

The Cryosphere Discuss., referee comment RC2
<https://doi.org/10.5194/tc-2021-175-RC2>, 2021
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Comment on tc-2021-175

Nicolas Jourdain (Referee)

Referee comment on "Brief communication: A roadmap towards credible projections of ice sheet contribution to sea level" by Andy Aschwanden et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-175-RC2>, 2021

Conflict of interest:

I was involved in the working group that designed the ISMIP6 ocean forcing for Antarctica (Jourdain et al. 2020) and I am one of the numerous co-authors of several ISMIP6 papers, although not those on Greenland. I am not part of the core team that framed and lead ISMIP6.

Summary and recommendation:

This paper is a comment on the relevance of using the Greenland ISMIP6 ensemble as a base for anticipation and mitigation of future sea level. The authors present four types of uncertainty (model, initial state, parameters, aleatoric) and explain that ISMIP6 does not account for most of them, and therefore likely underestimates the range of uncertainty on future sea level rise. They propose a path forward that consists of running more simulations to further sample uncertainty and conditioning ensemble predictions on observations. Then, they state that a volunteer effort such as ISMIP6 is under-resourced, e.g. compared to CMIP6. This short communication can be useful for the ISMIP community, but several aspects could be improved to make it more relevant (see details below). I therefore recommend this manuscript for publication after a major revision.

Major comments:

1- The authors are quite critical of the ISMIP6 design, but they should acknowledge that ISMIP is very new compared to CMIP which started in 1995. It is therefore normal that ice sheet projections are not as mature as ocean-atmosphere projections. I am sure that the ISMIP community is well aware of some of the mentioned limitations and this will

hopefully be improved in the future MIPs. I nonetheless agree that the range provided by ISMIP6 or its statistical emulation should not be considered as a comprehensive estimate of uncertainty. But this is also true for the CMIP6 ensemble in which the parametric uncertainty is poorly or not evaluated.

2- It will be challenging to make ISMIP simulations match the observational trends as long as ISMIP is forced by or coupled to CMIP models because: (1) the CMIP groups build their preindustrial control simulation by running multi-centennial or multi-millennial simulations as close as possible to a steady state (although some models still drift, e.g. Sen Gupta et al. 2013). Then, they branch off their historical simulation(s) randomly from this preindustrial run and constrain the simulation with observed anthropogenic emissions since 1850. This approach does not seem compatible with ice-sheet simulations initiated from long paleoclimate spin up. (2) Although the authors claim that natural climate variability has little effect on the observed sea-level contribution (L.208-211), low-frequency natural variability may still affect the trend values and make it difficult to compare individual CMIP-based ice-sheet projections to observed trends (only one model ensemble member should match the observational time series to consider that the model is good, not all members).

3- I am not convinced by the relevance of the "aleatoric uncertainty" (L. 110-122). First of all, the chaotic nature of the atmosphere and ocean forcing is represented in individual CMIP6 simulations (this is the aim of the multiple ensemble members provided by many CMIP groups since at least CMIP3). The resulting low frequency (interannual to decadal) natural climate variability is therefore represented in the ISMIP6 forcing, although probably underestimated due to non-eddy ocean models (Penduff et al. 2018) but this is more a CMIP6 issue than an ISMIP6 issue. If the authors have in mind shorter natural variability (extreme weather or seasonal events), then MISI is probably not a good example as the ice sheet may not be sensitive to short ice-shelf basal melt variations (e.g. Favier et al. 2019), and a better example would be hydrofracturing (e.g. Robel and Banwell 2019). In this case, however, the uncertainty is probably more on the ability of ice-sheet and firn models to represent the entire hydrofracturing process than on the atmosphere forcing.

4- The paper is mostly about ISMIP6 Greenland projections although the title seems to point to ice sheets in general. The MISI is also quite often used as an example although it is mostly relevant for Antarctica. I think that most comments are also valid for ISMIP6 Antarctica, so I recommend balancing the paper between the model intercomparison for both ice sheets.

5- The discussion on the lack of substantial financial support assumes that ISMIP and CMIP are two distinct entities. However, several modelling groups involved in CMIP are currently working on the coupling of ice-sheet models into Earth System Models (e.g., UKESM, CESM, IPSL-CM, EC-EARTH), with accepted projection papers for some of these groups (i.e. potential contributors as soon as CMIP7). The introduction of ice sheet models into these ESMs might change the financial aspects for these ice-sheet modelling groups. This should be mentioned.

6- Given that there are comments on the use of ISMIP6 results in IPCC-AR6, the final version of the report (now publicly available) should be cited and described more accurately both for the likely range they provide (section 9.4.1.3 and Tab. 9.2 of IPCC-AR6) and for their attempt to estimate high-end projections (Box 9.4).

7- There should be more discussion on how to account for the deep uncertainty related to processes that are not represented, e.g., ice-sheet feedbacks to the climate system (e.g. Sadai et al. 2020), hydrofracturing and MICI (Lai et al. 2020; DeConto et al. 2021), shear margins (Lhermitte et al. 2020).

Other comments:

- The title is a bit misleading as the paper is only about Greenland.

- L. 24: a recent ISMIP6 reference that is more related to CMIP6 is Payne et al. (2021) <https://doi.org/10.1029/2020GL091741>

- L. 30-31, about "Implicit in this approach is the assumption that the ensemble of ice sheet models perfectly spans, without bias, the range of potential sea level contribution": this is not specific to ice sheet models, it is also true for the CMIP6 ensemble, and it should be noted that a single regional atmosphere model (MAR) was used to calculate the surface mass balance and melt from the CMIP6 projections (Goezler et al. 2020).

- L. 31-32 about "This ISMIP6 distribution has since been adopted as the foundation for the IPCC AR6 consensus estimate of sea level contribution from ice sheets": the AR6 (section 9.4.1.3) gives likely ranges based on the emulation of the ISMIP6 ensemble (Edwards et al. 2020), not based on the raw ISMIP6 distribution; furthermore, the IPCC corrects the estimates from Edwards et al. by adding the historical trend (see Table 9.2 of IPCC-AR6). There is also an entire box on the deep uncertainty and possible high-end projections (Box 9.4 of IPCC AR6) which is made for stakeholders with low risk tolerance.

- L. 75: instead of "poorly represented", I would write "poorly or not represented".

- L. 96-109: it may be worth mentioning that Hill et al. (2021, <https://doi.org/10.5194/tc-2021-120>) and Bulthuis et al. (2019) also investigated parametric uncertainty but for Antarctic projections. A limitation is that this is often done

for a limited number of parameters and that it is difficult to define an acceptable range of parameter values.

- L. 140-151: assuming the parametric uncertainty provided by Aschwanden et al. (2019) is a reasonable estimate for all ISMIP6 models is probably a very strong assumption. First of all, some parameters used in Aschwanden et al. (2019) are related to atmosphere and ocean forcing method that significantly differ from ISMIP6 (with completely different parameters). Then the parametric uncertainty can be highly ice-sheet model dependent. Last, I guess that the methodology used to vary parameters while keeping the model trajectory consistent with paleo-climate proxies can be a matter of debates. Having said that, I agree that the parametric uncertainty was not explored in ISMIP6, and that it should be explored in future ISMIPs. This is also true for the majority, if not all, of the CMIP projections, and some groups are now using increasing computing power to quantify this uncertainty rather than increasing model resolution.