Authors’ Response to Reviewers for

The multilayer ocean circulation melting the 79N Glacier ice tongue

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This document contains the reviews (in blue) by Jonathan Wiskandt and Anonymous Referee #2 together with our answers (in black) to the comments made by the reviewers.

Review 1

Dear Jonathan Wiskandt,

Thank you very much for your thorough review of our preprint. We read your comments with great interest and found them very helpful in improving our manuscript. Please find in the following our responses addressing your comments. We structured your review into parts A, B, C and D, following your headings, to refer to them below.

A: 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?

We fully agree with your remark. Subglacial discharge (SGD) is likely not uniformly distributed, but enters the cavity mostly through channels in the ice base. The BedMachine dataset used as ice topography in our model does not show basal channels at the grounding line (Fig. 1c), but only a few kilometers downstream. This is due to the blending of different data sources that was performed in BedMachine (by the developers of BedMachine, see Morlighem et al. 2017). This was quickly mentioned in our Sect. 2.2, and we make it more explicit in the revised manuscript, as follows:

> In the vicinity of the grounding line, BedMachine contains smoothed data as a result of blending datasets from different sources: a mass conservation approach was used for fast-flowing grounded ice, gravity inversion was used for the floating ice tongue, and both data sources were connected smoothly by interpolation (Morlighem et al., 2017). This results in a less detailed ice topography and no channels visible in this area.

We thus think that choosing discrete locations for SGD in the absence of channels in the ice topography at the grounding line would be speculative, so we used uniformly distributed SGD. Nevertheless, it is an important limitation of our current setup, in particular when thinking about a non-stationary simulation with time-dependent SGD forcing and much higher runoffs in summer. Following your suggestions, we make the following additional changes in the revised manuscript:

1. We add the following explanation to Sect. 2.3:
   Even though subglacial discharge is more likely to cross the grounding line through discrete channels (Narkevic et al., 2023), rather than widely distributed (Hewitt, 2020), we did not use channelized discharge in our model, since the BedMachine ice topography does not show channels at and near the grounding line (see Sect. 2.2). However, we discuss the possible implications of this simplification in Sect. 4.5.
2. We add a new subsection 4.5 to the Discussion, where we discuss in detail the potential implications of this simplification and how the situation could change with channelized SGD, in particular under time-dependent (seasonal) forcing with high discharge in summer.

B: 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”.
Thanks for pointing this out. We were not fully aware that these terms can be interpreted differently outside the oceanographic community. To avoid misunderstandings, we replace the terms as follows, taking into account your suggestions and those by the second reviewer (see Review 2, part B):

- We replace “subglacial melting” by “ocean-driven basal melting” and variants of it.
- We replace “subglacial channels” by “channels in the ice-shelf base” and variants of it.
- We replace “subglacial plume” by “subglacial meltwater plume”, except in sentences that already contain phrases like “melting”, “meltwater” or “ice–ocean interface”. Furthermore, we add a reference to the paper “Subglacial Plumes” (Hewitt 2020) at the first use of this term.

With these changes, the term “subglacial” only appears in the revised manuscript as “subglacial discharge”, “subglacial cavity” and “subglacial (meltwater) plume”. We believe that these changes help to avoid misunderstandings.

C: 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
We replace the term with “melting at the ice–ocean interface” here.
2. Line 26: I always appreciate when the original name is at least mentioned in the introduction (Nioghalvfjerdsbræ)
We add the original name to the Introduction, but otherwise keep the abbreviation 79NG.
3. Line 34: I don’t think that is the actual name of the Fjord (Nioghalvfjerd Fjord)
We replace the phrase “The 79NG fjord” with “The fjord of 79NG, 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?
The regions of increased resolution are not prescribed; the temporal evolution of the vertical resolution depends only on the stratification and the distance from the ice/seafloor. To give a better idea of how the adaptive coordinates determine “regions of interest”, we add the following sentence:

> This is implemented by letting the vertical distribution of the model layers evolve over time, where the layer spacing is a function of the vertical density gradient and the vertical distance

from the boundaries, see Burchard and Beckers (2004) and Hofmeister et al. (2010) for the mathematical details. This results in a “zooming toward stratification” …

5. Line 120: What is the spatial resolution of the ice thickness data in Bedmachine? I assume it is finer than the model resolution?

That’s correct, the nominal resolution of BedMachine is 150 m. We add this information to the paper, together with the corresponding information for RTopo, which has a resolution of 1/120°, i.e. 155 m to 175 m in longitude and about 930 m in latitude.

6. Line 147: Uniformly distributed SGD, see above

See our answer to part A of your review.

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)?

We add a sensitivity analysis regarding the choice of isopycnal to the revised manuscript, see our answer to Review 2, Major Comment A 3. In short, the exact values (of e.g., the Froude number) change if different isopycnals are chosen, but the transition from sub- to supercritical flow is not sensitive to the precise density thresholds. To show the sensitivity, we plot the Froude number, plume thickness and vertically-averaged plume properties also for a second isopycnal in the revised Fig. 6.

The criterion u > 0 does indeed define the inflow, but it is not suitable to define the plume. With this definition, the “plume interface” at u = 0 would lie in a weakly stratified area and the plume would include strong stratification, which is in contradiction to the usual definition of a plume being a relatively well-mixed current. This can also be seen in the plume analysis by Reinert et al. (2023), where the well-mixed plume is thinner than the inflow (and the same holds for the outflow).

8. Line 165: Consider referencing figure 5 and 6 here either in addition to or instead of referencing section 3.4.

As suggested, we add references to the figures here.

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?

Yes, it is the inflow minus the outflow. We add this information explicitly to the revised manuscript.

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.

To make the unit conversion explicit, we write both numbers in the same equation in the revised manuscript. The different units are provided to make it easier for the reader to compare with the cited literature. While m^3/s and mSv are typically used in oceanography for volume fluxes, km^3/yr is more common in the cryosphere context for melt volumes. Since both perspectives are possible in this particular context, we think it is helpful to provide the number in both units.

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?

Thank you for this suggestion. We restructure the paragraph accordingly.

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

We change the range of the colorbar to -50 to +50 m/yr, which makes the negative melt rates in the satellite products more clearly visible. With the shorter color range, the focus of Fig. 3 shifts from the high melt rates near the grounding line to the medium melt rates in the center. Taking into account this shifted focus and your comment 13 below, we rewrite the paragraph as follows (new sentences marked in green):

The distribution of basal melting in our model fits well with satellite-based observations by Wilson et al. (2017), Millan et al. (2023b), and Wang et al. (2024), see Fig. 3b–d. Apart from the southwestern part of the ice tongue, the melting appears to be more intense in our model than in the satellite data, but the melt patterns are similar. The hinge zone, which is the part of the ice tongue near the grounding line where the ice is not freely floating, is generally excluded from satellite measurements. However, the hinge zone is also the part where the most extreme melt rates occur (Zeising et al., 2024), so – as pointed out by Kanzow et al. (2025) – the area-averaged melt rate derived from satellite data rather underestimates the total melting. The area-averaged melt rates are 8.2 m yr^-1 (Wilson et al. 2017), 4.6 m yr^-1 (Millan et al. 2023b), and 8.0 m yr^-1 (Wang et al. 2024), and thus smaller than the above-mentioned in-situ measurements and our model results. Reasons for the difference are the absence of measurements in the hinge zone, the presence of negative melt rates in the satellite products, and the different temporal ranges covered (Fig. 3).

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

See our answer to your comment 12 above.

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.

We looked at the data again in detail and decided to replace “a few meters” by “about 10 m”.

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.

Good catch! We leave out the phrase.

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

We add “as it is a turbulent current entraining ambient water” as a brief explanation and refer the reader to Reinert et al. (2023) for further details, who showed this effect in an idealized study.

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

Thank you, this sounds much better. We rephrase it as suggested in the revised manuscript.

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…”.

We rewrite it as: “the plume is deflected by the Coriolis effect such that it flows in loops with 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?

That’s a good thought, so we add here a reference to the discussion on the subglacial discharge. Presumably, other factors also play a role, in particular the ice topography, making the northern plume more horizontally spread out than the southern plume.

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.

Yes, there are several literature references that support this definition. As two recent examples, we add references to McPherson et al. (2024) and Wekerle et al. (2024) to the text, who define AIW as water with a temperature above the threshold of 1 °C.

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.

Yes, with “into the cavity” we mean a westward flow in the cavity ($u < 0$), and we add this information to the manuscript.

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?

You’re absolutely right, there was a mismatch between text and figure. Thanks for pointing this out! The arrows in Fig. 5b show the *vertically averaged* velocity of the inflow, which encompasses several model layers. The maximum velocity of the vertically averaged inflow is 0.4 m/s. When looking at the

vertical structure of the inflow, the velocity maximum is 0.6 m/s at 15 m above the seafloor. This is relevant for the comparison with observations (your comment 23 below), since the observations similarly show the vertical structure of the inflow.
We rewrite this sentence to mention the vertically averaged as well as the vertically resolved velocity maximum, and we remove the reference to Fig. 5b from this sentence, since it's hard to extract precise velocity values from a quiver plot. We keep the reference to Fig. 5b at the beginning of the paragraph, where we explain that it shows vertically averaged velocities.

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

The observed velocities range from 0.3 to 0.6 m/s. We add this information to the text.

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?

We looked at the data again and made this paragraph more specific by explaining that the inflow reaches down to about 600 m depth before it detaches from the ground. The denser water mass below 600 m is shown in the new panels (c–e) added to Fig. 5, following comment C 19 of Review 2. Since our model is initialized with 3D temperature and salinity fields from a realistic global model that resolves the 79NG cavity (see Sect. 2.3), we think that the presence of this dense water mass in the cavity is realistic. However, it is essentially stagnant in the steady-state circulation shown in our manuscript, so a transient simulation with time-varying ocean forcing is required to explain under which conditions this water mass enters the cavity. We add a short discussion on this in Sect. 4.5.

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".

We take on your suggestion and replace the "toward"-phrase with "providing heat to the glacier cavity for melting the ice shelf."

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.

We remove this sentence from the caption, as it is already explained in the text.

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.

We restructure this paragraph to avoid repetition with the previous section. For the details, see our answer to Review 2, Major Comment A 3. In particular, we now show bulk values for temperature, velocity and plume thickness. This shows the acceleration and cooling more clearly, as you expected. We also plot the bulk values for a second isopycnal to show the sensitivity to the chosen plume interface. Since the precise values differ between the two plume definitions, we remove the values from the text; they can be extracted from the new graphs added to Fig. 6 if needed.

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?

See our answer to your comment 7 regarding the velocity criterion, and our answers to your comment 27 and Review 2, Major Comment A 3, regarding the choice of isopycnals, the plotting of bulk values and their sensitivity. Regarding the colorbars, we now extended them in Fig. 6(a,b) to include temperatures up to 2.0 °C and salinities up to 34.6 g/kg. This shows that these warmer and saltier waters are held back by the sill, whereas the plume hardly exceeds 1.8 °C and 34.5 g/kg.

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?

These bulk values were computed with Eq. (10) (which is now Eq. (11) in the revised manuscript), see Section 2.6. We add this reference to the text.

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?

We agree, the last sentence was not really clear. To make the explanation clearer, we split it into several sentences and explicitly mention both effects, the melting and the entrainment, as follows:

> Regarding the outflow, its bulk values decrease eastward [...]. This is due to two effects. Firstly, the outflowing plume accumulates more and more meltwater as it flows to the east, making it colder and fresher. Secondly, the eastward flowing plume rises along the ice tongue, passing through the stably stratified water in the cavity. Therefore, the ambient water that is entrained into the plume becomes lighter toward the east (Mohammadi-Aragh et al., 2025).

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?

Thank you for raising this point. The original wording here ("has almost disappeared") could give the impression that there is no signature of SGD near the fjord mouth. However, this is not the case – the signature is only reduced compared to the T–S diagram in the central cavity, but not absent. To avoid this misunderstanding, we remove the sentence in question and add instead the following two sentences to the paragraph above, also answering your question regarding observations:

> Further away from the grounding line, the influence of subglacial discharge on the T–S properties of the exchange flow is reduced, but still visible in deviations from the Gade line toward lower salinities (Fig. 8c). This looks similar to CTD profiles taken directly in front of the 79NG ice tongue (Huhn et al., 2021).

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?

We add a discussion on the SGD distribution as Sect. 4.5, see our answer to part A of your review.

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.

We add a reference to the new Sect. 4.5, where the distribution of subglacial discharge is discussed.

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"

In the revised manuscript, we change the sentence as suggested.

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.

We add the following sentence to the Methods section: "These channels are typically between 500 m and a few kilometers wide (Rignot and Steffen, 2008; Sergienko, 2013; Zeising et al., 2024), so the larger basal channels are resolved in our model." and we add the same numbers to the Conclusions at the suggested location.

36.Line 568: Here would be another good opportunity to mention the uniformly distributed subglacial discharge.
In the revised manuscript, this part is moved from the Conclusions to the new Discussion Sect. 4.5, where we discuss the implications of uniformly distributed discharge in detail, following your next comment 37.
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.
We agree, this paragraph can profit from more details, so we move it from the Conclusions to the Discussion as the new Sect. 4.5, give more explanations and include references to the literature. In the new section, we discuss in detail what will be necessary to make the setup fully realistic: seasonally and interannually changing SGD in combination with channelized SGD, time-dependent oceanic boundary conditions, atmospheric forcing combined with sea ice modeling – and how these modifications can change the results in transient simulations.

D: Typo(s)

1. Line 412: "... its bulk values decreases …"
Thanks for spotting this, we correct the phrase in the revised manuscript: "its bulk values decrease".

Review 2

Dear Anonymous Referee #2,

Thank you very much for your thorough review of our preprint. We read your comments with great interest and found them very helpful in improving our manuscript. Please find in the following our responses addressing your comments. We structured your review into parts A, B and C, following your headings, to refer to them below and in Review 1.

A: Major Comments

1. Subglacial Discharge Implementation (Lines 145-150, Sections 4 and 5)

One of my primary concerns is that the SGD is implemented as a uniform flux distributed along the entire grounding line (~120 km). There is substantial literature demonstrating that subglacial discharge in Greenland's glaciers exits through a small number of discrete channels, often just one to a few, and that channelized SGD drives dramatically different circulation and melt patterns than distributed discharge. A concentrated buoyant plume from a localized source would create a vigorous, focused upwelling with qualitatively different T-S properties, melt channel geometry, and outflow signature compared to the diffuse forcing used here. Perhaps the authors would consider one or more of the following.

- Justify the choice of uniform SGD in Section 2.3 or explain the caveat.
- Discuss the consequences of uniform vs. localized SGD for (a) the northern plume (p3), which starts at the grounding line and has the freshest signature; (b) the central plume's reversal behavior; and (c) the melt distribution near the grounding line where peak rates exceed 100 m/yr.
- The cone-shaped ice features discussed in Sections 3.3.2 and 4.2 are tentatively attributed to the subglacial plume, but the plume's structure in your model is strongly conditioned by the uniform SGD distribution. Acknowledge that localized SGD might create a fundamentally different plume geometry in these regions.
- At a minimum, add a few sentences in Section 4 (Discussion) addressing the SGD caveat, and briefly revisit it in the Conclusions. Additional possible places for brief acknowledgment also exist at Lines 471, 547-549, and 568.

We agree with your remark that channelized subglacial discharge (SGD) would be more realistic than a uniform distribution along the grounding line. However, please note that the grounding line in the 79NG fjord is only about 25 km (not ~ 120 km) long, see Fig. 1b. We add this number to the manuscript. Furthermore, we implement your suggestions as follows:

- We add an explanation to Sect. 2.3 why we chose a uniform SGD distribution and state explicitly that this is a simplification, see our answer to Review 1, part A.
- We add a new section to the Discussion (Sect. 4.5) in the revised manuscript, discussing in detail how the results could be different with channelized SGD. We discuss in particular how the melt distribution and the peak melt rates near the grounding line could change, compared to our results. Furthermore, we discuss the impact of seasonally varying SGD, because the discharge is strong in summer (June to August) but almost absent during the rest of the year. We refer to this new Sect. 4.5 in those places of the manuscript, where SGD is mentioned, in particular Sect. 2.3, Sect. 3.3.3, Sect. 3.5.3 (which has become Sect. 3.6 in the revised manuscript) and Sect. 4.2.

2. Overstated Cone-Formation Hypothesis (Lines 326-331, 479-485, 552-558)

The suggestion that the cone-like features in the ice topography are “shaped by the subglacial plume itself” (Line 557) is an interesting hypothesis but somewhat speculative although perhaps indirectly supported by the presented simulations. The model uses a prescribed, fixed ice topography and cannot demonstrate that the plume creates these features; it only shows that the plume’s behavior is consistent with the presence of these features. I suggest one or more of the following:

- In the text, try to reframe this as a hypothesis for future investigation rather than a finding of this study. The language in Lines 480–483 and 555–558 currently reads closer to a conclusion than a conjecture.
- Strengthen the physical argument if you wish to retain the hypothesis.
- In Line 482, change “is also responsible for” to “might be responsible for”, and adjust similar phrasing throughout.

We agree that this remains a hypothesis, as our model can only show that the creation of these cone-shaped features is plausible, given the prescribed ice topography and the simulated flow of the plume. Accordingly, we change both sentences in lines 481 to 483 as suggested. In the Conclusions (lines 556 to 558), we replace “are shaped” with “may be shaped”. In the abstract, we change the phrasing to “may have formed” to emphasize that this is a hypothesis. Finally, we add an outlook at the end of the paper, containing as a suggestion for future investigation: “a coupled ice–ocean model should be used to test the hypothesis that the meltwater plume creates cone-like structures in the floating ice tongue.”

3. Hydraulic Control Analysis: Plume Definition and Sensitivity (Section 3.4.2, Lines 160-180)

The Froude number analysis is central to the hydraulic control argument, but the plume definition using a fixed isopycnal of 27.5 kg m^{-3} is acknowledged to be imperfect (Lines 377–381). As the plume entrains ambient water and becomes lighter, this isopycnal can transition from the plume interior to below the plume interface along the transect. The Froude number depends directly on plume thickness and buoyancy, both of which are sensitive to this choice.

- Explain in a brief comment how sensitive the computed Froude number (Fig. 6f) is to the choice of the 27.5 kg m^{-3} threshold. A brief sensitivity test with an alternative isopycnal (e.g., 27.3 kg m^{-3}) or a velocity-based criterion would greatly strengthen the conclusion.
- Provide bulk property estimates (plume thickness, mean temperature, mean salinity, mean velocity) as a function of distance along the transect (complementary to Fig. 6).
- The claim that mixing increases by “several orders of magnitude” (Line 373) should be quantified precisely. From Fig. 6d the range appears to be roughly 10^{-8} to 10^{-5} (°C)^2/s, i.e., approximately three orders of magnitude. Please state this explicitly.

Thank you very much for this remark. We checked the sensitivity, implement the proposed changes in the revised manuscript and extend Fig. 6 as suggested:

- We verified that the transition from subcritical to supercritical flow on the slope downstream of the sill does not depend on the exact density threshold and can also be obtained if the two isopycnals are varied by ±0.05 kg/m^3 or ±0.1 kg/m^3. We add this information to the text. Regarding a velocity-based criterion, see our answer to Review 1, comment C 7.
- We show bulk values for plume thickness, mean velocity and mean temperature in new panels added to Fig. 6; salinity and temperature look almost the same, so showing both would add little information. To show the sensitivity of the bulk values, we show them for 27.5 and for 27.45 as the plume interface. 27.45 is the isopycnal the furthest from the seafloor that lies within the inflow over the whole transect (i.e., 27.4 only lies within the inflow for part of the transect, so it is less relevant and thus not shown).
- We rewrite the sentence: “This leads to a strong increase (at least three orders of magnitude) of vertical temperature mixing at the interface between plume and ambient water.”

To add this new information to the revised manuscript, we restructure Sect. 3.4.2 as follows: The first introductory paragraph still motivates the analysis of this transect. The second paragraph describes the development of the inflow over the whole transect, including the increase of mixing, but without mentioning the Froude number. The third paragraph explains the hydraulic control by referring to the Froude number. The next paragraph discusses the sensitivity of the results to the chosen isopycnals. The last paragraph mentions the caveat that any isopycnal can only approximate the plume interface, similar to the first version of our manuscript.

B: Terminology: Use of “Subglacial”

The term “subglacial” is used throughout to describe the meltwater plumes and channels at the ice-ocean interface (e.g., “subglacial plume,” “subglacial channels,” “subglacial melting”). In glaciology, “subglacial” conventionally refers to the ice-bedrock interface, not the ice-ocean interface. I understand why you use it here for a floating tongue (“below part of the glacier”), it risks creating confusion for readers from other subfields.
I suggest replacing “subglacial plume” with “meltwater plume” or “buoyant basal plume,” “subglacial channel” with “basal channel” or “ice-shelf channel,” and “subglacial melting” with “basal melting.” The term “subglacial discharge” for the grounding-line runoff is conventional and should be retained. Similarly, “basal” in the context of “basal melt rate” (e.g., Line 6) could refer to ice-bedrock melting and should be clarified as “ocean-driven basal melt” on first use.
Thank you very much for pointing this out. To avoid this possible confusion, we change the terms in the revised manuscript, see our answer to Review 1, part B.

C: Minor and Specific Comments

1. Abstract, Line 5: Specify model resolution (“~500 m horizontal resolution, ~85,000 grid cells, with 100 adaptive vertical layers”) to give readers immediate context for the study’s capabilities and domain size.
We add “(500 m horizontal resolution, 100 adaptive vertical layers)” to the abstract; the domain size is described by the term “a model of the fjord” in the same sentence.
2. Abstract, Lines 8-10: The “ice cones” are mentioned prominently in the abstract but rest on speculative interpretation not directly demonstrated by the model. Either downgrade the language (“consistent with formation by...”) or remove from abstract.
We change the phrase to: “suggesting that these cones may have formed through plume-induced melting,” where the words “suggesting” and “may” emphasize that this is speculative.
3. Line 26: Consider mentioning the glacier’s name (Nioghalvfjerdsbræ) on first use.
As suggested, we add the glacier’s name here.
4. Lines 104-109: Add detail on how the adaptive coordinates detect and refine resolution near stratification layers. Is there an explicit density-gradient criterion, or does refinement happen implicitly through the coordinate transformation?
We add the following sentence to the revised manuscript, explaining in more detail how the adaptive coordinates refine vertical resolution at stratification:

> This is implemented by letting the vertical distribution of the model layers evolve over time, where the layer spacing is a function of the vertical density gradient and the vertical distance from the boundaries, see Burchard and Beckers (2004) and Hofmeister et al. (2010) for the mathematical details.

5. Line 110 (Model grid size): It would help to explicitly state the total grid dimensions (e.g., Nx × Ny). You do state the Nz.
Good suggestion. We add this information to Sect. 2.1 (312 × 273 grid cells) and move the total number of grid cells (85,000) from Sect. 2.3 to Sect. 2.1 as well.

6. Line 120: State the spatial resolution of BedMachine v5 (approximately 150 m) and note that it is considerably finer than the model grid (500 m), meaning the model does not resolve all of the channel structure present in the dataset.

We add to the text that the nominal resolution of BedMachine is 150 m and that of RTopo is 1/120°, i.e., 155 m to 175 m in longitude and about 930 m in latitude. Furthermore, we add the following sentence regarding the channel width, as suggested in Review 1, comment C 35: "These channels are typically between 500 m and a few kilometers wide (Rignot and Steffen, 2008; Sergienko, 2013; Zeising et al., 2024), so the larger basal channels are resolved in our model."

7. Lines 163–165: Justify the choice of 27.5 and 27.2 kg m^{-3} as the layer-defining isopycnals. State whether these were chosen to match Schaffer et al. (2020) for comparability, and note any sensitivity.

See our answer to your Major Comment A 3.

8. Line 254: Clarify whether "total volume inflow across the main calving front" refers to the net flux (inflow minus outflow) or only the one-directional inflow transport.

The term refers to the difference of in- and outflow. We add this clarification to the revised manuscript.

9. Lines 260 and 264: Q_melt is given in both mSv and km^3/yr in consecutive sentences. Explain the unit conversion or use consistent units throughout.

In the revised manuscript, we show the unit conversion explicitly by writing both numbers in the same equation. The different units are provided to make it easier for the reader to compare with the cited literature. The units mSv and m^3/s are more common in oceanography for volume fluxes (as in line 260), whereas the unit km^3/yr is more common in glaciology for melt volumes (as in line 264).

10. Lines 261 and 299: Both sentences end with "explored further in the following" without completing the reference (i.e., "in the following subsection" or specifying the section number).

Thanks for pointing this out. We complete the references.

11. Lines 263–269: Restructure the paragraph. The warm-bias caveat (Line 268) logically precedes the comparison with Huhn et al. (2021), not follows it. Suggested order: (1) model result, (2) warm-bias caveat from forcing, (3) comparison with Schaffer et al. (2020), (4) comparison with lower Huhn et al. (2021) estimate explained by their cold-period measurements.

Good idea. We restructure the paragraph almost as suggested. We mention at first the model result (making the unit conversion explicit), then we compare with Schaffer et al. (2020), who use the same units. Since our result is on the higher end of the confidence interval, we explain the warm bias. Then we mention the lower, tracer-based estimate, and the possible explanation in the cold period.

12. Figure 2: This figure is too small to resolve the velocity quiver structure, which is central to some of the paper's claims. Please make Figure 2 substantially larger. Additionally, the text states the barotropic flow is "to first order in geostrophic balance," but much of the flow in Fig. 2 is clearly not aligned with the water-column thickness contours. An explanation is warranted: is the misalignment primarily a result of ageostrophic eddy kinetic energy within the anticyclonic vortex, or of other effects (e.g., bottom friction, transient eddies)? Please comment on this in the text.

We agree that Fig. 2 shows a large part of the barotropic flow to be in geostrophic balance, but there are regions where this balance is violated. In the revised manuscript, we make this clearer by describing more precisely where the barotropic flow follows the contours of the water column thickness and where it deviates from them. In particular along the northern fjord wall, the vertically integrated flow is directed opposite to what barotropic geostrophy predicts. This is the signature of the northern meltwater plume (compare Figs. 2 and 4a–b). Importantly, the buoyant plume is a baroclinic current driven by density differences, not a barotropic current. Therefore, it is geostrophically adjusted with respect to ice draft contours, instead of water column thickness contours. Accordingly, we now explain in the text that deviations of the barotropic flow from geostrophic balance are mainly due to the baroclinic plumes and refer the reader to Sect. 3.3 for further details. As suggested, we also make the figure 33 % larger, so that it has the same width as Figs. 3 and 5. Furthermore, we change the colors for higher contrast, make the lines thicker and enlarge the quiver arrows for better visibility.

13. Figure 3 colorbar: The melt rate range (−100 to +100 m/yr) makes the dominant 0–10 m/yr range nearly invisible. Consider using an asymmetric or logarithmic colorscale, or at minimum a diverging scale with finer resolution near zero.
We change the range of the colorbar to -50 to +50 m/yr. This shifts the focus of the figure from high to medium melt rates and also makes melt rates below 10 m/yr visible. See also Review 1, C 12.
14. Section 3.3.1 (Line 300, southern plume p1): This is arguably the most dynamically interesting plume, carrying the strongest momentum signal near the ice base. Yet its contribution to the total melt rate is not discussed in proportion to its prominence in the velocity field. The authors should comment on why p1 does not contribute more visibly to the integrated melt. Is it because the plume is colder than the ambient AIW, and its insulating effect partially offsets the enhanced friction? Furthermore, the asymmetric basal geometry visible in the T1 transect (Fig. 4c–e) appears consistent with a Coriolis-deflected plume preferentially melting the right flank of basal channels. Perhaps worth noting explicitly. Finally, the drag coefficient used at the ice-ocean interface for this fast plume may be worth briefly justifying, as ice-ocean-plume drag parameterizations remain somewhat contentious in the literature.
We agree that this is the most dynamically interesting plume. In fact, the locations of high plume velocities do correspond to stronger melting, and we add this information to Sect. 3.3.1, highlighting a number of areas where this can be seen by comparing Fig. 4a with the (updated) Fig. 3a. Furthermore, we add a sentence to Sect. 4.2 (discussion on the meltwater plumes), noting explicitly the asymmetric channel geometry, as this applies also to the central plume.
Our model uses the drag parameterization by Burchard et al. (2022) at the ice–ocean interface. We add the equation for the drag coefficient with a brief justification to Sect. 2.1.
15. Section 3.3.2 (central plume p2, Lines 321-331): This section would benefit from additional physical clarification. The plume turns clockwise and reverses, apparently multiple times. It is not clear whether (a) the plume loses significant momentum with each turn and is effectively “restarted” from near-rest, or (b) it retains momentum and the reversals represent inertial oscillations at the Rossby scale. Figure 4a may not clearly show the momentum evolution of the plume through these turns. Please add a sentence or two clarifying this, and if the time-averaged figure obscures the dynamics, note that explicitly.
We looked at the model results again and add the following sentences for additional clarification:

> Initially, the direction of the flow is toward the calving front, but after a few kilometers, the flow splits up. One part of the plume continues toward the calving front, the other part turns clockwise, reverses direction and then merges with the southern plume.

Thus, the flow does not lose its momentum and is not restarted, but it diverges. This process is rather a geostrophic adjustment than an inertial oscillation, because the plume is density-driven; an inertial oscillation would develop in the absence of forcing and would have a radius of $r = u/f \approx 1$ km (for a plume velocity of $u = 0.15$ m/s), smaller than the Rossby radius of 2–4 km, which fits better to the size of the turns in our simulation.
16. Line 340: The transitional sentence “This inflowing plume is the topic of the following Sect. 3.4” is redundant given the section structure. Consider removing it or replacing with a brief physical statement connecting the outflowing p3 to the AIW inflow immediately below it (which is already visible in Fig. 4f).
We remove the sentence and instead add the shorter clause “which is the topic of Sect. 3.4”. While this can be considered redundant, we think it is acceptable to keep these six words, as they can be helpful to guide readers through the paper.
17. Figure 6 (T/S sawtooth patterns, Lines 363-367): The sawtooth structure visible in the temperature and salinity fields near 300 m depth between 0 and 5 km along the transect is conspicuous. Is this numerical noise from the adaptive coordinate discretization, a real feature of the stratification, or an artifact of how model layers are output? Please add a sentence in the text or caption acknowledging and explaining this feature.
This is a purely visual artifact from the mesh plot. To show the model data along a transect, we plot it directly without any color smoothing. The T/S-values computed at cell centers are used to color the

full quadrilateral model cell, including the cell edges. Over steep topography, the model layers are tilted, so cell edges are above and below the cell center. Therefore, T/S-values of adjacent cells can differ visibly in regions of strong stratification, leading to the sawtooth pattern you describe. We add a brief explanation to the figure caption.

18. Line 346: Justify the 1°C temperature threshold for AIW identification.

We add the following phrase to justify the 1 °C temperature threshold: "following the AIW definition used, for example, by McPherson et al. (2024) and Wekerle et al. (2024)."

19. Line 355-356: The denser water mass below the plume in the northern cavity (spin-up artifact) should appear in a figure somewhere. If it is visible in Fig. 6 or Fig. 8, point readers to it; if not, add an inset or supplementary panel.

We add three new panels to Fig. 5 with vertical profiles of temperature, salinity and velocity in the northern part of the cavity, showing both the inflow and the denser water mass below. We also discuss the origin of this deep water mass in the new Sect. 4.5 of the revised manuscript.

20. Line 358: Rephrase "within 100 m of the ice tongue" to clarify this is vertical proximity; the plume is still separated from the ice base by 100 m of water. "Bringing warm AIW to within 100 m of the ice base" is clearer.

Thanks for pointing this out. To avoid misunderstandings, we adopt your suggestion and reformulate this sentence as follows: "The warm (> 1 °C) AIW plume comes within 100 m of the ice base, providing heat to the glacier cavity for melting the ice shelf." This phrasing also takes into account comment C 25 of Review 1.

21. Sections 3.5.1-3.5.3: These three subsections are quite short and function largely as figure walk-throughs. Consider merging them into a single Section 3.5 with a brief introductory paragraph, or at minimum reducing the heading hierarchy. The figure-by-figure structure currently breaks up what is a naturally unified overturning analysis.

You're absolutely right. We remove the headings 3.5.1 and 3.5.2 and merge both subsections into 3.5, which allows shortening the introduction paragraph of Section 3.5. We promote subsection 3.5.3 to Section 3.6 in the revised manuscript, so that we end up with two sections (3.5 and 3.6) of comparable length, and a reduced heading hierarchy.

22. Lines 415-419 (freshwater budget and overturning): It is noted that subglacial runoff makes up ~10% of the total freshwater flux leaving the cavity but contributes only ~2% to the cavity overturning. This apparent discrepancy deserves a brief physical explanation. Since overturning strength scales with the density contrast between in- and outflowing water masses (not just their volume), the dilute runoff, which has already been mixed into a much larger outflow, contributes little to the overturning even though it is a significant volume source. Making this explicit would help readers understand the TEF framework and the relative roles of melt vs. freshwater flux.

Thank you for pointing this out. We agree that this paragraph was not easy to understand. To make it clearer, we now give all volume fluxes in m³/s for a more direct comparison of the values. Furthermore, we add a physical explanation for the fact that the total freshwater flux contributes only 2 % to the cavity overturning, based on the mixing in the fjord. Finally, we add that measurements gave a similar value of 1.4 % (without using the TEF framework). The revised paragraph now reads:

> The outflow at the fjord mouth of Q_{out} = 35.7 mSv = 35.7 × 10³ m³/s is larger than the inflow of $|Q_{in}|$ = 35.0 mSv = 35 × 10³ m³/s, where the difference of 708 m³/s is explained to 90 % by meltwater, Q_{melt} = 638 m³/s (Sect. 3.2), and to 10 % by subglacial discharge, Q_{runoff} = 70 m³/s (Sect. 2.3). The total freshwater flux leaving the cavity, $Q_{melt} + Q_{runoff}$, contributes 2 % to the cavity overturning $|Q_{in}|$, and the mixing completeness, $-Q_{in}/Q_{out}$ (MacCready et al., 2018; Burchard et al., 2019), is 98 %. This means, the freshwater from ocean-driven melting and subglacial runoff is mixed by entrainment of ambient water to almost ocean salinity within the cavity before leaving the 79NG fjord. These results agree with mooring data from 2016/2017 within the measurement uncertainties, which gave an overturning strength of (46 ± 11) mSv and a contribution of 1.4 % from the total freshwater flux (Schaffer et al., 2020).

23. Lines 570-574: The claim that seasonal and time-dependent forcing would “not have a big impact” on the cavity circulation is not well supported. Time-varying pycnocline depth driven by atmospheric or tidal forcing at the shelf could modulate the AIW inflow over the sill. This conclusion should be softened or qualified.

We move this paragraph from the Conclusions to the Discussion as part of the new Sect. 4.5, where we substantially expand the treatment of this topic. In the new section, we explain how time-dependent forcing, seasonal discharge and atmospheric processes can impact the AIW inflow and the cavity circulation in transient simulations, referencing the relevant literature.

24. Figure 6, panel (f): Add a horizontal dashed line at Fr = 1 to make the supercritical transition visually explicit.

We add the horizontal dashed line at Fr = 1 as suggested (now panel i instead of f).

25. Figure 7, panel (a): Consider annotating the depth of the sill and the approximate depth range of each subglacial plume’s outflow for easier cross-referencing with the text.

For easier cross-referencing with the text, we put ticks on the depth-axis in 100 m-spacing on both the left and the right side of the figure.

26. Typo, Line 412: “its bulk values decreases” to “its bulk values decrease.”

Thank you, we correct this typo in the revised manuscript.