Review of “Modelling ocean melt of ice melange at Greenland’s marine-terminating glaciers”

Jain et al.

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Assessment: Major revision

Reviewed by: Erwin Lambert

The authors have configured an idealised model setup for a Greenland fjord filled with melange. Based on several backed up constraints, they have created a distribution of icebergs and added these to their MITgcm setup. They have analysed the impact of melange melt on the fjord circulation and heat distribution, and the sensitivity thereof to temperature forcing, subglacial melt, and melange thickness. They have expressed the sensitivity to temperature forcing and subglacial melt in a parameterisation, which may apply to melange-filled fjord systems more generally.

The study is very relevant as the community is actively developing methods to represent climate forcing of the Greenland ice sheet. The ocean-driven melt of melange, through buttressing, is an important aspect of this. The study is generally well-designed with an extensive sensitivity analysis and a thoroughly constructed iceberg distribution. The paper overall was a pleasure to read and the figures clearly convey their message.

I do have several concerns with the current manuscript and therefore recommend a major revision before publication. The major concerns are threefold:

1) The choice of iceberg areal fraction lambda
    a. The authors back up their choice of 0.6 in lines 148-154. However, I believe a mistake was made in their calculation, and a maximum iceberg-to-sea ice ratio of 0.35 should imply a maximum total iceberg fraction of 0.26. A ‘best estimate’ between 0 and 0.26 should then be 0.13, a factor four smaller than what the authors used. I think the authors should reconsider their choice of lambda. If it is unfeasible to redo the simulations with a lower value, they should extensively discuss the impact of this choice on their results and conclusions, ideally backed up with a sensitivity run.
    b. I was surprised not to see lambda back in the Discussion. The authors discuss various sources of uncertainty, without picking up this one. I think this deserves some additional discussion.

   c. Following on that, the authors should reconsider how valuable their comparison to observations of total subglacial meltwater fluxes (Sec 4.3) is considering this uncertainty. If indeed a lambda of 0.13 is more representative, I imagine the simulated meltwater flux should reduce by a factor four. The authors should critically consider which observational values are valid to compare to (and if there are none, so be it).

2) The description of the Results
   a. I see a few obvious patterns in the figures that the authors do not discuss, which I think is a shame. The manuscript would benefit from pointing out these results and analysing them.
      i. The asymmetry in Fig. 4b
      ii. The asymmetry in melt rate in Fig. 5b
      iii. The spatial correlation in Fig. 5b, which is absent in Fig. 5a
   b. The paragraphs in 3.2.1-3.2.3 very neatly describe one subpanel each. To my taste, this is a bit too neat, making me lose sight of the larger picture and the connection between the figures/sections. I suggest that the authors invest in a bit more inter-linkage here and perhaps step away from the 1 paragraph per subfigure. For example, in l. 330-331, the same process is pointed out as before. Make this explicit: "Similar to an enhanced subglacial meltwater flux and a warmer ambient ocean, a thicker melange increases upwelling and generates a more vigorous circulation. Hence, each of these processes causes an increase in upper-layer velocities".

3) The discussion: contents and structure
   a. You end with 'limitations of this study'. Depends on your taste, but I'd put this first, and then end with the good stuff (comparison and implications).
   b. Sec 4.2 (Buttressing) appears out of place. It is unclear how the new insights gained in this study reflect on buttressing. If they do, mention this explicitly, otherwise, this topic should be treated as motivation and be moved to the introduction.
   c. In Sec 4.3, the authors compare their submerged areas to observations. However, this is model input, not output. Hence, this comparison should be transferred to the methods section. Also, the authors should consider whether comparing absolute values in area makes sense considering the uncertainty in lambda.
   d. This study would benefit from more focus on 'lessons learned' in the discussion. As an example, the ISMIP7 team is currently developing protocols for the ocean forcing of the Greenland ice sheet. What guidance can you give that team based on your results? And if they cannot directly use your parameterisation, what additional research steps are necessary to do this?

Minor comments:

Abstract. Half of the abstract is intro/motivation, the rest mostly methodology. Please put more emphasis on the results/conclusions.

l. 55. Add explicitly that this impact (melange melt and not buttressing) is the topic of this paper.

l. 59. Mention 'due to entrainment'

l. 68. .. how **the** meltwater flux from icebergs influence**s the** large-scale ....

l. 75, 77. 'Exactly' and 'precisely' are unreasonable expectations for a model. Change to 'accurately' or similar.

l. 81 'This study' -> 'that study' (avoid ambiguity)

l. 120 Mention explicitly that the salinity profile is kept constant for all runs (this is only mentioned somewhere at the end of the manuscript)

Fig 1.b Make the colours in your different figures consistent. Here: use the same colours as in Fig 9.

l. 135 'constituted' -> 'constitute'

Eq 2. Becomes a bit clearer when changing –beta+1 to 1-beta

Fig 2.c Again, try to use the same colours and markers as in Figs 9 and/or 10

l. 220 No calving front melting in the analysis. But is it included in the simulation? (So an extra buoyancy source)? Please state this explicitly

l. 229 'simulate' -> 'simulating'

l. 232 **an** approximate steady state

l. 253 has been lost -> is lost

l. 272 **The** temperature

l. 274 mid-depths -> mid-depth

Fig 4b: xlabel should be 'across fjord'

Fig 4c: I'd replace 'velocity magnitude' with 'speed'. Velocity implies that it can be negative

l. 282 **a** considerable fluctuation; **a** variation

l. 283 a sum -> the sum

Fig 5: It is unclear why Fig 4 is smoothed, and Fig 5 is not. Is there a particular interest in the fine-scale variations? If not, I'd opt for consistency across the figures. Particularly in Fig 5b, a smoothed curve would better visualise whether there is an asymmetry in the meltwater flux or not

Fig 6: The near-overlap of blue and red curves makes them indistinguishable from the purple one. I suggest swapping orange and red. This also makes the order of the colours more intuitive.

l. 302 Is the shallower depth of neutral buoyancy determined by a fresher plume, a warmer plume, or significantly impacted by both?

l. 311-317 It's a bit difficult to follow the reasoning, while the point you try to make is quite straightforward. Please rewrite and to more directly get to the point.

l. 336 profile -> profiles

l. 339 This sentence implies that sublinear means a relatively weak sensitivity. This is not per se the case.

l. 340 Can you explain why this sensitivity is sublinear (what is the negative feedback / saturation process that decreases the sensitivity?)

l. 340 'Approximately linear'. Elsewhere, this is referred to as supralinear, which you indeed quantify in the parameterisation. Here, you give the explanation for the positive feedback inducing the supralinearity. Please link these statements and explanations together explicitly and make sure that what you interpret as supralinear is referred to as supralinear throughout the paper.

l. 344 **a** higher subglacial discharge

Fig 7: The caption isn't fully self-explanatory. Please explain that all variables are averaged horizontally over the full domain. And again, I'd suggest changing 'velocity magnitude' to speed (same for Fig 8)

l. 361 'as as' -> 'as a'

l. 363-367 I think it's more logical to say that T and S are the 'restoring / forcing' temperature and salinity. Because you state earlier that the results are independent of the initial conditions. The fact that you choose to initialise with the restoring conditions is irrelevant.

l. 363 You have a double use for lambda in the paper. I'd keep this here (usual nomenclature for the freezing point parameters) and change lambda for the iceberg area fraction. (Unless that's also a common usage, in that case, ignore this point)

l. 412 **the** deep ocean temperature

l. 419 'three times larger'. How does this compare to your results? And if they differ, can you explain why? If you don't compare this to your own results, it's not a relevant result to mention in your discussion.

Sec 4.3 You explicitly mention the seasonality in observations. How should I as a reader interpret this? Can your results be compared to the seasonally biased

observations? Or are your results representative of annual means? Please provide a bit of perspective

l. 431-437 These discussion points are again (strongly) dependent on the choice of lambda. Reconsider how these points should be perceived considering the uncertainty in lambda.

l. 440 this study -> our study (avoid ambiguity)

l. 442 ‘uses’: this is unclear. Do they prescribe velocities, in which case the result (maximum at 100m) is trivial? Or do they restore velocities to this half-sinusoid, in which case the velocities are actually resolved? Please be specific

l. 467 ‘The precise values … might vary’. If you link the values to physical interpretation (see point above), you may be able to provide useful constraints: that, based on physical reasoning, c will generally be < 1, and d >1.

l. 478 Can you, based on the sensitivity to melange thickness, and your analysis of down-fjord gradients in melting, provide some insight into how important you think the exact distribution may be?

l. 473 (Sorry for going on about this the whole time) A third source of uncertainty is the total areal fraction of icebergs. This deserves a mention here.

l. 498 Are you sure this is ‘the key limitation/uncertainty’? If so, why did you not do a sensitivity test to this? The simplest reply to this point is to change this to ‘a key limitation/uncertainty’, but perhaps, based on your knowledge and trial and error, you can provide a bit more insight than that on how important this aspect is.

l. 510 This is a contradictory sentence. The constants define the sensitivity, so the sensitivity cannot be independent of these constants.

l. 531 Here, it’s again ‘approximately linear’ to ocean temperature. Make sure your own interpretation is consistent throughout.

l. 535 predict -> project