

Anonymous Referee #1:

1 General

In this paper, the authors describe the framework for the ISMIP6 numerical experiments. This is clearly an important piece of work that documents a tremendous amount of effort, minutia, and thought. At the outset, it was not clear to me that a peer-reviewed scientific publication is the best venue for such a manuscript. However, given the readership of the The Cryosphere, I think that this is a fair choice and would support publication after a few minor revisions.

We thank the reviewer for this review and all the suggestions. Indeed, designing the framework for the ISMIP6 protocol is the result of the work of a whole community, and as an endorsed MIP of CMIP6, we are required by CMIP6 to record in a peer-reviewed journal the detailed protocol for our experimental framework.

2 Remarks

1. From my perspective, the compilation of surface temperature anomalies and surface mass balance anomalies for several different models for both Greenland and Antarctica is very interesting. These are plots that I will likely refer back to and possibly use in talks. That said, I am confused about whether these results are published elsewhere and included here for succinctness or if this is their first presentation. If, indeed, this is their first presentation, I suggest highlighting this fact in the paper to a greater extent.

We thank the reviewer for this suggestion. This is indeed their first presentation in the literature (and the first time that these datasets were generated). We have modified the text slightly to highlight this in sections 1, 4.1, 4.2 and 7.

2. The number of acronyms in this paper is off the charts. I understand that this comes with the territory, yet it is still a hurdle to understanding the contents of this paper. I suggest (a) a table of acronyms in the appendix before the list of tier 2 simulations and (b) at every instance possible, avoid using an acronym or use both words and acronyms. I would understand if the authors find this request difficult to implement, my main request is that they think critically about whether or not every acronym is actually required and make an effort to reduce the total number.

We thank the reviewer for this comment. We have significantly reduced the number of acronyms used, as well as provided a list of acronyms in Appendix E.

3. Numerous 'under review' papers are cited. This makes sense because this paper and the cited papers are pieces of a larger puzzle, however, it would be ideal for the authors of this paper to explain the results of the cited papers to a greater degree, given that the referees have no access to the contents of those papers. In the future, this will also be beneficial as it will highlight the connections between each piece of the ISMIP6 puzzle.

Two of the "under review" papers are now published, so we revised the manuscript. The other two manuscripts are being revised. However, we revised the manuscript as suggested, and provide a summary of these related ISMIP6 publications in order to highlight the connections between each pieces of the ISMIP6 puzzle.

3 Specific comments

page 11, line 23: here the 'ISM' is in the subscript whereas in other places, e.g. line

25, it is not. Which one is correct? I find the double subscript cumbersome but also think that the $T_{ISM_{RCM}}$ notation is difficult to wrap my head around.

Thank you for having spotted the mistake in equation 5. We have corrected the mistake and double checked all equations. We think that it is really important to keep track of whether a field comes from an RCM or AOGCM, and whether the field refers to a forcing applied to the ice sheet model, despite resulting in a more complicated notation. We have revised our equation using $SMB_{ISM,RCM}$, $SMB_{ISM,AOGCM}$, $T_{ISM,RCM}$ and $T_{ISM,AOGCM}$ instead of the original notation $SMB_{ISM_{RCM}}$, $SMB_{ISM_{AOGCM}}$, $T_{ISM_{RCM}}$ and $T_{ISM_{AOGCM}}$.

page 15, line 4: here and elsewhere, e.g. equation (7), I suggest removing the x symbol.

Done: we have removed the x symbol in equation (7) and elsewhere.

page 17, line 15: what role does sliding due to subglacial hydrology play in these experiments? Here subglacial discharge primarily affects melting at the front, yet could also substantially affect sliding, which would be worth mentioning.

We thank the reviewer for pointing out that we did not discuss how surface runoff could affect basal sliding. As there is a current disagreement in the literature on the implications of this process on ice sheet evolution, ISMIP6 protocol does not include this process in our “standard experiments”. The dataset provided to the modelers taking part in the “open experiment” could be used to address this question. We revised the manuscript to mention 1) that surface runoff could enhance basal sliding and 2) that this forcing is not part of the standard approach but could be included in the open approach should modelers wish to do so. This revised text is placed in the experimental protocol overview (section 2) instead of the paragraph corresponding to the review’s comment, simply because the focus of section 5.2 is the oceanic forcing implementation.

page 18, line 21: I find the presentation of this conditional statement a bit odd. It is possible that this is due to the weird spacing, but my main thought is that it is not clear what the intent of the presentation. Possibly a table or flowchart describing the different retreat scenarios would be better?

We thank the reviewer for this comment, as indeed something happened in formatting of this conditional statement, and a similar comment applies for the `ice_shelf_collapse_mask` conditional statement (Section 6). We have removed the conditional statements and use sentences instead to describe the implementation.

5. Figure 9 and 10: these figures are extremely small and are a little difficult to read for that reason.

We have revised Figures 9 and 10.

6. page 20, section 6: how does the ongoing discussion of the ‘marine ice cliff instability’ play into these choices?

As there is still a lot of research to better understand the marine ice cliff instability hypothesis, including how and why ice shelves collapse, our experimental protocol focusses on hydrofracturing for ice shelf collapse instead. Our choice is motivated because it is a mechanism

for ice shelf collapse that has been observed, studied and implemented in ice sheet models. This allows for an assessment of the impact of this type of ice shelf collapse on a large variety of ice flow models. This assessment becomes even more important in the ongoing discussion of marine ice cliff instability as hydrofracture is thought to be a precursor to marine ice cliff instability. However, as we do acknowledge that hydrofracturing is only one possible mechanism that can explain ice shelf collapse, we modified the text to emphasize the motivation for this experiment, as well as how our experiments fits in the ongoing discussion of marine ice cliff instability.