Review for „Modelling the effect of submarine iceberg melting on glacier-adjacent water properties“ by Benjamin Davison and co-authors

The paper by Benjamin Davison and co-authors combines a general circulation ocean model (MITgcm) with a submarine iceberg melting module, in order to investigate the impact of iceberg melting and cooling, and their vertical distribution, on fjord water properties close to Arctic glaciers. The work is to my understanding an extension of the 2020 study by Davison et al. that was focused on a single Greenlandic tidewater glacier, by studying different likely iceberg "scenarios" and different simplified geometric fjord configurations in order to cover the wide range of Greenland fjord configurations. The work has potential implications also for efforts to project Greenland Ice Sheet behaviour with models, which usually can make use only of far-field properties beyond the fjord's mouth to force these ice sheet/ice shelf components, so the scientific relevance of the study is high; and the paper should, in my opinion, be published soon.

I think that the model setups are defined very elegantly in order to answer how iceberg melting affects glacier-adjacent water properties. While almost all simulations show a cooling in the upper 60m or so, below that level either warming or cooling can occur depending on the "icescape" and configuration.

Specifically, the paper implies that projections for the large fast-flowing Greenland glaciers that contribute most negatively to the mass balance are potentially affected by the lack of iceberg effects on fjord water properties (hosting numerous and large icebergs), and this is very clearly shown with simple model configurations. These "details" can potentially matter a lot for the "large-scale" mass balance of Greenland. Notably, the authors even provide a first idea for simple parameterizations (l.434-436) in their paper and I hope that these ideas will be picked up in the community promptly.

The last paragraph of section 4.1 attempts a comparison to observations with some success. Here, it would have been great to close the circle by saying more clearly (or even plotting in the same panels) which simple six model configurations can mirror panels a-f in Figure 8. Or in other words, which assumptions are needed to model profiles similar to the observed profiles (e.g. presence of a sill) with the simple fjord geometry used in the study.

The paper is written and organized excellently (with no obvious typos, which is rare), and the arguments are easy to follow. All results are clearly described and discussed and the conclusions are based entirely on the model results. The quality of the figures is also okay. Below, you can find a short list of line-by-line comments that the authors could still work on. I suggest to accept the paper with (very) minor revisions.

Thomas Rackow

We thank the reviewer for providing a thorough and supportive review of our manuscript. We agree with all of their comments and have implemented all of their suggestions in the revised version of the manuscript.

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Line-by-line comments:

l.89/90 Here or somewhere else, I think the high-impact study by Schaffer et al. (2020) should be mentioned who conclude that near-glacier sill-controlled ocean heat transport can play a crucial role for glacier stability.

Reference:

Schaffer, J., Kanzow, T., von Appen, WJ. et al. Bathymetry constrains ocean heat supply to Greenland's largest glacier tongue. Nat. Geosci. 13, 227–231 (2020). https://doi.org/10.1038/s41561-019-0529-x

Done.

l.94-109 What is the surface boundary condition at the atmosphere-ocean interface? I missed that somehow and it would be good to know whether this could influence the surface representation of the profiles (e.g. holding them close to some value).

The reviewer is correct to point out that the atmosphere-ocean boundary could influence the surface ocean conditions. In our simulations, we chose not to include any atmosphere-ocean interaction, so as to isolate the effect of icebergs on the fjord conditions. We mention this briefly on line 109 of the original manuscript (also line 109 in the revised manuscript): "…do not simulate the effects of sea ice, atmospheric forcing or tides". Although not realistic, this is in keeping with the approach of many other Greenland-focused fjord modelling studies (e.g. Cowton et al., 2015, 2016; Carroll et al., 2017) and allows us to more easily isolate the effect of icebergs melting on ocean conditions. We appreciate that some authors have chosen to include atmosphere-ocean interactions (e.g. Fraser et al., 2018), and that there may be interactions between the atmosphere-ocean interactions and that effect of iceberg melt on water properties that our simulations will not capture.

References:

Cowton, T., Slater, D., Sole, A., Goldberg, D., and Nienow, P. 2015. Modeling the impact of glacial runoff on fjord circulation and submarine melt rate using a new subgrid-scale parameterization for glacial plumes. *Journal of Geophysical Research: Oceans*, 120, 796-812. doi:10.1002/2014JC010324.

Cowton, T., Sole, A., Nienow, P., Slater, D., Wilton, D., and Hanna, E. Controls on the transport of oceanic heat to Kangerdlugssuaq Glacier, East Greenland. 2016. *Journal of Glaciology*. Doi: 10.1017/jog.2016.117

Carroll, D., Sutherland, D. A., Shroyer, E. L., Nash. J. D., Catania, G. A., and Stearns, L. A. 2017. Subglacial discharge-driven renewal of tidewater glacier fjords. *Journal of Geophysical Research: Oceans*, 122, 6611-6629. https://doi.org/10.1002/2017JC012962

Fraser, N. J., Inall, M. E., Magaldi, M. G., Haine, T. W. N., Jones, S. C. 2018. Wintertime fjord-shelf interaction and ice sheet melting in southeast Greenland. *Journal of Geophysical Research: Oceans*, 123, 9156-9166. https://doi.org/10.1029/2018JC014435

l.129/130 I think it would be good to also cite the much earlier Hellmer & Olbers (1989) study for the three-equation formulation, which is often forgotten:

Hellmer, H., and D. Olbers (1989), A two-dimensional model for the thermohaline circulation under an ice shelf, Antarct. Sci., 1, 325–336, doi:10.1017/S0954102089000490.

Agreed and done.

l.396-398 The constants are also from Jackson et al. 2020?

These constants for calculating the freezing point were taken from Cowton et al. (2015), which in turn were based on those originally presented in Holland and Jenkins (1999). The exact value for the constants used do vary slightly between publications, though many papers do not provide the values used. We now cite Cowton et al. (2015) on line 397 (in both the original and revised manuscript).

Holland, D. M. and Jenkins, A.: Modeling Thermodynamic Ice–Ocean Interactions at the Base of an Ice Shelf, J. Phys. Oceanogr., 29(8), 1787–1800, doi:10.1175/1520-0485(1999)029<1787:MTIOIA>2.0.CO;2, 1999.

Section 4.2: You tried to explore the relative change in submarine melt rate quantitatively, which is great. To my understanding, this is a simple diagnostic and the model does not see the different melt rates. I was wondering whether any feedbacks are to be expected, or whether your conclusions might be different in a model setup that would account for the iceberg-induced melt changes?

In our simulations, we used 'IcePlume' to simulate melting of the calving front. IcePlume does simulate submarine melting in areas distal to the runoff-driven plume, and these are affected by the inclusion of icebergs in the domain due to the iceberg-induced changes to the water column temperature. However, parameterisations of submarine melting in these regions is extremely uncertain (e.g. Jackson et al., 2020), which is why we chose to use the relative method of Jackon et al. (2014). The relative method does assume that all changes in temperature affect submarine melt rates (i.e. if an increase in temperature increases heat supply to the glacier face, it assumes that all of that heat supply is used in submarine melting). In reality, this is likely an upper-bound on the effect of temperature changes on glacier submarine melt rates – we have modified the wording in the revised manuscript to reflect this: "It is worth noting that changes in melt rate calculated using this method assume that all changes in heat supply are accommodated by changes in submarine melt rates, and so this method provides an indication of the maximum relative changes in submarine melt rates expected due to changes in ambient ocean temperature" (line 398-401 in the revised manuscript)

We don't expect there to be strong feedbacks between glacier melting and fjord circulation associated with iceberg-induced changes to the glacier submarine melt profile. This is because the additional volume flux of meltwater from those portions of the glacier experiencing accelerated submarine melting is small in comparison to that provided from runoff and iceberg melting. For example, glacier submarine melt rates in these regions are thought to be around 0.5 metres per day. There's uncertainty in these values, so let's suppose a maximum value of 3 metres per day. We find, at most, a 60% increase in melt rates in the 100-200 m depth range, which equates to a volume flux of ~10 $m^3 s^{-1}$ when distributed over a 5 km-wide ice wall. The water motion driven by this freshwater flux might act to slightly increase melt rates higher in the water column, but we suspect that more powerful currents driven by plumes, iceberg melting and tides, for example, would make a positive feedback unlikely. Put more succinctly: although the melt rates of the glacier face are similar to that of the icebergs, the submerged area of the calving front is a small fraction of the submerged iceberg area, so the impact on the fjord circulation due to small changes in glacier submarine melt rates is likely to be proportionally small.