Review: The multilayer ocean circulation melting the 79N Glacier ice tongue

Dear Authors,

Thank you for submitting your work and contributing to our exciting field of research. I enjoyed reading your article and think it will be a useful publication that fits the scope of “The Cryosphere”. I appreciate that more and more work is going into modeling Greenland’s fjords in 3D and think that your article's contribution to the field is relevant and timely. In my opinion the article is well written and concise; all results are adequately presented.

While the overall results and conclusions of the article are interesting and conclusive I have a concern about the implementation of Subglacial Discharge (SGD) in the model and the use of the word “subglacial” throughout the manuscript (see below for details) . In addition to these two, what I consider more important, concerns I list some smaller suggestions for improvement of the manuscript with respect to understandability, completeness and reproducibility below. While I did not specifically check for spelling or grammar mistakes, I mention those that I found while reading through the article at the end.

All in all, I am happy with the article and think it would be a great publication in “The Cryosphere” once the authors have considered my suggestions.

Subglacial Discharge Implementation and Discussion

One important point I would like the authors to clarify regards the model setup, where SGD is distributed uniformly along the grounding line. There is ample research showing that subglacial discharge is unlikely to be injected uniformly distributed along the grounding line, but rather as localized channels and that channelized subglacial discharge can lead to channelized flow and locally enhanced melting.

I understand that running 3D models is expensive and one has to carefully consider how to implement SGD in those simulations. While I don’t expect the authors to run more simulations with varying SGD distribution, the influence of a uniformly distributed vs. channelized SGD should be discussed as a potential caveat for the presented results.

I would like to ask the author to answer the following questions:

1. Why did you choose to implement SGD as uniformly distributed and not as localized injections? (for example in Section 2.3)
2. What are potential implications for the resulting circulation and melt distribution of local injection of highly buoyant SGD? Are the channels in the ice base maybe even a result of localized SGD?

Use of “subglacial”

Throughout the article you use the word “subglacial” in the context of “subglacial channels”, “subglacial plumes” and “subglacial melting” (among others). I am worried that at first glance these phrases could be interpreted as melting and channels at either the ice-ocean interface or at the ice-bedrock interface (depending on the reader's background one more so than the other). While I don’t think “subglacial” is strictly wrong in the context of an ice shelf cavity (it is below part of the glacier after all) I would suggest using a less ambiguous word. While not ideal either, I would suggest something along the lines of “subshelf channels”, “melt water plumes” or “marine basal melting”.

List of other suggestions

1. Line 6: Similar to “subglacial” the word “basal” can refer to melting at the ice-bedrock interface and is hence ambiguous here
2. Line 26: I always appreciate when the original name is at least mentioned in the introduction (Nioghalvfjerdsbræ)
3. Line 34: I don’t think that is the actual name of the Fjord (Nioghalvfjerd Fjord)
4. Line 106-107: I would be curious to read how the “regions of interest” (regions where vertical resolution is increased) are chosen? Is there some algorithm in the code to detect high density gradients or are these regions and increased resolutions prescribed? Or is it just implicit in the method?
5. Line 120: What is the spatial resolution of the ice thickness data in Bedmachine? I assume it is finer than the model resolution?
6. Line 147: Uniformly distributed SGD, see above
7. Line 163-164: Why did you choose these isopycnals? How sensitive are your results to the choice of isopycnals? In Fig. 6 it looks like the inflow (region of positive vel) exceeds the region defined by the chosen isopycnal by quite a bit. How do you justify your choice? Did you consider using u>0 as an alternative to define the inflow (or other variables/thresholds)?
8. Line 165: Consider referencing figure 5 and 6 here either in addition to or instead of referencing section 3.4.
9. Line 254: It was not immediately clear to me what you mean by “the total volume inflow across the main calving front”. Is it the net flux, i.e. inflow minus outflow?
10. Line 260 & 264: From the symbol I assume it is the same Q_melt? I am not sure why you change units suddenly but I assume you have your reasons. Please consider explaining the change of units explicitly here.
11. Line 263-269: Please consider a restructuring of this paragraph. The sentence starting in line 268 (“In contrast, …”) seems to be better connected with the first and second sentence of the paragraph. Your model melt rates are clearly on the higher

end of the range observed by Schaffer et al 2020, so why not give the explanation before mentioning the tracer based estimates by Huhn 2021?

12. Line 272 and Figure 3: The choice of range of colors makes it hard to see the strength of the melting. Consider changing the colorbar, as there are almost no areas of negative melt visible in the figure

13. Line 278-279: What is the satellite derived integrated melt rate (compared to your melt rate)?

14. Line 305: “The plume is a few meters thick…” Could you be more precise here? Maybe give a range based on the figure or do a similar analysis as for the bottom plume.

15. Line 313: “At the second transect…”. You never mention a first transect so maybe just leave out the phrase since you refer to the figure panel anyways.

16. Line 317-318: I would be curious to read why you think this happens.

17. Line 324: “...the flow goes in the direction of ...” I suggest rephrasing this. Maybe write instead “... the direction of the flow is towards …”.

18. Line 331: “... deflected by Coriolis into circles of Rossby radius …”. This sentence could be rephrased. Consider rewriting it to something like “deflected due to the Coriolis force forming circles the size of the local Rossby radius…”.

19. Line 335: Do you have an explanation for why the plume is thinner? I could imagine that the distribution of SGD plays a role here. Less SGD in the northern plume as rotation forces it more towards the south?

20. Line 346: Is there a reason you chose this temperature threshold for AIW? Is there a literature reference for the definition of AIW that could support this? Please elaborate on your choice.

21. Line 347: How do you define “flow direction into the cavity”? Just as westward or do you rotate the coordinate system somehow? Please indicate in the text what you do.

22. Line 350: I find it hard to see the acceleration of the plume and its velocity of 0.5m/s from figure 5. Did you mean figure 6?

23. Line 351: What are the observed velocity values?

24. Line 355-356: Is the denser water mass shown in a figure somewhere? If it is a remnant of the spin-up, this could be mentioned in your model setup section and in the discussion. Does this have consequences for how to interpret our results?

25. Line 358: I am not convinced that “within 100m” of the ice tongue is the same as “toward the glacier base” if you say before that the plume is only about 10 meters thick and insulates the ice from the ambient water. By that logic “within 100m” means basically away from the ice. I would maybe reformulate this to “providing heat for melting to the ice shelf cavity” or elaborate on what you mean by “toward the glacier base”.

26. Caption Figure 5: The last sentence starting with “The inflow becomes..” should maybe be moved to the text instead of being in the caption.

27. Line 363-367: This paragraph partly repeats what you have been writing in section 3.4.1 if I understand it correctly. Furthermore, looking at figure 6a/b parts of the inflow (by your definition using the 1027.5 isopycnal) seem both colder and fresher than the values you write here. I would be interested to see the thickness and bulk values of T,S and velocity as a function of the distance along the transect (essentially analogous to the Froude number). That would clearly show the acceleration and the freshening and cooling (line 375) of the inflow due to the enhanced mixing.

28. Line 380: See also comment 7. I am not fully convinced that fig 6 “shows that the chosen isopycnal delimits quite well the area that can be considered a plume”. While it does in part delimit a well mixed layer (at least in the plotted temperature and salinity ranges of the colorbar, can’t tell if there is further stratification above 34.5 g/kg or 1.8°C but I assume you chose the colorbar sensibly), there are significant areas of high glacierward velocity outside of the plume as you define it. Plotting bulk values (my comment 27) would allow you to quickly analyse how sensitive these values are to your choice of definition of the plume region. Did you consider using a velocity criterion to characterize the inflow plume?

29. Line 410: Here you talk about bulk values for T and S of the inflow but based on the stream functions. How did you calculate these bulk values?

30. Line 413-415: The last sentence in this paragraph is not clear to me. I thought that the plume is essentially melt water, that is entraining AIW (what you call “relatively warm and salty water at depth” I think?) while it rises along the ice base. During the plume’s ascent it accumulates more and more melt water, freshening and cooling the plume as it is flowing away from the GL.
    Now you mention “colder and lighter ambient water”. I assume that those are the upper parts of the AIW that get cooled by some recirculating part of the plume? This is, however, neither shown nor explained here or somewhere before. So maybe elaborate more on where the “colder and lighter ambient water” comes from.
    How can you attribute the cooling and freshening of the outflow to mixing with colder and lighter ambient water rather than to accumulated melt? Does this cooling and freshening happen after the plume detaches from the ice?

31. Line 439: Why is there no signature of SGD at the fjord mouth? Is SGD just too weak and hence spread too thin to show up in the TS diagram? Is that in line with observations?

32. Section 4+5: As mentioned above, I think it is important to add a discussion of the uniformly distributed SGD and its implication for the results of the simulations. What kind of differences would you expect from channelized subglacial discharge?

33. Line 471: “... presumably due to subglacial discharge.” Here would be a good point to mention the caveat of uniformly distributed subglacial discharge in your model setup.

34. Line 482: “... is also responsible for the coneshaped features… “. As you already say in the next sentence this is in no way certain. So I suggest to write “might be responsible”

35. Line 547: It would be helpful to state explicitly how the channel size compares to the resolution. Consider even doing this earlier than here, for example in the method section.

36. Line 568: Here would be another good opportunity to mention the uniformly distributed subglacial discharge.

37. Line 570-574: Are you saying that time-dependent subglacial discharge (I assume that would include seasonally changing SGD?) will not have a big impact on the circulation in the cavity? Similarly, I would assume that seasonally changing ocean and atmospheric forcing will have an impact. What kind of changes would you expect compared to your simulations? If they are forced by annual averages, I guess your simulations are representative for spring and fall?
    If you include atmospheric forcing, I would assume that you would get winddriven up and downwelling outside of the fjord (at least when it is ice free). This would lead to variable pycnocline depth which will influence the inflowing plume. I don’t think we can say with certainty that this would “not have a big impact on the oceanic circulation under the 79NG tongue” as it could alter the strength and temperature of the inflow and hence the melt dynamics in the cavity.

    In summary I would appreciate a more nuanced discussion of the model setup here or potentially rather in Section 4 (Discussion) in connection to the SGD distribution discussion instead of in the conclusions.

Typo(s):

1. Line 412: “... its bulk value**s** decrease**s** …”