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

Overall Assessment

This manuscript by Reinert et al. presents a sophisticated and insightful 3D ocean model of the 79 North Glacier (79NG) fjord, with the distinguishing feature of using adaptive vertical coordinates (GETM) to simultaneously resolve the subglacial meltwater plumes and the inflowing Atlantic Intermediate Water (AIW) gravity current. The study makes genuine methodological advances over prior z-coordinate approaches, and the results (three distinct subglacial plumes, hydraulic control at the sill, and meltwater export at 100-200 m depth) are broadly consistent with observations and shed light on boundary layer structure within and near the cavity circulation.

I recommend publication after moderate revisions. The most important concerns are (1) the treatment and discussion of subglacial discharge (SGD) implementation, (2) insufficient sensitivity analysis for key topographic and forcing choices, and (3) a need for more cautious language around the speculative cone-formation hypothesis. Several secondary issues concerning completeness and reproducibility are listed below.

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.

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.

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.

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.

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.
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.
3. Line 26: Consider mentioning the glacier's name (Nioghalvfjerdsbræ) on first use.
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?
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.
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.
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.
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.
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.
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).
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.
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.
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.

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

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.
24. Figure 6, panel (f): Add a horizontal dashed line at Fr = 1 to make the supercritical transition visually explicit.
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.
26. Typo, Line 412: “its bulk values decreases” to “its bulk values decrease.”