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Earth System Dynamics Discussions

Interactive comment on "Partitioning climate projection uncertainty with multiple Large Ensembles and CMIP5/6" *by* Flavio Lehner et al.

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The manuscript is well presented, scientifically rigorous, timely and in my estimation, certainly worthy of progressing towards rapid publication.

However, I do have a few relatively minor comments and suggestions:

1. In the introduction, while discussing the three sources of uncertainty, the authors discuss (a) uncertainty from internal unforced variability as well as (b) response uncertainty or model uncertainty. I would suggest the authors add at least a pointer (with a citation) to what has also been called "large ensemble of climate model simulations" which are obtained from "ensemble of model versions constructed by varying model parameters". One citation can be to Murphy et al. (2004), Nature 430, 768-772. How

this multi-parameter uncertainty fits into (or not) the overall discussion here could be useful for a reader to understand.

2. The skill versus consensus considerations when assigning uncertainties to projections may need to be discussed along with the need for physics consistency. Smith et al. (2009), Journal of the American Statistical Association, 104(485), pp.97-116, attempt to develop a statistical method for balancing skills versus consensus. An example of using physical basis for constraining uncertainty from models in provided in the context of precipitation extremes by O'Gorman and Schneider (2009), PNAS 106 (35) 14773-14777.

3. The Deser et al. (in review) paper is cited multiple times. Most journal allow authors to upload their manuscript on preprint servers such as arXiv without compromising novelty. Is that possible in this case?

4. Under uncertainty partitioning, the authors assume the three types of uncertainties are additive, without providing any context or caveats. The authors may need to clearly state whether this additive (and linearly separable) formulation for the three types of uncertainty is an assumption or a hypothesis, or if this is an assertion. In either case, the caveats of the assumption or a way to falsify the hypothesis or a rationale for the assertion should be provided.

5. While the manuscript makes a few distributional assumptions, I would suggest a more thorough discussion of a couple of points: first, the (assumed) asymptotic behavior (or the lack thereof) for the single-model initial condition ensembles (in other words, is there a reason to believe that with larger number of ensembles the distribution will converge or asymptote to a statistical distribution, and if so, is there any issue of ensemble sufficiency that needs to be investigated) and second, any assumption about the shape of the distributions (e.g., symmetric, or even Gaussian, etc.). I am not sure if there is enough basis to calculate the mathematical forms of the distributions from HS09.

6. The authors may need to discuss (and set appropriately in context) the visual intersection of the overall uncertainty with the zero-line in one case but not the other (Figure 1: (e) and (f)) and the relative changes in model and scenario uncertainty in CMIP5 versus CMIP6 (Figure 3: (b) and (c)).

7. The impact of internal variability on the phase difference of climate oscillators in model simulations may need to be discussed a bit more thoroughly. The others do provide an example with the Sahel. One example is provided in the context of the Indian monsoon is provided in Kodra et al. (2012), Environmental Research Letters 7(1): 014012, especially in the supplement.

8. As a stylistic matter, while I personally agree with the statement made by the authors that HS09 "created a powerful narrative of reducible and irreducible uncertainties in climate projections", I would nevertheless suggest deletion of superlatives such as "iconic framework" and "landmark paper". I would especially recommend this since one of the two authors of HS09 is also an author of this manuscript, but even otherwise.

9. As a "nice to have" suggestion, I am wondering if the value of this manuscript may be increased by a simplified exemplar. I am reminded of the ESM 2.0 paper: Schneider et al. (2017), Geophysical Research Letters, DOI: 10.1002/2017GL076101. That paper uses the Lorenz-96 model as an exemplar. While that GRL paper and this manuscript are very different indeed in scope and content, I was wondering if an exemplar such as a variant of the Lorenz model (with different initial conditions, different parameterizations as proxies for different "models", and a mock-up of different "forcing") may be developed here to clearly and concretely illustrate the basic points.

10. Finally, since most readers may not be familiar with HS09, I would suggest a clearer and more easily understandable (to a broad audience) discussion on what this means for the climate community, both for understand the science and for translating (or even starting to develop a conceptual framework to translate) to risk-informed decisions.

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