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Geoscientific Model Development Discussions

Interactive comment on "Explicit and parametrised representation of under ice shelf seas in a z* coordinate ocean model" *by* Pierre Mathiot et al.

Anonymous Referee #1

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Recommendation: Minor revision

The authors describe the implementation of ice-shelf cavities in the NEMO ocean model, and assess its behaviour in (1) the idealized ISOMIP framework, including sensitivity experiments and comparisons to previous modelling results, and (2) "real ocean" 0.25° simulations around Antarctica, including comparison to observational data and sensitivity analyses. Interestingly, they also present a new way to parameterize the input of ice-shelf melt water into ocean models with no explicit representation of cavities. They show that such parameterization is able to capture the ice-shelf influence on sea-ice thickness and on the ocean circulation over the continental shelf, which sounds promising for coarse climate models.

This is a substantial piece of work that clearly describes the implementation of ice shelves in NEMO, but that is also very useful beyond the NEMO community. The comparisons and sensitivity tests are conducted in a robust way, and the results are generally well presented and discussed. I have a bunch of minor comments that will hopefully contribute to improve the paper, but no major objection, and from my point of view, the paper is already quite good as it is.

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Minor comments:

- The authors should make clear that their "parameterization" parameterizes the way to distribute ice-shelf melt water and therefore the circulation induced by ice shelves, but does not provide the amount of melt water. It is important to clarify this because readers from the ice-sheet or paleo-climate communities would probably expect an "ice shelf parameterization" to provide melt rates or melt fluxes. This is currently very clear in the conclusion, but maybe not enough in the text, and the title might be misleading.

- In section 2.2.1, it is assumed that the ice shelf is "in hydrostatic equilibrium in water at the reference density isf, taken to be the density of water at a temperature of $-1.9°C$ and a salinity of 34.4". Can the authors explain why they make such assumption?

- Section 3.3: what would happen in case of a "Losh TBL" thicker than the vertical resolution? Then, in Fig. 3, the authors show the effect of using 31, 46 and 75 levels based on standard stretching parameters. They conclude that 75 levels might not be enough, but they don't issue any recommendation on how many levels should be used in standard NEMO simulations. Including greater values in Fig. 3 (e.g. L100, L150) would be useful for the community. Finally, these sensitivity results likely depend on the slope of the ISOMIP ice draft, and the authors should probably discuss the generalization of these results.

- The year/time-period represented in the "real ocean application" is not clearly stated.

As far as I understand, the results represent 1985, which is presented as sufficient to complete a 10-year spin-up and to give the first-order response to changes in ice shelf representation. Does it mean that the interannual variability is of secondary importance compared to the sensitivity to the representation of ice shelves? What about the comparison to the ice-shelf melt estimated by Rignot et al. (2013) that is undertaken in section 5.6? How strong is the interannual variability in basal melt, and can we expect melt rates in 1985 to resemble those in the 2000s? I do not expect a perfect match here, but at least, the possible limitations should be stated.

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Other very minor suggestions & typos:

- Abstract, 5th sentence: “decrease” -> “decreased” or “decrease in”.

- Section 2.2.2: expand “ISOMIP”.

- Section 2.2.2, after equ. 14, expand “tbl” and mention that it’s defined further in the text.

- Tab.1: heat capacities should be in J/kg/K. And it would be better to use the Greek letter for Rhoi (as in the equations).

- Section 2.3, 3rd paragraph: I would replace “equilibrium depth” with something clearer like “floatation depth” if it’s what the authors mean.

- Section 2.3, last paragraph: expand “fwf”.

- Section 3.1: please add some information about the initial state and T,S restoring if any.

- It would be better to have the labels for the x-axes in Fig.2a,b . Also, (a) and (c) are swapped in the figure caption.

- Section 3.2, about refreezing: is there any frazil formation in the water column?

- Last sentence of section 3.3 (about Fig.3b): another reason could be that overturning and barotropic circulations have physically the same dependence on total melt rates.

- Section 5.1: the authors need to tell a bit more about how tidal mixing data from FES 2012 are used in NEMO, and maybe how it accounts (or not) for the effects of tides on ice shelf melt rates.

- Section 5.1, about “The model is run for 10 years starting in 1976, and the first order response is investigated using output from the last year of the simulation”: given that there seems to be interannual variability in these simulations, why analysing only one year? Isn’t there “first order” variability at the interannual time scale?

- Last sentence of section 5.2: “results from R_MLT are used to evaluate the 3 equation ice shelf melting formulation in NEMO” -> I think it’s not only the 3 equation formulation that is evaluated, but also the bathymetry, the ocean thermal forcing, vertical mixing, etc, etc. Same comment for the first paragraph of section 5.6 (although it is clear in 5.6.3).

- Fig.9: could the difference between Dutrieux et al. (2014) and NEMO simulations come from different periods under consideration?

- Section 5.6.3: a reference to Millan et al. (GRL, 2017) could be included to highlight uncertainties in bathymetry and ice drafts.

- Section 5.6.3: Makinson et al. (GRL 2011) estimate that tides double the net melt rate underneath FilchnerâĂŘRonne Ice Shelf.

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