The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC2
https://doi.org/10.5194/tc-2022-136-RC2, 2022

Comment on tc-2022-136

Anonymous Referee #2

Referee comment on "Impact of icebergs on the seasonal submarine melt of Sermeq Kujalleq" by Karita Kajanto et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-136-RC2, 2022

Review of manuscript Impact of icebergs on the seasonal submarine melt of Sermeq

Kujalleq, by Kajanto et al

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.

Major comments

1) 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.

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)?

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.

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.

2) 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.

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?

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).

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.

Other specific comments:

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

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

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

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

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.

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?

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.

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).

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 m^3/s in the winter to see if this has a noticeable effect on 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).

L103. What is the peak discharge of 1200 m^3/s based on?

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

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?)?

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?

L148. See earlier comment 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).

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)?

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

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

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?

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).

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

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

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

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').

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.

L271-2. How is this quantified?

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.

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?

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.

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).

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

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?

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.

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'.

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).

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).

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).

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

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

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.

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.

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.

L416. ‘Entrainment’ is unspecific – clarify the process in question.

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).

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

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

See annotated PDF for further minor corrections.

References

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.

Please also note the supplement to this comment:
https://tc.copernicus.org/preprints/tc-2022-136/tc-2022-136-RC2-supplement.pdf