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Earth System Dynamics
Discussions

Interactive comment on "Projecting Antarctica's contribution to future sea level rise from basal ice-shelf melt using linear response functions of 16 ice sheet models (LARMIP-2)" *by* Anders Levermann et al.

Anonymous Referee #1

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In this manuscript, the authors use the linear impulse-response model previously presented in Levermann et al. (2014) to fit Antarctic ice sheet simulations from sixteen different ice sheet models, allowing the construction of a simple statistical emulator of these models' behavior in five aggregate ice-sheet sectors. This is a useful tool for producing future probabilistic sea-level projections.

It is, however, somewhat regrettable that the paper does not give more attention to at least attempting to explain the differences among ice-sheet responses – the differences among models that can be observed in figures 4 and 5 are stark, and some

discussion would be very welcome. As just one example, there are major differences among models in the amount of high-frequency variability, and an attempt to put some explanatory taxonomy on these differences would be extremely useful.

What are we to take away from the GCMs with poor fits and long delays (e.g., IPSL-CM5A-MR and MRI-CGCM3)? Is it valid to include these in projecting the forcing? It would be helpful to have supplemental figures showing the underlying data.

It would also be useful to have some quantitative measure of the quality of the linear response approximation for each ice sheet model. This can be eyeballed from Figure 4, but it'd be better to have an objective metric.

Structurally and stylistically, the paper has some significant flaws that hinder its comprehensibility. Most importantly, the paper clearly expects the reader to have recently read Levermann et al 2014. While I understand the authors' desire not to be repetitive, the paper needs to be comprehensible on its own. A clear summary of the assumptions and approach of Levermann et al 2014 needs to be provided early on. The second half of section 2.4, which provides the equations underlying the statistical emulator, comes way too late – the structure of the emulator needs to be clearly described first, followed by a clear description of the calibration, and then followed by a clear description of the projection method. The current version is all tangled together.

In the discussion, I would encourage the authors to contextualize their results based on other available information. For example, the structured expert judgement study of Bamber et al. (2019, 10.1073/pnas.1817205116) might provide useful context.

The authors' tone is also peculiarly (and conversationally) beseeching and apologetic. It is important for the authors to be clear about the limitations of the approach, but polite imperatives ("Please note this", "Please note that") are excessively circuitous; the authors should just state what they wish the reader to note (and do it in a place that makes logical sense, given the flow of the paper).

On page 10, the last paragraph seems a bit excessive given the decision not to weight the different models. I would either drop the equation and keep only a qualitative discussion of model weighting, or (perhaps better) do a sensitivity study using model weighting.

The paper is also missing a proper conclusion. A paragraph explaining the decision not to weight is a peculiar way to conclude.

Figure 1: "intervall" -> "interval"

Tables 11 and 12 have no units.

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