Review Hager et al. 2023

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General comment:

In this paper, Hager et al. address a topic which is extremely relevant for the future sea-level contribution of the Greenland ice sheet (and as such, well within the scope of this journal): evaluating the ability of ISMIP6 ocean thermal forcing parameterizations to predict thermal forcing at tidewater glacier termini. This is accomplished through experiments with the MITgcm, using a set of idealized Greenland fjords and ocean boundary conditions, and parametrised subglacial discharge, glacier submaring melting (IcePlume package) and icebergs (IceBerg package). Sensitivity tests are designed by varying tidal amplitudes, subglacial discharge, iceberg coverage, and bathymetry. Incorporating and assessing the impact of iceberg melting in fjord simulations represents an important innovation, and the approach and methodology used by the authors is sound, although I think some additional clarifications and reorganization are needed in the Methodology section (see minor specific comments below). The authors indicate that the bathymetric control on the intrusion of Atlantic water into the fjords is the primary control on near-glacier thermal forcing, followed by iceberg submarine melting. It is found that grounding line thermal forcing varied by 2.9 ^{o}C across all simulations and is heavily dependent on the depth of bathymetric sills in relation to the Polar-Atlantic Water thermocline. The authors highlight that using a simple adjustment for fjord bathymetry, the ISMIP6 submarine melt implementation is able to predict grounding line thermal forcing within 0.2 ^{o}C. Finally, Hager et al. introduce new parameterizations accounting for icebergdriven cooling, which accurately predicted interior fjord thermal forcing profiles in both iceberg-laden simulations and observations from Ilulissat Icefjord. The results are presented in a very clear and structured way, and fully support the authors' conclusions, which are extremely relevant for the ice-sheet modelling community.

In view of this, I recommend this work for publication, and I only have some minor comments which are listed below.

We thank Dr. Petrini for reading our manuscript and providing a thorough and positive review, which we believe will be beneficial to the paper. We have provided responses to Dr. Petrini's concerns and have outlined how we will edit the manuscript accordingly.

Specific comments:

1) It would be good to have some additional text (either in the main text or in the supplementary) explaining the choice on the simulation length and output averaging choice (L96-99). From what I read in the text, I am left with two main questions: (1) why

water properties stop evolving after different amount of time in different simulations (2) as simulations are meant to represent a seasonal evolution, it is somehow strange to see they are extended up to 2.5 years. I don't expect this to be a major issue, but it would be nice to see an explanation.

Water properties stopped evolving after all fjord water had been flushed, which occurred from the surface down the water column. Sill depth was the primary control on flushing time, because shallower silled simulations had a larger volume of water below sill depth, which additionally renewed at a slower rate than water above the sill. Subglacial discharge and plume type (which controls entrainment and displacement of deep water in the plume), as well as tidal forcing, also influenced flushing time. As simulations evolved at different rates, it was important to run simulations to steady-state to ensure each simulation had fully responded to its unique forcing conditions and we were thus comparing apples to apples. Although in reality it is unlikely Greenland glacial fjords are ever at steady-state, this is a tacit assumption of the ISMIP6 parameterizations that we want to remain consistent to. We will add 1-2 sentences at Line 98 explaining 1) why steady-state is important to our simulations, and 2) why simulations reached steady-state at different rates.

The averaging of output over the last 10 days of our simulation was done to remove any influence of tides, internal waves, etc. from our results. As the runs are at steady-state, averaging over this time period will not impact our results other than to remove noise generated by tides. Again, we can elaborate on this point at Line 99.

It is a bit confusing to find the new parametrizations in Table 1 well before they are defined in the text. One simple solution could be to refer to the section where they are introduced in Table 1 (for instance: New Parametrizations (see section xxx);

Thank you for this suggestion – we will add "(see Section 4.2)" to the Table 1 caption

2) I think table C1 should belong to the main text, as it is extremely informative and widely referenced to. Moreover, in Subsection '2.1 Model setup', I found it not immediately easy to have a broad overview of the differences in each simulation. Including Table C1 in the main text would likely be enough, but also some simple text reorganization could be useful (for instance: the total number of simulations is provided only at the end in L134-135);

Thank you for making this point. Table C1 will be added to the Methods section of the main text. We will also add a new short paragraph at Line 99 that will provide an overview of the forcings used and the total number of runs before diving into the rest of the model setup.

Technical comments/suggestions:

L29: it could be good to specify/expand to what extent these processes are small scale (spatial and temporal) compared to those in global climate models (and ice-sheet models).

Thanks for the suggestion. This sentence will be changed to:

"However, such processes are too small scale (~1 m to ~1 km length scales at hourly to seasonal timescales) to be resolved in global climate models (grid resolution of ~30-60 km at annual timescales; e.g., Watanabe et al., 2010; Golaz et al., 2019)."

L30: Suggest splitting sentences, e.g., "To date, sea level projections have instead …".

This change will be made.

L31: Maybe 'simplified'?

This change will be made.

L32: Suggest 'that are large sources of uncertainty'. Also, 'future mean sea levels'.

These changes will be made in the next version of the manuscript.

L87 and elsewhere throughout the text: Suggest either adding South/North/West/East arrays in Fig. 1, or use different naming (e.g., along fjord, across fjord?) as it is not immediately clear where S/N/W/E are.

Thank you for the suggestion – a compass rose will be added to Figure 1a

L105: Maybe explain why significant tidal mixing was expecting, or add a citation?

The expectation of significant tidal mixing in shallow-silled glacial fjords is based on previous work by Hager et al. (2022) and Bao and Moffat (2023). We will add additional background with citations to this sentence, as well as add a sentence at line 169 describing why we are interested in sill-driven mixing.

L243-245: missing reference to Fig./table? Don't know where percentages come from

Thank you for pointing this out. Iceberg and submarine meltwater fluxes will be added to Table C1, which will now be Table 1 in the main text (see above).

L280 and formula 12: not sure if this explanation should be moved to the methods section, similarly as subsections 2.3, 2.4, 2.5.

The formula and surrounding text will be moved to the end of Section 2.2. For consistency, we will also introduce the use of root mean square errors in Section 2.2.

L319: perhaps something like 'its contribution to the variability of near-glacier...'?

Thanks for the suggestion - this change will be made in the next version of the text.

L372: Maybe better use 'Such an approach'? Same for later occurences.

These changes will be made in the next version of the text.

L462: 'ISMIP6 parametrizations'.

We think this should be kept as is because it is preceded by "neither".

Figure 2: I am confused by the presence of Qberg and Hberg shadings: what are they (Hberg is introduced only later in Fig. 4.), and are they cited in the text? It is ok to keep them, but at least an explanation in the legend is needed. Also, there is a typo in the inbox legend, purple line should read ISMIP6melt & AMmelt.

Qberg and Hberg are included to illustrate the extent of iceberg melting with depth, particularly in relation to sill depth and variability in upper water column. Qberg and Hberg are first introduced in Section 2.4 (Equation 6). Additional references to Figure 2 will be made at lines 224 and 236, and Lines 215-216 will be changed to "…with only minor variability occurring when iceberg keels extended below sill depth (see Q_{berg} and H_{berg} profiles in Figure 2) …" to draw specific attention to the Qberg and Hberg profiles.

We will also change the last sentence of the caption to "The vertical distribution of iceberg freshwater fluxes (Qberg) and heat fluxes (Hberg) are provided in a-c and d-f, respectively, to depict the depth of iceberg melt relative to sill depth and profile variability."

Thank you also for spotting the typo in the legend – this will be fixed in the revision.