Impact of icebergs on the seasonal submarine melt of Sermeq Kujalleq
Karita Kajanto et al.
RC2: 'Comment on tc-2022-136', Anonymous Referee 2

Summary

This manuscript presents a study into the effects of icebergs on the circulation and water properties within Ilulissat Isfjord, a major ice-choked fjord in western Greenland. The study utilises the recent 'IceBerg' package developed for MITgcm (Davison et al 2020, 2021) to simulate the evolution of the fjord with and without icebergs, with the results then compared to (sparse) available observations. It builds upon the two earlier Davison papers in its application of the model to a new fjord system, and one of particular significance to this subject due to its exceptionally high ice concentration and proximity to Greenland's largest outlet glacier.

The manuscript makes a useful contribution to the growing literature on Greenland's fjords, clearly demonstrating the potential for icebergs to strongly modify fjord processes, and elucidating some of the mechanisms through which this can occur. I have one major comment which related to the experimental design which needs to be addressed to allow clear and confident interpretation of the presented results. Beyond this, I have some specific questions on aspects of the model set up and quite a long list of further comments, questions and points of clarity. Finally, I have attached a separate PDF with typological issues highlighted.

- Thank you so much for your time and effort and the thoughtful comments that have greatly improved the manuscript.

Major comments

Experimental design

The experiments are run from initiation in March through to August, at which point modelled fjord conditions are compared with observations. While this shows the transition from winter through to summer conditions, terminating the experiments in August seems premature and makes it difficult to assess the modelled evolution of the fjord. In particular, it is not clear how fjord water properties would continue to evolve beyond high summer, and if and how they return to something like the initial conditions in time to undertake this evolution again.

The fjord undergoes freshening and cooling over the duration of the model run, reaching something approximately resembling the observations by the end of the run in August. It's not clear though whether the August temperatures are the end of the journey (with the fjord existing in a new quasi equilibrium state similar to the experiments by Davison et al 2022), or whether the modelled fjord would actually continue to cool and freshen into the autumn and winter if the model was allowed to run on. If it's the latter, this implies the fjord is periodically returned to something resembling the original conditions by some other mechanism, only for the icebergs to resume the cooling process.

- Originally, we did not include the latter part of the runoff season, due to a lack of observations to compare the model with, and the general uncertainty relating to the autumn/winter season. We do see your point in running the model longer as a modelling exercise, and now include the latter part of the runoff season. The results demonstrate how density-driven inflow over the sill becomes dominant as subglacial discharge decreases. This behaviour is similar to the observational findings from LeConte glacier in Alaska (Hager et al., 2022), providing credibility to the model results. The results from the late season are included throughout Sections 4 and 5 and are presented in Figs. 3–6.
- We did run the model forward through a second winter season (Fig. S2), and we do see that the model starts to recover towards the original density during the winter, not quite reaching it, and temperature not recovering. While we feel that the model captures the general features of wintertime forcing and provides general understanding of the circulation regimes of the system, the model is not designed to include processes that might become relevant during winter, such as storm surges, sea ice formation and iceberg volume decrease. Therefore, we do not think that the validity of our results for the runoff season is impacted by a discrepancy between the intial state and the following winter.
- We performed an additional wintertime test (presented in Fig. 1 at the end of this document), where the iceberg distribution switches to *IB200* mid-winter, to simulate melting of deepest icebergs (Enderlin et al. (2016) show the decrease in iceberg draft during winter). The switch triggers temperature increase, since in the absence of deep icebergs no more IMAW is formed, and the basin gets filled with umodified Disko Bay water at sill depth. This test highlights the need to include the decrease of iceberg volume during winter.

I think this is important for at least two reasons. Firstly, it is has implications for the validity of the comparison of model results with August observations. If it is necessary for the fjord to start off warmer / saltier at the start of the melt season in order to approximately match observations by August, then the difference between the applied initial conditions and the summer observations becomes critical in determining whether the model matches the observations. As the winter profile (initial conditions) comes from 2018, it's not clear whether this does represent an appropriate starting point relative to the summer observations (which come from 2014). There is also no evidence provided to show that the assumption that winter conditions inside the fjord simply match shelf water properties at sill depth is appropriate. If water properties inside the fjord were already cooler/fresher than those at sill depth at the start of the melt season, would they end up even cooler/fresher by August (in which case the mismatch with observations will increase)?

- We base our summer boundary condition to a single observed profile from August 2014, which is not necessarily a good representation of the densest inflowing water. Since we compare to observed profiles within the fjord, we need to make sure that it is theoretically possible for the model to reach the observed properties. This is why we adjust the properties at the sill. The winter observations from 2018 extend only to 150 m depth, below which we join the simulated winter profile with the summer one. Thus, at sill depth the properties are the same, as are the adjustments we make, leading to no difference between winter and summer at sill depth. We have revised the description of the boundary condition on lines 141–150.
- The general understanding originating from Gladish et al. (2015) is that the fjord basin is filled with Disko Bay water at sill depth, with no significant renewal of water otherwise. We acknowledge that this study, as all other studies of this fjord, is from summer, but we have used this assumption as a starting point. This is explained in lines 82–85. We are not aware of any winter observations extending down to the deep basin from this fjord.
- As for running a spinup for over one year, unfortunately this is not practically possible with IceBerg package due to the very long computation time. In addition, a spinup extending over winter would need to address the variability of the iceberg distribution, as discussed in our earlier reply regarding the following winter. However, we see a dedicated winter model including relevant processes as an important future study.

Secondly, it raises interesting questions about the key processes and the seasonality of fjord water properties (which is a key aspect of the paper and the focus of the title). In the way the experiments are set up, you are implicitly assuming that there is an annual cycle of winter warming and summer cooling within the fjord. It's not clear though from the evidence presented whether this is correct or whether given sufficient time a relatively consistent offset between shelf and fjord temperature/salinity would be established.

- We are not assuming any seasonality below sill depth, as properties at sill depth (grid cell where the densest water is able to enter the basin) are kept constant throughout the run. The seasonality of the boundary condition only applies above sill depth, and due to smaller density, these waters do not enter the deep basin. Thus, all changes in the deep basin properties presented in the results are due to other processes than changes in the boundary condition. We have revised description of the boundary conditions to make this more clear in lines 141–150.

Without further investigation / discussion of these points, it's difficult to assess the validity of the seasonal evolution of water properties, which is a key aspect of the study as it is currently presented. This could be partially addressed by running the experiments at least until the end of the melt season (which is I think fairly conventional for studies of seasonal processes) so that we can at least assess whether cooling and freshening continues beyond the August observations. I think though that at least one scenario should be run for >1 year to allow for a proper spin up, reduce the dependency on the initial conditions and allow you to look into the questions of seasonal cyclicity raised above. If results diverge from those presented for the first melt season, then this raises an interesting discussion over whether other processes are serving to balance the impact of icebergs over this timescale. Finally, you could also run the model with a few steady discharge values to see what equilibrium conditions are eventually reached (similar to Davison et al, 2022). This would help to reduce the temporal dependency in the results, and allow you to assess what the impact of a given discharge and iceberg concentration would be on the fjord in the longer term. This would help to assess how far down this path the fjord has progressed on the timescale of the experiments presented here.

- We have revised the Results section, now including also the late season, included in Figs. 3–6, and also present the results from following winter in the Supplementary Figures S2 and S3.

Model details

There are a few points in the paper where the description of model function differs from that in the original paper describing the IceBerg package by Davison et al. (2022). This raises questions over whether the model is being accurately described (or whether it has been modified), and if the results accurately interpreted.

- Thank you for the comment. We have not changed the IceBerg package, nor has it been our intention to describe it differently. In the revised manuscript we have improved our description of the model, please see our replies to the specific comments below.

L125-126. This sentence describes how ice adjacent currents are calculated, but it doesn't explain the representation of iceberg drift in the IceBerg package. According to Davison et al (2020, p10) 'Melt rates derived using the velocity-dependent three-equation formulation are sensitive to the current velocity at the ice-ocean interface. For icebergs, this is the difference between an iceberg's drift velocity and the ambient water velocity at any given point on the iceberg. In ice mélange—a dense matrix of icebergs and sea ice often found adjacent to large tidewater glaciers—iceberg motion is typically slow relative to the surrounding currents; therefore, in this region of our domain, we assume the icebergs are fixed in place. Elsewhere in the domain, we calculate iceberg drift velocity as the average water velocity from the fjord surface to the iceberg keel depth (but we do not use this to update the location of each iceberg) ... We calculate the submarine melt rate of every face on each iceberg individually at each model vertical level using ice-parallel current speeds (relative to the calculated drift of the iceberg).' This raises the question of whether or not drift is included in the present simulations? For a fjord like Ilulissat Isfjord, where dense melange is widespread, it may be more appropriate not to include drift?

- Thank you for pointing this out, we have now corrected the sentence, please see lines 156–159. We have not changed the code, and are including the effect of drift. As pointed out in this comment, there are arguments to both including and excluding drift. Since in 2014 there was relatively short and scattered episodes of rigid melange and none during the melt season (Joughin et al., 2020), we assume that icebergs were relatively mobile this season, and thus we see it more appropriate to include drift that to exclude it. Furthermore, Fig. S3 demonstrates that water velocity is slow within the extent of most icebergs due to the deep GMW, causing the drift speed to be small (0.02–0.1 ms^{-1}), which is comparable to the assumed background velocity of 0.06 ms^{-1}.

L152-154. Unless it has been edited, I think the plume in IcePlume is programmed to terminate upon reaching neutral buoyancy rather than zero momentum (see Cowton et al, 2015).

- Thank you for pointing this out. There was a mistake in our diagnostics calculation, which has now been corrected and we use the outflow from the plume to the model grid as an indicator for the neutral buoyancy depth. This does not change the results nor the interpretation.

L398-399. This line states that icebergs do not present an obstacle to flow in the model, but according to Davison et al (2020, p10), 'As well as drifting with ocean currents, icebergs also act as a barrier to water flow. We represent this effect using partial cells within MITgcm—essentially forcing a portion of some of the cells to be 'dry'. The fraction of the cell that is dry is equivalent to the proportion of the cell volume occupied by icebergs. In this way, the blocking effect of all of the icebergs in a cell is represented using a single value, rather than representing individual icebergs as solid bodies within grid cells.' So unless the package has been modified, it seems the icebergs should be exerting a physical obstacle to flow in these simulations (albeit a simplified one based on a cell averaged approach)? This seems to be noted on L127-128.

- Thank you for pointing this out. We have not modified the code of the IceBerg package. As described in the attached citation, the IceBerg package does accoung for the volume that icebergs uptake. However, we wish to point out to the reader that this approach does not fully encompass changes to flow due to an iceberg distribution, since it does not force the flow to change direction, as in Hughes et.al., 2021. While we do believe that the approach of the IceBerg package is sufficient for our type of study and the results from Hughes et.al., 2021 indicate that a single obstacle could be a reasonable representation, we wish to point out to the reader the complexity of flow within an iceberg network. We have revised the formulation on line 487–488.

Other specific comments

Introduction

L18. In what sense are the glaciers 'controlled' by the geometry and stratification?

- Thank you for the comment, this point is now removed from the revised introduction, since it is not necessary to our study.

L19-22. I feel this would benefit from a little elaboration. How does the ice-ocean interface create uncertainty in sea level contribution predictions?

- Thank you for the comment, we have revised the text, lines 20–22.

L26-7. Be more specific - do you mean that calving is reduced when dense ice melange is present?

- Thank you for the comment, the Introduction is revised with reference to comments from Reviewer 1, and now includes a better description. Please see lines 40–44.

Methods

L80. Why not include Coriolis force? This is discussed later, but should be justified here.

- Thank you for the comment, text edited, line 101.

L81-82. Why not vary the runoff smoothly by interpolating between monthly values? I think this is the default set up for MITgcm, and would prevent unrealistic step changes.

- Thank you for noticing, that is indeed what we do, we have revised the text.

L92-93. I don't quite follow: if water is draining through the shear margins, wouldn't this suggest drainage close to the lateral margins rather than across the full width of the glacier?

- Thank you for the comments regarding plume width. We have rearranged the text to better convey our justification, see lines 104–125. The choice of a 1.2 km plume is a middle-ground assumption for the entire runoff season. We expect the degree of channeling to vary within the season (Cook et al., 2021), and potentially outlets to form also close to the shear margins (Cavanagh et al., 2017). We assume that over the season, discharge is somewhat distributed on average, and our choice of a wider plume width represents this.

L94. Why 1.2 km? I appreciate plume width is hard to constrain but why this value in particular? It seems very wide compared to the sort of values that are normally used (e.g. a recommendation of 200 m by Jackson et al 2017). Without further justification, it gives the impression it was chosen to give the best fit to observations – if this is the case, it should be stated.

- Thank you for the comment, please see the reply above.

L99. As above - a 'narrow' plume of 400 m wide is still wide by conventional standards (e.g. Jackson et al 2017, Slater et al 2022).

- Thank you for the comment. Since the grid resolution in our model is 400m in the y-direction, we are restricted by this resolution regarding the narrowest plume.

L102-103. It's unlikely that subglacial discharge at a glacier like Jakobshavn is 0 in the winter months - it will be a lot smaller than summer but with such a large catchment and such high sliding velocities there will likely be a non-neglible winter discharge of subglacially-derived meltwater. This would affect the result that in the NoIBP scenario there is no circulation in the deep basin before May. It's very hard to quantify subglacial melt rates, but it might be worth trying using a discharge of a few m3/s in the winter to see if this has a noticeable effect on results.

- Thank you for the comment. It is indeed true that there is very likely some small amount discharge during winter in this glacier. We have now noted this on lines 129–130, and also in the Discussion on lines 475–477. As you point out, the difference would in this case be similar to the difference from April to May in our results, which is a relatively small difference and has little implications to our results.

L102-103. What is the justification for such a large lag time? In a pressurised drainage system, there should be almost no lag between input to the system (i.e. surface runoff) and output from the system (i.e. subglacial discharge). There will be some subglacial storage which will serve to smooth the peaks, but this wouldn't cause the peak to be displaced by several weeks. For example, Mankoff et al. (2020) assume instantaneous routing between runoff and outlet discharge, and find good agreement in the timing of discharge peaks with observations (with a 7 day smoothing applied).

- Thank you for the comment. We point out that our subglacial discharge forcing is synthetic, and we chose to keep the forcing as simple as possible. Since we assume a gaussian temporal distribution, and assume May to November to be the runoff season, it follows that the peak will be in August. We have revised the text to point out that the forcing is synthetic, lines 128–129.

L103. What is the peak discharge of 1200 m3/s based on?

- Thank you for the comment. The peak is and estimate from (Enderlin et al., 2016), as cited in the text.

L114-116. Give the years and dates of these data here – presently this is only stated in the SI.

- Thank you for the comment. We have revised the description of the boundary condition based on comments from both reviewers, now including also the years, lines 141–150.

L117-118. I don't follow - why modify the forcing in this way (and at what point in the seasonal cycle were these values obtained?)?

- Thank you for the comment. We base our boundary condition to a single observed profile, that is not necessarily a good representation of the densest inflowing water. Since we inted to compare to observed profiles within the fjord, we need to make sure that it is theoretically possible for the model to reach the observed properties. The observed profiles both in and outside of the fjord are from 15th to the 16th of August, 2014 (Beaird et al., 2017). We have revised the description of the boundary condition on lines 141–150.

L124. The description that 'Melt and negative salinity flux are computed' seems odd. What about heat? Should it state that melt rates, and thus salinity and heat fluxes, are calculated?

- Thank you for the comment, we have revised the text, lines 154–155.

Results

L148. See earlier comment regarding winter discharge.

- Thank you for the comment, please see our earlier reply regarding winter discharge.

L163-4. This is the case in the model (melt in areas of low current velocity is poorly constrained in iceplume), but there needs to be a bit more comment on whether or not this is deemed realistic – recent research suggests there should be much less discrepancy between melt rates within and outside of the plume area (Jackson et al. 2020; Sutherland et al. 2019).

- Thank you for the comment, we point this out in the Discussion, lines 498-500.

L165-7. I don't follow. The deep basin starts off colder than the shelf (due to the initial conditions) but seems to be steadily warming through the summer due to inflow over the sill (shown in Figure 3/4)?

- Thank you for the comment. The deep basin is initialized with ambient water properties at sill depth since that is the densest water accessing the basin (see Gladish et al. (2015)). Early-season refluxing of GMW causes the basin water to cool by up to 0.2 degC during the summer, compared to the initial basin properties, as best seen in Fig. 8a.

L167-8. Ambiguous - do you mean it is 2 C cooler than the equivalent Disko Bay temperature?

- Thank you for the comment, we have reformulated the sentence, line 199.

L177-8. Keep in mind this is likely underestimated due to poorly resolved boundary processes (see earlier comment)

- Thank you for the comment, please see our earlier reply.

L176-6. This doesn't sound very likely - even the plume melt rate in figure 5 only reaches 4.5 m/d in August (Figure 5). Is this a mistake, and if not where is it shown?

- Thank you for the comment, sorry this was freshwater flux, not melt rate, lines 219–220.

L201. See earlier question on 'drift'. And if drift is turned on, how do you distinguish between 'drift induced' melt and melt due to the flow of water past the berg? (Given that the net flow velocity past the berg is the difference between the drift velocity and the current speeds at any given depth).

- Thank you for the comment, we mean velocity-induced. However, this statement no longer appears due to a comment from Reviewer1.

L210. It's also notable that they result in a much larger export of freshwater and GMW from the fjord.

- Thank you for the comment, this is true, however, not relevant to our main conclusions.

L218-9. How is the GMW outflow identified in the observations? Is this based purely on the T-S properties?

- Thank you for the comment, yes the interpretation is based on the CTD-data only, see Beaird et al. (2017) for further analysis. We have revised the paragraph describing comparison to observations, lines 277–287.

L220-2. Again, how is this determined? This section would benefit from a little elaboration.

- Thank you for the comment, we have revised the comparison to observations, lines 277–287.

L237. Does 'entrainment of GMW' here just mean entrainment of outflowing GMW into shelf water flowing inwards over the sill, or does it also include recirculation of GMW where the plume termination depth is deeper than sill depth? (The latter being perhaps a slightly different thing to 'entrainment').

- Thank you for the comment, we have revised this paragraph based on comments from Reviewer1, lines 289–300.

Discussion

L268-9. The fjord in the IBP scenarios gets steadily cooler and fresher below 200m over the course of the summer (Figure 8). Does this trend continue if the simulation is allowed to run on for longer (into autumn and winter), such that model results and observations continue to diverge, or is a new equilibrium reached? See earlier major comment.

- Thank you for the comment, please see our reply to the major comment and the new late-season results.

L271-2. How is this quantified?

- Thank you for the comment. This is achieved by comparing the mid-fjord profiles of both *IBP* and *NoIBP* to the summer Disko Bay boundary condition.

L275-8. Muilwijk et al (2022) show that subglacial discharge represents a small fraction of GMW, but that upwelling of AW by the plume is very important in the formation of GMW. The role of plumes doesn't seem to be properly captured by this sentence.

- Thank you for the comment, we are discussing here the small contribution of suglacial discharge to the cooling, while we do not expect upwelled AW to contribute to cooling.

L278-9. Could this be tested based on whether the difference between the modelled and observed properties sits on a melt or runoff mixing line?

- Thank you for the comment. Outside the range of Fig. 7 the observed profiles do turn to align with runoff eventually, but they do also have a significant contribution of melt close to the surface. So, there is for sure a runoff-contribution, but some contribution from small icebergs is also likely, as stated in the text, lines 344–347.

L294-5. It would be valuable to compare other aspects of the results to Davison et al (2022) as well, given the similarity in these studies. Davison et al used an idealised domain to investigate the impact of icebergs across a parameter space representing the diversity of Greenland's fjords. As Ilulissat Isfjord represents one end member of this range, it would be valuable to examine how closely it aligns with the predictions of Davison et al.

- Thank you for the comment. See comparison on lines 372–388.

L299-300. Jackson et al. (2017) propose that a line plume of 200 m width gives best agreement with their observations – this is much narrower than the tested range of 400-4000 m, so it doesn't really justify the choice of plume widths used in this study (see earlier comment).

- Thank you for the comment. Please see our reply to the plume width earlier. We have reformulated the discussion so that this reference no longer appears here.

L331-2. Should qualify that this is true over the parameter space considered

- Thank you for the comment, we have included the fluxes, line 407.

L340-2. I don't follow - wouldn't the change in geometry due to undercutting make icebergs more likely to rotate top-first into the fjord?

- Thank you for the comment, this sentence no longer appears in the revised formulation of the Discussion.

L353-5. I feel this needs some substantiation. The currents in question are a summer phenomenon, whereas rigid sea ice format occurs towards the end of winter. Would a weaker plume during one summer really affect sea ice formation the following winter / spring? I'm not saying it's impossible, but it seems highly speculative and would benefit from stronger justification.

- Thank you for the comment, we have reformulated this part of the discussion, also in reference to comments from Reviewer1, see lines 418–433.

L357. Need to be more specific with terminology, to make clear you are talking about recirculation of deep waters rather than some other form of entrainment. Same for 'iceberg modification'.

- Thank you for the comment, we have reformulated the discussion so that this statement no longer appears here.

L361-3. This is broad statement which doesn't really do justice to the rich literature on calving, including on the impact of undercutting on calving (e.g. O'Leary and Christoffersen 2013; Ma and Bassis 2019; Benn et al. 2017; Slater et al. 2021).

- Thank you for the comment, we have reformulated this paragraph, please see lines 409–419.

L377-382. The comparison of model results and observations hinges on implications of the experimental design, and may need to be reconsidered (see earlier major comment).

- Thank you for the comment, we refer to our earlier reply regarding the experimental design.

L381. It is hard to compare these two plots. Could additional curves (or even additional plots) be added to Figure 8 to allow comparison? (Also, there is no Figure 3m).

- Thank you for the comment, we have considered adding isopycnals to the figure, however, this led to more confusion than excluding them.

L385. Why not vary this smoothly (see earlier comment)?

- Thank you for the comment, we refer to our earlier reply.

L392. As earlier, is this 'drift induced', or is it water flowing past stationary grounded icebergs?

- Thank you for the comment, this paragraph is reformulated and this statement no longer appears.

L392-4. I'm not sure I follow this. I would assume that freshening causes upwelling and outflow of GMW, and that it is entrainment into this flow that drives subsurface inflow to the fjord (as well as entrainment into the main plume)? In which case it's not obvious to me why the rate of entrainment/inflow should be greater due to the negative salinity approach. I can see that if you used a real freshwater flux from the melting icebergs this would increase the outflow, such that there was a net outflow from the fjord (equal to the meltwater flux), but it's less obvious to me why this would reduce the inflow to the fjord in absolute terms. If you used a real freshwater flux, there would also be a question over whether it is appropriate to add a physical volume of meltwater whilst not simultaneously decreasing the volume of the fjord occupied by icebergs, as the two should approximately balance each other out.

- Thank you for the comment, this is indeed what we intend to discuss. We want to point out that in reality there should be a net outflow of iceberg meltwater over the sill, and in this context the validity of the negative salinity approach reqires further study. We have revised the formulation, lines 480–482.

L406-7. Given that the results presented already over-estimate cooling and freshening, this raises further questions over why the impact of icebergs in the model seems to overestimate the impacts of icebergs, and should probably be referenced in this context.

- Thank you for the comment, we have expanded the discussion on deep basin cooling and freshening, lines 457–469.

L410-1. Have 'misleading interpretations' been presented in the paper? If not, perhaps better to simply state that it's important to take icebergs into account when studying and simulating these systems.

- Thank you for the comment. We have presented the diverging interpretations of deep basin water modification by Gladish et al. (2015) and Beaird et al. (2017) on lines 366–371, and explained that this difference is due to the lack of icebergs in (Gladish et al., 2015), and also due to the missing concept of IMAW as separate from GMW.

L416. 'Entrainment' is unspecific – clarify the process in question.

- Thank you for the comment, this sentence no longer appears in the conclusions.

L416-8. This sentence is hard to follow. Also the influence of discharge on the plume and frontal melt rates has been demonstrated in many other places (more so that here, where it isn't really the focus of the paper).

- Thank you for the comment, we have reformulated the sentence, lines 508–509.

L419. Need to be specific about the mechanism here - is it purely due to changes in the stratification?

- Thank you for the comment, we have reformulated the sentence, lines 509–513.

L421. This seems to be overstating things given the speculative nature of this connection. A 'potential link' would seem more appropriate.

- Thank you for the comment, we have reformulated the sentence, lines 511–513.

See annotated PDF for further minor corrections.

Benn, Douglas I, Jan Åström, Thomas Zwinger, Joe Todd, Faezeh M Nick, Susan Cook, Nicholas RJ Hulton, and Adrian Luckman. 2017. 'Melt-under-cutting and buoyancy-driven calving from tidewater glaciers: new insights from discrete element and continuum model simulations', Journal of Glaciology, 63: 691-702.

Davison, Benjamin Joseph, Tom Cowton, Andrew Sole, Finlo Cottier, and Pete Nienow. 2022. 'Modelling the effect of submarine iceberg melting on glacier-adjacent water properties', The Cryosphere, 16: 1181-96.

Jackson, R. H., E. L. Shroyer, J. D. Nash, D. A. Sutherland, D. Carroll, M. J. Fried, G. A. Catania, T. C. Bartholomaus, and L. A. Stearns. 2017. 'Near-glacier surveying of a subglacial discharge plume: implications for plume parameterizations', Geophysical Research Letters, 43: 6886-94.

Jackson, RH, JD Nash, C Kienholz, DA Sutherland, JM Amundson, RJ Motyka, D Winters, E Skyllingstad, and EC Pettit. 2020. 'Meltwater intrusions reveal mechanisms for rapid submarine melt at a tidewater glacier', Geophysical Research Letters, 47: e2019GL085335.

Ma, Yue, and Jeremy N Bassis. 2019. 'The Effect of Submarine Melting on Calving From Marine Terminating Glaciers', Journal of Geophysical Research: Earth Surface, 124: 334-46.

Mankoff, K. D., B. Noël, X. Fettweis, A. P. Ahlstrøm, W. Colgan, K. Kondo, K. Langley, S. Sugiyama, D. van As, and R. S. Fausto. 2020. 'Greenland liquid water discharge from 1958 through 2019', Earth Syst. Sci. Data, 12: 2811-41.

Muilwijk, Morven, Fiamma Straneo, Donald A Slater, Lars H Smedsrud, James Holte, Michael Wood, Camilla S Andresen, and Ben Harden. 2022. 'Export of ice sheet meltwater from Upernavik Fjord, West Greenland', Journal of Physical Oceanography, 52: 363-82.

O'Leary, M., and P. Christoffersen. 2013. 'Calving on tidewater glaciers amplified by submarine frontal melting', The Cryosphere, 7: 119-28.

Slater, DA, DI Benn, TR Cowton, JN Bassis, and JA Todd. 2021. 'Calving multiplier effect controlled by melt undercut geometry', Journal of Geophysical Research: Earth Surface, 126: e2021JF006191.

Sutherland, DA, Rebecca H Jackson, Christian Kienholz, Jason M Amundson, WP Dryer, Dan Duncan, EF Eidam, RJ Motyka, and JD Nash. 2019. 'Direct observations of submarine melt and subsurface geometry at a tidewater glacier', Science, 365: 369-74.

References

Beaird, N., Straneo, F., and Jenkins, W.: Characteristics of meltwater export from Jakobshavn Isbræ and Ilulissat Icefjord, Annals of Glaciology, 58, 107–117, https://doi.org/10.1017/aog.2017.19, 2017.

Cavanagh, J. P., Lampkin, D. J., and Moon, T.: Seasonal Variability in Regional Ice Flow Due to Meltwater Injection Into the Shear Margins of Jakobshavn Isbræ, Journal of Geophysical Research: Earth Surface, 122, 2488–2505, https://doi.org/10.1002/2016JF004187, 2017.

Cook, S. J., Christoffersen, P., and Todd, J.: A fully-coupled 3D model of a large Greenlandic outlet glacier with evolving subglacial hydrology, frontal plume melting and calving, Journal of Glaciology, pp. 1–17, https://doi.org/10.1017/jog.2021.109, 2021.

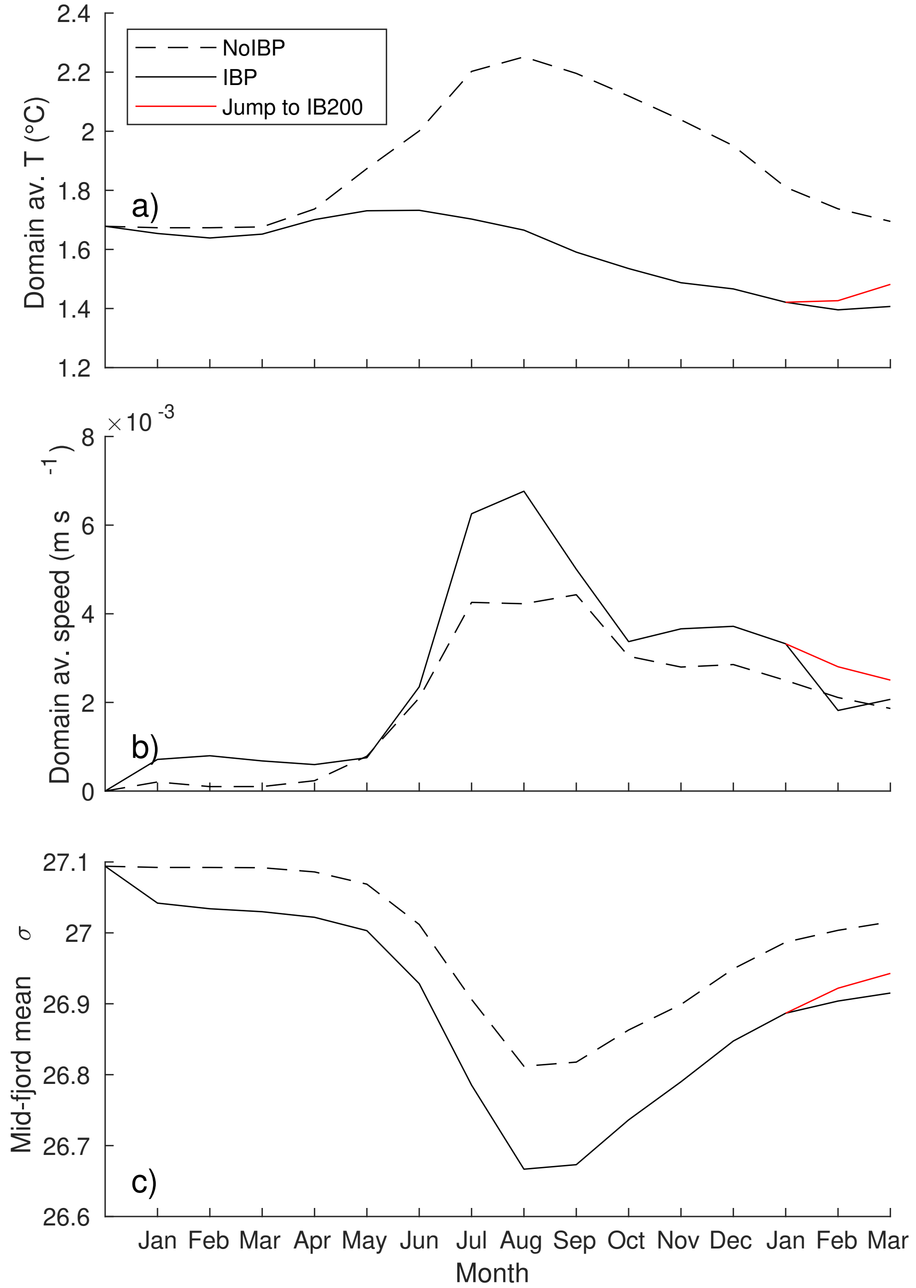

Figure 1: Time evolution throughout the model run of a) domain averaged temperature, b) domain averaged speed and c) vertically averaged potential density at mid-fjord location for experiments with and without icebergs (*IBP* and *NoIBP* respectively). Red line indicates the response to mid-winter change of iceberg distribution of *IB200*, to test the response of the model to shallowing of icebergs due to submarine melt during winter.

Enderlin, E. M., Hamilton, G. S., Straneo, F., and Sutherland, D. A.: Iceberg meltwater fluxes dominate the freshwater budget in Greenland's iceberg-congested glacial fjords, Geophysical Research Letters, 43, 11,287–11,294, https://doi.org/10.1002/2016GL070718, 2016.

Gladish, C. V., Holland, D. M., Rosing-Asvid, A., Behrens, J. W., and Boje, J.: Oceanic boundary conditions for Jakobshavn Glacier. Part I: Variability and renewal of Ilulissat Icefjord waters, 2001-14, Journal of Physical Oceanography, 45, 3–32, https://doi.org/10.1175/JPO-D-14-0044.1, 2015.

Hager, A. O., Sutherland, D. A., Amundson, J. M., Jackson, R. H., Kienholz, C., Motyka, R. J., and Nash, J. D.: Subglacial Discharge Reflux and Buoyancy Forcing Drive Seasonality in a Silled Glacial Fjord, Journal of Geophysical Research: Oceans, 127, e2021JC018 355, https://doi.org/10.1029/2021JC018355, 2022.

Joughin, I., E. Shean, D., E. Smith, B., and Floricioiu, D.: A decade of variability on Jakobshavn Isbræ: Ocean temperatures pace speed through influence on mélange rigidity, Cryosphere, 14, 211–227, https://doi.org/10.5194/TC-14-211-2020, 2020.