it observed in the MIRCA dataset directly? The paragraph is not very clear.

5. p.3, l.24 and l. 30: You seem to do a bias correction here, even twice: Once in LPJ-GUESS and once in PLUM, making simulated yields match observed yields. I miss an explanation on why this is necessary and justified, as well as a discussion on which implications this has for the conclusions that can be drawn from your model. (Is it still applicable for extrapolation into the future, i.e. out-of-sample?) (see Ehret et al. 2012, Hydrology and Earth System Sciences, albeit for a different context)
6. p.5, l.31 : Strictly speaking, you are referring to coefficients of variation rather than standard deviations as you are using percentages, aren't you? Or do I misunderstand the paragraph.
7. p.7 l.3; p.7 l.10; p.5, l.17: Although based on established secondary data, there still seems to be quite an "ad hoc" component in the specification of probability distributions for parameters: Is there a justification for assuming normal distributions for all parameters? Is there a justification for assuming independence between input factors. One might argue that uniform or heavy-tailed distributions might better reflect the uncertainty about these distribution themselves and one might also imagine that some parameters could be covariates. E.g. which population projections are assumed by the studies you consulted for projections of global GDP development? Are certain population projections correlated with lower or higher GDP development? Independent sampling from normal distributions for many parameters means that cases where several parameters are at their extremes are unlikely to occur.
8. p.7 l.15 : Did you consider to use an alternative sampling schemes (importance sampling, Sobol' sequences etc.) to increase sampling efficiency?

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9. p.8. l.21ff.: As the description of yield simulations in section 2.1 left me in some doubts, I cannot really follow at this point either. The paragraph here is not clear about your intentions nor the details of implementation in using SVD for analysis and resampling. What "pattern resulting from the GCM-RCP yield projections" is to be preserved? Is it the differences between countries? The difference between GCMs? The differences between RCPs? All in all, you do not explain yourself well here...
10. p.8. l.24f.: Isn't it SINGULAR value decomposition rather than single value decomposition? Equation 1 is pretty cryptic. Can you use SVD in a formula and add to it? Isn't the outcome of SVD three matrices? Which one is used in this equation here? (Is there any reference for this use of SVD or did you come up with this approach yourselves?)
11. p.8. l. 32 (Eq.2): A bit more space between the comma and the second epsilon would greatly increase readability.
12. p.9. l.14: There is a bit of a confusion about references here. What is the Sobol-jansen method? Is it an R implementation of the method described in Lilburne and Tarantola 2009 or Saltelli 2010 or Engström 2016? Or is it a specific method of GSA? Please clarify and elaborate a bit more.
13. sections 3.2, 4 and 5: I found your use of convergent and divergent quite confusing. You use it to refer to the fact that three scenarios (F2,F4,F5) have strongly overlapping domains of the outcome distribution, which you describe as convergent, while F1 and F5 are clearly distinct from the each other and from the other three, which you describe as divergent. But, on the other hand, F1 and F3 show rather peaked distributions, which one might also associate with convergence (uncertainty within the scenario converges around a common mean) and which would be closer (although also not matching) the common use of convergence in statistics, i.e. the convergence to a value as repetitions increase. (F2-F4 show

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rather wide distributions in contrast that would then rather be divergent. ) Especially, when you speak of a convergent (resp. divergent) scenario, at least I rather have the association of convergence (divergence) WITHIN the scenario and not between several scenarios.

At least to me, this caused some confusion, especially since you state "the confidence in the model outcomes for F1 and F3 are the highest, DESPITE the fact that these two scenarios show divergent global cropland development." If you meant convergence/divergence in the statistical sense, this contrast between confidence and divergence would warrant the despite. However, divergence in your use, i.e. the observation that certain scenario assumptions lead to other outcomes than the (slight) majority of other scenario assumptions is not necessarily at odds with higher confidence.

14. I would avoid the use of convergence/divergence for the pattern you describe. E.g. you could maybe use "strong similarity" instead of "strong convergence" in p.10 l.14, and "represent the extremes of simulated" instead of "show divergent" on p.10,l.10. On p. 11, l.18, you could simply replace "converge" by what you put in parenthesis anyway, and so on.
15. section 3.3: Why didn't you include the RCPs in Fig.5 (you mention in section 3.1 that their influence is minor, but nevertheless this would complete the picture and put a number to your statement.)
16. sections 2,4: While you do an extensive uncertainty analysis on parameters that capture how the future might develop, you seem to use - in several places - model parameters that are rather ad hoc, without deeper justification, e.g. a cereal-to-cropland share of 60
17. section 5 (Conclusions): I miss two important aspects in your conclusions:

- I miss a short summary of the crucial assumptions incorporated in our model that the obtained results are based on. "Considering current knowledge on crop responses incorporated in crop growth model X, a fixed cereal to land share, etc .. , we conclude .. " Especially in uncertainty analysis such as yours, where a lot of parameters are varied, it is easy to lose track of what are the crucial pillars that the analysis is built on and that give structure to the analysis. You do not vary everything, but there are certain assumptions and model structure that are fixed and bound the results that you obtain.
- I also miss a reflection of what we have learned and in how far you were successful in dissecting the large uncertainty range you mention in the first paragraph of the introduction. Certainly, the uncertainty range was not reduced. So, what have we learned? We know something more about which conditions are necessary to produce the extremes of the distribution. Anything else ...? To what extent is this a new outcome and not yet visible from looking at and disentangling the results of the yet unpublished comparison study (Alexander et al., under review ) you cite?

18. p. 13 l.15f: Again the use of divergent and convergent here is confusing and misleading (see above).
19. Anywhere: What about the Shared Policy Assumptions? Are they reflected anywhere?
20. Fig.4: These graphs - as they are - are only readable in color. I could not distinguish different futures on a grayscale print out. If possible, consider placing a scenario label next to each line, then one could also read these graphs in black-and-white.

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