Comments to "Eploring ice sheet model sensitivity to ocean thermal forcing using the Community Ice Sheet Model (CISM)" by Mira Berdahl et al.

1 General comments

In this study, the authors implement the ice-sheet model CISM to explore the mass change of Antarctica ice sheet in 500 years with various combinations of basal sliding and sub-ice-shelf melting parameters.

The study is a supplementary of Lipscomb et al., 2021. In the previous study, the authors explored the uncertainties of Antarctic sea-level contribution caused by grid resolution, forcing scenarios, basal friction law, melt schemes and ocean forcing. In this study, the authors expand the sample range of the basal friction parameter and added the ocean circulation parameter that modulates sub-ice-shelf melting.

My major concern is that this study doesn't add significantly more information to the existed study of Lipscomb et al., 2021. Some significant effort is needed to match the standard of The Cryosphere. I have some suggestions on this:

- Model validation. Model results from simulations present major mass loss from Ross and Weddell sea region, and East Antarctica to a smaller level, while Amundsen sea area contribute the least to sea level. This is very different from present-day observations in the recent decades.

- Historical runs could be conducted before forcing with the future scenarios. The spin-ups are done to have steady-states, while some basins are not at steady-state. And transient runs at historical time could constraint the model (Reese et al., 2020 https://doi.org/10.5194/tc-14-3097-2020).
- The difference between the high limit and low limit of gamma differ by 5 times, which also means 5 times melt rates difference due to gamma. Constraining this parameter by observational melt rate may exclude some samples and reduce the uncertainty of the model results. If that has been done, please describe in the manuscript.

- Model physics: Revisit the parameters related to the Coulomb type friction law. The initial state after spin up give a very different grounding line line positions at some fast streams. I wonder if that's due to the initialization method. At some of the fast streams such as Pine island glacier, Coulomb like friction law applies, in which case C_p plays little role. However, C_c is a fixed value from Asay-Davis et al., 2016, in which experiments and parameters are chosen for ideal geometry. Specifically, $C_c = 0.5$ is chosen to have a continuous basal traction, not necessarily apply to real glaciers. This may also the reason of low sensitivity to basal friction, simply because the nudging parameter doesn't impact the basal traction in the fast flow areas. I suggest more effort on the validation of the parameters related to the basal traction, in order to get the grounding line locations closer to observation.

2 Specific comments

- The second part of the introduction should be in method.
- L21: The Antarctic ice sheet...sea level (GMSL). Need to add the time scale.
- L27: 'deep uncertainty' → 'poorly known processes'
- L29: MICI is not physics. Hydrofracture and cliff failure are the processes that induce MICI.
- L47: Put acronym (AIS) the first time 'Antarctic Ice Sheet' is used.

- L69: 'friction' → 'basal friction'
- L62-L76: The motivation of this study is not clearly described. Lipscomb et al., (2021) explored the two parameters in their previous study using the same model and same spin up. This study is a good supplementary to the previous work, but this work does not add much info to the existed work.
- L77-L85: How did you select the CMIP6 models?
- section 1.1: This section is more suitable for method section? Furthermore, this section could be simiplified because some of the ISMIP6 ocean parameterization methods such as PIGL is not used in this study. equation (2): how did you define and calculate the slope? Does it change with time when the geometry changes?
- L117-L123: Please justify the ocean forcing parameterization method. The authors mentioned that the method in this study results in melt rates that differ appreciably from observational estimates. Can you present 1: the deltaTs needed to have reasonable spin-ups, and what's the difference between spin-ups; 2: melt rates and its variation in between the spin-ups.
- L135: ρ_{sw} instead of $\rho_s w$?
- L145: Can you explain the limitations of Lipscomb et al., 2021 that are improved in this study?
- L53: Reference of ABUMIP should be Sun et al., 2020.
- Can you present the model drift of both velocity field and ice geometry? Furthermore, can you present the variation of velocity, ice thickness and grounding line locations among the spin-ups? The initial state of simulations such as different ice geometries could result in essential differences in dynamical responses of the ice sheets.
- L169: delete the sentence 'There is no hydrology in the basal friction field.' because water pressure used in this study is simplified basal hydrology.

- L174: Label the Anumdsen sector and the Kamb Ice Stream in the map.
- How did you decide the range of the parameter gamma0? It seems (from Figure 3) there is no experiment with both high gamma0 and low p values, what's the advantage of the non-uniform sampling methods?
- L198-L203, Figure 4:
  - Dash line missing in Figure 4d?
  - Why grounded ice area and grounded ice mass are different from observation from the beginning?
  - As mentioned before, can you show the spatial variation of the spin-ups?
- L222: 'monotonically increase' → 'monotonically change'? According to Figure 6, negative anomalies exist.
- Quite a few of climate models present non-linear behaviour in the future thermal forcing change. Why not using the original model results?
- L232: What scenarios are the TF anomalies (1, 1.5 and 2 degrees) represent?
- L266: Figure 7 seems to suggest the differences caused by the parameters increase over time (instead of 'less pronounced after 100 years')?
- L279: 'faster-than-linear' → 'non-linear'
- L296-L297: Why smaller p results in higher effective pressure? Seems to be conflict with equation (4).
- L299-L301: This is contradict with observation. I think some model validation work should be done to explore parameter ranges or processes.
- L351-353: The effect of high topographic seafloor points is not clear to me from Fig. A9. The labelled pinning points are close/higher than sea level. Grounding lines form around them because these mountains are never ice free thanks to surface mass balance.

- L409-L410: I think more effort is worth made to improve on constraining the parameters sample range.
- L420: The hypothesis of MISI (or any instability) could only be proved at steady states, which is not the case of this study. Fast retreat doesn't mean MISI.
- L426-427: Again, grounding line hovering around these areas doesn't mean they are stabilizing upstream flow.