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The Cryosphere Discussions

Interactive comment on "Increased glacier runoff enhances the penetration of warm Atlantic water into a large Greenland fjord" *by* A. J. Sole et al.

Anonymous Referee #1

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General Comments

This is a well-written manuscript describing an interesting and important natural phenomenon: the transfer of heat through Kangerdlugssuaq Fjord (KF) towards the Kangerdlugssuaq Glacier (KG) at the head of the fjord. The paper takes as its starting point earlier published temperature, salinity, and velocity data for the fjord waters in 1993 and 2004. The Bergen Ocean Model (BOM) is configured to simulate KF in order to understand the controls on the heat transfer up the fjord as the conditions at the mouth changed from cold (1993) to warm (2004). After spinning up the model with 1993 water properties, various experiments are carried out which purport to show that ocean warming at the mouth of the fjord has a more significant influence on melting at the terminus of KG when there is substantial melt-water runoff entering the fjord from under the glacier. I think the paper adequately reports its model results and does a good job putting the importance of the question of fjord dynamics in context by citing relevant references.

The work is successful in showing, in a certain context, that subglacial runoff enhances heat transfer and therefore increases the sensitivity of the glacier to ocean warming beyond the mouth of the fjord. However, I think it is a much too limited context in which this conclusion is meaningful. It was shown in (Straneo 2010, Nature Geosci.) that vigorous wind-driven baroclinic currents flush Sermilik Fjord in a matter of days. As a result of this externally forced 'intermediary circulation', Straneo concludes that water properties in Sermilik Fjord track sub-seasonal changes of water properties on the shelf. If KF, which like Sermilik lacks a shallow sill, is flushed so rapidly by external forcing that the water properties in the fjord always essentially match shelf properties (with a lag of no more than a few weeks) then it is hardly possible for subglacial discharge to enhance the penetration of shelf waters. The paper under review does not adequately simulate the effect of external forcing on ocean circulation in KF and is therefore unable to demonstrate that subglacial runoff has a significant effect on water properties in KF beyond the massive effect that external forcing already likely has on fjord properties. It may be that subglacial runoff does significantly enhance melting at the glacier terminus (as argued in Jenkins 2011, JPO) but this has to do with the enhancement of heat transfer over the last few meters of ocean next to the ice. This is a rather different question from the question of how ocean heat is transported into the fjord in the first place.

I would strongly recommend that model experiments be carried out to investigate whether subglacial runoff has a significant effect on heat transport into the fjord beyond that which would be accomplished by externally forced intermediary flows. For instance, it would be straight-forward to apply a boundary condition at the fjord mouth where the pycnocline is artificially oscillated up and down with an appropriate amplitude and period to simulate the effect of wind events. The paper's main conclusion is not adequately supported unless an experiment of this nature is carried out that could potentially rule-out the dominance of the externally-forced fjord flushing mechanism presented in (Straneo 2010).

A second general deficiency is the paper's discussion of glacial melt rates. The paper calculates the area-integrated heat flux Q along the fjord at a certain flux gate (it wasn't clear to me where along the fjord the chosen flux gate was) and then assumes that this energy is completely consumed as latent heat in order to obtain a total melt rate of ice. My objections are two-fold. First, when water temperatures in the fjord volume between the glacial terminus and the flux gate are not steady, the method is not valid. Energy conservation would say that $Q = c^*dT/dt + E$ where dT/dt is the rate at which the mean water temperature T is increasing and E is the rate at which melting consumes energy (from which melt rate can be inferred). Perhaps the flux gate in the paper is close enough to the terminus that the proportionality constant c is tiny, but this isn't demonstrated. Otherwise, and especially in a fjord that is rapidly flushed, you would expect Q and c^*dT/dt to both be quite large in magnitude and nearly equal to each other, with E being essentially uncorrelated to Q. My second objection is, why not just show melt rates as calculated by the model? The paper does not describe the model's treatment of the ice-ocean interface and the omission of directly calculated melt rates suggests there may be some deficiency there, which is not properly discussed.

I have several specific comments:

1. If there is no forcing in the spin-up period, why doesn't the model relax to a state of no motion? Is it that melting at the terminus drives a circulation, even in the absence of subglacial runoff?

2. Line 35-36: I am not convinced by this paper that reduced buttressing from sea-ice and ice mélange will be less important than undercutting. This claim goes beyond the scope of the model results, in my opinion.

3. Line 126: It would be appropriate to point out that a non-hydrostatic model would

have diminished accuracy in simulating narrow buoyant plumes.

4. Line 132: There ought to be a description of how ice-ocean interactions are represented in the model.

5. Line 201: What are the velocity boundary conditions during the spin-up? This is potentially important. If north-south velocity is set to 0 during the spin-up then the large transient velocities during phase 'a' in Figure 4 may due to the change in boundary condition rather than the commencement of forcing.

6. I think lines 211 to 219 should be re-written carefully. To make a suggestions, perhaps what is meant is: "For y=0 to y=7, model-generated velocities are adjusted towards prescribed external values (u_EXT,v_EXT,w_EXT) with a relaxation time constant T which varies from T=0 at y=0 (instantaneous adjustment) to T=∞ at y=7 (no adjustment)."

7. Figures 2 and 3 give contradictory impressions of the fluid domain in the model. Figure 2 has realistic coastlines but Figure 3 looks like a rectangular fluid domain.

8. Figure 10: The difference between a,b,c, and d for temperature and salinity are quite small and are not really discussed in the text. It may be better to omit the salinity plots altogether to make more space. For temperature plots, perhaps show anomalies relative to the initial conditions. For the velocity plot, the arrows are too small to be legible at actual figure size. Show streamlines of across-fjord-integrated along-fjord velocities instead. Streamlines could be plotted overlain on the temperature anomalies.

9. To show how the heat transport varies seasonally, a plot of the strength of the overturning circulation in the lower cell as a function of time would be very illuminating. In particular, the overturning in phase 'a' of Figure 4 ought to be highlighted and explained since it occurs in the absence of subglacial runoff and then goes away in phase 'b'.

10. Line 419: You have shown that wind-induced intermediary circulation is not necessary to bring about the temperature changes from 1993 to 2004. But you can not con-

clude that subglacial discharge is the dominant driver of heat transport. If KF flushes in a matter of weeks like Sermilik fjord (which we can not check due to limited data) it is probably not subglacial runoff that drives the main fjord circulation and it may have very little effect on heat transport. With a model, you could attempt to show that wind-driven intermediary flows are not sufficient to deliver heat to KG at the minimal rate that observations imply (although I suspect a model would not, in fact, show that).

11. Line 465: At the risk of being repetitive: the claim that subglacial runoff “increases the sensitivity” of KG to temperature changes beyond the fjord mouth is un-proven since the model can not disprove the default hypothesis (in light of what is know at Sermilik fjord) that KG is already always in contact (with a delay of just a few weeks) with whatever water mass appears outside the fjord.

Minor/technical corrections:

1. line 12: change “transmission of this warming” to “transfer of oceanic heat”

2. line 20: The word “estuarine” should perhaps be retired from discussions of glaciated fjords like KF/KG since it is becoming clear that such fjord are not like estuaries (river-fed embayments). Here the more general label “buoyancy-driven flow” is perhaps appropriate.

3. Line 35: “in agreement with obs, that maximum submarine melt rates occur... present at the fjord mouth” suggests that direct obs. of submarine melt rates exist for KG, but this is surely not the case.

4. Figure 1 caption: “AVHHR 4km” is undefined. There is no reference for the ocean current schematic.

5. Figure 2: Missing labels “A” and “B” on inset figures.

6. Line 135. Punctuation. Change “extent of the fjord, water” to “extent of the fjord. Water”

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7. Figure 3 caption: "FRSZ" is not defined yet in the main text when Figure 3 is first referenced. To correct this, just put panel 3d in its own figure and move the reference to it somewhere after the point where FRSZ is defined in the text.

8. Line 168: remove the period

9. Equation 1:

a. units are in italics, change to non-italics

b. 1.2x10ˆ{-3} should be typeset as $12\times 10ˆ{-3}$ to get the correct 'x'

c. |W_x|W_x|W_x should be |W_x|ˆ2 W_x

d. |W_y|W_y|W_y should be |W_y|ˆ2 W_y

10. Line 179: what is the treatment for basins other than KG shown in Fig 1?

11. Line 182: "constant" means "vigorous"?

12. Line 186: "external forcing of across shelf properties" is awkward

13. Line 199 The symbols CF,TF,RF,and WF are only defined implicitly. It only becomes clear later in the text that these are not just acronym labels and not just logical true/false flags, but actually variables that will later be set equal to various values. The variables are not really necessary in section 3.1. Perhaps introduce them later.

14. Line 202: does "stable" mean "steady" or "stable in the sense of stratification"?

15. Line 201: The sentence "While we acknowledge . . ." is unnecessary.

16. Line 207: How long does the spin-up take?

17. Line 212: Does "at each time step" mean "after each time step"?

18. Line 214: "From within the fjord" is unclear. Does this actually mean "the velocity produced by the model after the current timestep" or does it mean "the velocity generated by the model at y=7 (or 8, or . . .)"?

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19. Line 214: change “contains” to “refers to”. Also, “unrelaxed” is undefined here and unclear.

20. Caption of 3d is redundant with lines 216 to 221 (ie “The external. . .”)

21. Line 216: Previously the paper states that (u,v,w) at the boundary were set equal to tidal values, so shouldn’t phi_EXT simply be these tidal values?

22. The notation u,v,w_EXT = u,v,w_INT should be written (u_EXT,v_EXT,w_EXT)= (u_INT,v_INT,w_INT) if that is indeed what is meant. It is unclear as it is.

23. Line 227: What is the boundary condition for sea-surface height?

24. Line 242: define RF,CF,TF, and WF here.

25. Line 253: Citing (Jenkins 2011) after “as observed elsewhere” is inappropriate since that paper is theoretical and does not present new observations.

26. Line 257-260: This sentence seems like a cut-and-paste error since CM and CM2D are not defined yet and this is more of a model-internals remark.

27. Figure 5: Please indicate the time of these model outputs. Day 365?

28. Figure 6: The color scale for velocity is bad (ie too much green).

29. Line 286-290: Is the correlation calculated at a fixed time (ie day 365) or is it a time mean?

31. Line 290: CF appears to mean the scaling factor in: P_model_2004(z) = P_obs_1993(z) + CF*(P_obs_2004(z) – P_obs_1993(z)). This should be written in the text, if correct.

32. Line 297: “intrusion” is misspelled.

33. Table 1: Shallow salt agreement is better at most points when WF=0. Can you explain?

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34. Line 214: What does “standard” mean?

35. Line 339: Spelling of “equivalent”.

36. Line 340-343: This is mistaken reasoning, I think. If the entire energy flux Q is assumed to be converted to melting, then convection at the icefront is not relevant unless it increases or decreases the amount of energy available for melting at the end of the day (since the energy argument is agnostic about the dynamics of how heat is delivered to the ice). Introducing buoyant meltwater at the grounding line does introduce gravitational potential energy that could possibly contribute to melting, but that is not quantified here (or elsewhere, if I'm not mistaken) so can not be used in support of the claim “This calculation is a minimum estimate of submarine melt rates”.

37. Line 346: Model outputs at which time?

38. Line 348: “3x10ˆ9” should say “3x10ˆ10” I believe.

39. Line 351: Summer melt rates which are four times the annual average suggests that there is essentially 0 melting in the three non-summer seasons: (0+0+0+1)/4 = $\frac{1}{4}$. If it is true that melting is essentially absent without subglacial discharge, that ought to be made clear at some point.

40. Figure 8 caption: The vertical axes are labeled “mean heat transport”. Is this the time mean over the entire 365 day simulation?

41. In the calculation of Q, which flux gate is being used, section 2?

42. Line 402: The sentence “Although...” is unnecessary.

43. Line 408: “barotropic” is incorrect here. The intermediary circulation is generated by baroclinic pressure gradients created by moving the isopycnals up or down at the fjord mouth.

Interactive comment on The Cryosphere Discuss., 6, 4861, 2012.