The manuscript describes the output from a regional, three-dimensional numerical model of the ocean. It includes two lateral boundaries in the north and south where water properties (temperature and salinity) and velocity vectors are specified and complex coastlines. The model geometry includes a floating ice shelf (Petermann Gl.) as well as realistic bottom topography both under the floating glacier and everywhere else within the modeling domain. The presence of ice (sea ice and glacier ice) is prescribed (not modeled) as a surface boundary condition with a surface stress (vertical momentum flux) condition that depends on the prescribed (not modeled) ice concentration and ice velocity. The ice is allowed to melt because of conductive and turbulent heat flux from the ocean to the ice. Much of this model set-up and selective comparison to limited data is published as Prakash et al. (2022). Reading this publication, I consider essential to critically evaluate the present manuscript. I recommend against publication for the following three major at least 10 minor reasons:

A_ Prior ice-ocean models of the region exist (Shroyer et al., 2015; Shroyer et al. 2017) and substantial overlap exists with few new or original results. Unlike the present model, the prior models include a prognostic (as opposed to a prescribed) sea ice model. Basal melt rates are predicted under the floating portion of the glacier by both present and prior models. Both present and prior models evaluate the differences between sea ice in the adjacent ocean and fjord that is mobile vs sea ice that is not mobile. I cannot discern fundamentally new results or insight in the present model application that basically repeats prior modeling with a different model, that, I feel, has more weaknesses than strength over prior models.

B_ The present model uses temperature and salinity at the boundaries that are both poor and unrealistic. This "cold bias" is prominently discussed in Prakash et al. (2022), but it is introduced in the present manuscript almost as an after-thought in passing on Line-375. Moored and synoptic observations indicate salinity and temperature of about 34.7 psu +0.2 C for the Atlantic-influenced waters both in Petermann Fjord and below the ice shelf (Muenchow et al., 2016; Washam et al, 2018). The model provides "… annual mean temperature and salinity of the water masses that overflow the inner sill…" that are 34 psu and -0.7 C (Line-230). So, the modeled ocean is almost 1.0 C too cold and 0.7 psu too fresh at a depth where seasonal and interannual variations are less than 0.1 C and 0.01 psu! So, the model includes huge biases that I find unacceptable. Note that Prakash et al. (2022, page-27, Line-6/7) " … suggest applying a depth-dependent bias correction to the upstream A4 T-S fields such as in Shroyer et al. (2015)." Please follow your own suggestion.

C_ The present model attempts to compensate for the "cold and fresh bias" by artificially increasing the vertical turbulent exchange coefficient to produce melt rates that are somewhat agreeable to observations of melt rate. So, the model includes a wrong (northern) boundary condition (from the nested A4 model) and a wrong "turbulence model" to make things right. From my observational perspective two wrongs rarely make a right and I thus loose trust in this model as, it appears, the model can "nudge" or "fix" anything as there is always a parameter or dial that one can change to obtain any desired result. I do not endorse this practice, especially since the authors know the proper way to remove these biases and perhaps use realistic turbulent exchange coefficients.

In addition to the above major weaknesses, there are a number of more minor concerns

1_ The manuscript is too long and unnecessarily complex. I could not discern much substantial difference between the “Thick-Mobile” and the Thin-Mobile” case. What differences there are, I feel, may fall into the domain of model uncertainty, noise, and poorly constrained observational and/or model parameters.

2_ The manuscript is too long and contains trivial co-ordinate transformations. Just state that co-ordinates are rotated into along- and across-channel components rather than spelling out what I perceive as trivial algebra (Lines-143 through Line-182). I understand that the present model is finite element that that this may not be as trivial computationally as it is in finite-difference models or observations.

3_ I am confused and disturbed by streamlines that start at boundaries and end in the interior. Figure-3 offends my sense of mass conversation and lateral boundary conditions. Would you not expect a zero velocity along the coast?

4_ Figure-4 has large spatial oscillations that result from a poorly used graphics package. Please learn how to draw contours properly. Furthermore, I prefer distances in km as opposed to Longitude. The strong bottom-intensified slope current under the western ice shelf during the summer (Fig.-4b) caught my attention. Strangely, no such current exists in the fjord where bottom slopes may be similar, well, I cannot tell, because Longitude rather km is used for distance.

5_ On Line-310 it is stated that “… upwelled AW [Atlantic Water] from the adjacent Nares Strait enter the fjord…” How does this work? Would not winds from north to south in Nares Strait that may cause the AW to upwell along Greenland in the east also lower sea level along Greenland relative to Canada? Would then Petermann Fjord not respond with a large outflow the way an estuary would with lower sea level at is mouth?

6_ Please provide uncertainty estimates to your estimates of heat fluxes such as summarized in Table-2 as it is done with observational estimates of these same fluxes.

7_ In my view the authors mistakenly equate heat flux into the fjord and/or glacier cavity with basal melting (Line-368, Line-373). First, most of the heat flux into the fjord leaves it. Only a small fraction of the heat is actually used to melt the ice which in the present model is largely caused by the artificially increased vertical turbulent exchange coefficient. Second, did the authors actually check, if mass is conserved? Does the volume flux add to the amount of melting?

8_ The comparison of model predicted basal melt rates with observations appears to me less systematic than it could be. The authors appear to pick whatever value from whatever paper for whatever season that fits their purpose. Perhaps this part of the discussion can be strengthened by more clearly delineate seasons, space, and vastly different observational techniques (snapshots vs. moored observations or remotely satellite vs fixed radar stations).

9_ Line-438/439/440: The Rueckamp et al. (2019) reference clearly states that the “still attached” new ice island has already “dynamically detached,” that is, from a practical or physical perspective, this segment of the ice shelf is already gone.

10_ Line-511: The “Nares Strait sea ice arches” (mobile sea ice) add 2 m/y basal melt to the 24 m/y (Line-243), so the entire paper is about a 10% effect. The supply of heat to the fjord or glacier cavity matters little, but both the vertical turbulent mixing and the grounding line discharge of freshwater (not included in this model) may dominate over this 10% effect.

In summary, the manuscript should not be published without properly fixing both water mass bias and the vertical exchange. Even with these fixes, substantial overlap with prior modeling work may limit new and original insights that warrant publication.

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