Review of Mathiot et al. "Explicit and parametrised representation of under ice shelf seas in a z * coordinate ocean model"

Reviewer: Anonymous Referee #1

Recommendation: Minor revision

General comments:

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.

Authors: We would like to thank you for the very constructive, positive and encouraging comments. You will find below a reply to each point in *italic blue* with, when necessary, the new text between quote.

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.

About your point on the misleading title, the title has been changed to mentioned we parametrised the impact of under ice shelf seas. The second reviewer and the editor mentioned the text should include the model name and version. The new title is "Explicit representation and parametrised impacts of under ice shelf seas in the z coordinate ocean model NEMO 3.6".*

We do not modify the rest of the text as we think it is quite clear in the text as it is:

Abstract:" Mimicking the overturning circulation under the ice shelves by introducing the meltwater over the depth range of the ice shelf base, rather than at the surface, is also assessed." In this part we do not mention we will assess a parametrisation of the melt rate.

Section 2.3: "In this part of the study we focus on how to inject the observed ice shelf meltwater flux into the ocean model. Therefore, the ice shelf melting is prescribed and the heat flux is derived from the freshwater flux using Eq. 8. The computation of the melt rate from the off-shore ocean properties and ice shelf geometry could be included using the BG03 parametrisation or some adaptation of the Jenkins (2011) plume model, but testing these interactive melt parametrisations is beyond the scope of the study."

Section 5.5: "The parametrisation directly addresses this latter feature of the sub-ice-shelf ocean circulation and so is able to represent the ocean dynamics associated with the overturning circulation within the cavity." As for the abstract we do not mention that it is a melt parametrisation.

And as you mentioned, it is quite clear in the conclusion: "We do not describe a way to compute the melt rate itself. To tackle this issue, this work needs to be combined with a parameterisation of ice shelf melting (for example: Beckmann and Goosse, 2003; Jenkins et al., 2011)."

- 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?

In the ISOMIP case, this assumption is used as the initial condition of ISOMIP are -1.9C and 34.4 PSU. In realistic case, we kept this value by simplicity. -1.9°C is a good estimate of the water temperature at an ocean/ice interface. 34.4 PSU is a good estimate of the mean salinity over the Antarctic continental shelf. The mean salinity from WOA2013 between 0-1000m everywhere the bathy is shallower than 1000m and south of 55S is 34.42. Text is now: "We assume the ice shelf to be in hydrostatic equilibrium in water at the reference density ρ_{isf}, taken to be the density of water at a temperature of −1.9°C (freezing point) and a salinity of 34.4 PSU (mean salinity over Antarctic continental shelves)."

- 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 text to described what is happening in the Losh boundary layer is described in section 2.2. It has been reformulate like this: "Following L08, the noise due to the spatially varying size of the top cells is suppressed by computing T_w and S_w in Eq. 7,9 and 10 as the mean value over a constant thickness, assumed to represent the top boundary layer thickness (H_{TBL}, i.e. properties are averaged over the cells entirely included in the top boundary layer and a fraction of the deepest wet cell partly included in the top boundary required to make up the constant H_{TBL})."

The case where HTBL larger than the horizontal resolution has also been highlighted by the reviewer 2. Result of experiment with a resolution of 5m and 10m with a Losh boundary layer set to 30m are now described in the text.

The new text is "The choice of vertical resolution and Losh H_{TBL} strongly affects the ice shelf melting. When H_{TBL} is tied to the vertical resolution, finer resolution gives lower melting. Under melting conditions, a thin, fresh and cold top boundary layer appears in the top metres of the ocean next to the ice shelf base. With finer vertical resolution, a thinner and colder top boundary layer can be resolved, resulting in weaker melting (Fig. 3a). Our sensitivity experiments show a maximum melt rate 4 times higher in the I_150M simulation (4.3 m y^{-1}) and 3 times higher in the I_60M simulation (3.1 m y^{-1}) than in the I_5M simulation (0.9 m y^{-1}). In analogous experiments, L08 found a similar sensitivity, with maximum melting 3 times larger at 45 m resolution than at 10m resolution. However, when H_{TBL} is kept constant (I_5M30M, I_10M30M and I_30M), the total melt is insensitive to the vertical resolution. The total melt at high vertical resolution (5 m or 10 m) with a 30 m Losh top boundary layer thickness (respectively I_5M30M and I_10M30M) is converging toward I_30M (Fig. 3a). This suggests that a more physical definition of H_{TBL} (based on stratification, melt rate, etc …), rather than a constant $H_{TBL,}$ could significantly change the melt rate with a high resolution model (beyond the scope of the paper)."

About the recommendation and the test of other vertical resolution, we tested 31L, 46L and 75L because these resolutions are (or were) commonly used for global hindcast. We are not aware of higher vertical resolution commonly used with NEMO. Extra sensitivity test will be pertinent only if the stretching function used to build the vertical coordinates is suitable for a global configuration (such test are out of the scope of this study). References to the 3 variable level configurations mentioned are added:

"With variable vertical resolution (I_31L, I_46L and I_75L), such as is typically used in global configurations of NEMO (Timmermann et al, 2005, Drakkar group, 2007 and Megann et al., 2014), the coarsest resolution in the cavity seems to determine the total melt."

Generalisation of this work is really not straight forward as many factor could influence the results (slope of the ice shelf, coordinate system used, melt formulation …). This kind of generalisation will be maybe be tackle in the paper describing the results of the ISOMIP+ experiment (Asay-Davis et al., 2016).

- 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.

The end year has been added. And by the way we found a typo in the start date. The run started in 1979 and run for 10 years. The new text is : 'The model is run for 10 years starting in 1979 and ending *in 1988, and the first order response is investigated using output from the last year of the simulation.'*

In Jourdain et al. (2017), figure 2 shows clearly that after 5 years the fresh water flux from the melting reach an equilibrium state. Similar behaviour is found for cold and warm ice shelves (Fig. 5 in Timmerman et al., 2012). In R_MLT, the same is happening for Ross and Pine Island Glacier ice shelves (Fig. 1 in this review). After 5 years, the ice shelf melting is well span up (even after the first year it is mostly spin-up).

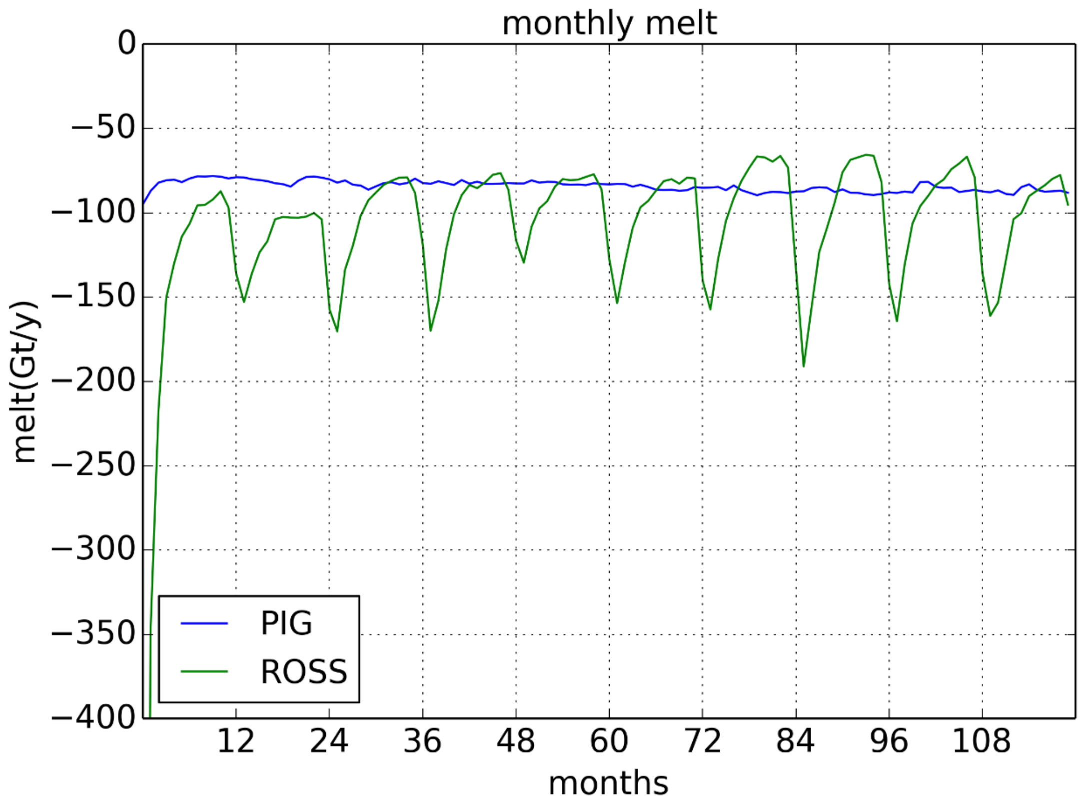

Figure 1: Monthly melt beneath Ross and Pine Island Glacier ice shelves in Gt/y.

About the melt rate in the last year and comparison with recent estimate, as the geometry used is a recent geometry (Fretwell et al., 2013) and the ice shelf regime (cold or warm) did not change over the last 40 years, we can reasonable assume that the model melting should match the Rignot estimates. Text has been added in section 5.6:

'The total ice shelf melting simulated in R_MLT (1865 Gt y^{-1}) is slightly above the range of the observational estimate of Rignot et al. (2013) (Table 3). In R_MLT, as in the observations, we can separate the ice shelves into two different regimes based on the temperature of the water masses on the continental shelves (Fig. 7d) and the average melt rate: the cold water (Fig. 13b-d) and the warm water (Fig. 13a) ice shelves. As the ice shelf cavity geometry is based on recent estimates (Fretwell et al., 2013) and the ice shelf regimes modelled in R_MLT are similar to those in the observations, the modelled ice shelf melting are expected to match the Rignot et al. (2013) estimates.'

About the ocean properties, in figure 2, we clearly show that the mean salinity over Amundsen Sea in front of Pine Island Bay is spin up after 7 years. Differences between R_noISF and R_ISF or R_PAR are much larger than the inter-annual variability. Comparison between R_ISF and R_PAR shows the same inter-annual variability in both runs. Therefore, it do not rule out the analysis and the conclusion we made in the run comparison. No change in the text.

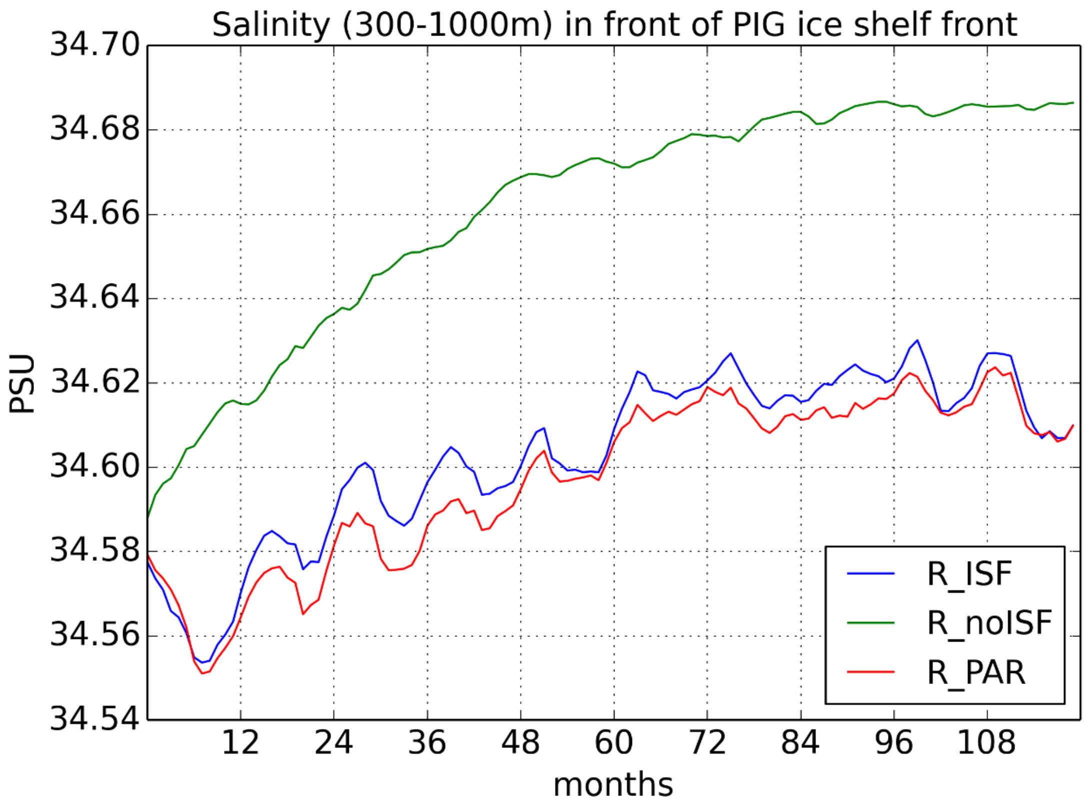

Figure 2: Monthly salinity (average between 300 and 1000 m depth) in front of PIG ice shelf.

Jourdain, N. C., P. Mathiot, N. Merino, G. Durand, J. Le Sommer, P. Spence, P. Dutrieux, and G. Madec (2017), Ocean circulation and sea-ice thinning induced by melting ice shelves in the Amundsen Sea, J. Geophys. Res. Oceans, 122, 2550–2573, doi:10.1002/ 2016JC012509.

Timmermann, R., Wang, Q. and Hellmer, H. (2012): Ice shelf basal melting in a global finite-element sea ice/ice shelf/ocean model , Annals of Glaciology, 53 (60) . doi: 10.3189/2012AoG60A156

Other very minor suggestions & typos:

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

- Section 2.2.2: expand “ISOMIP”. *DONE*

- Section 2.2.2, after equ. 14, expand “tbl” and mention that it’s defined further in the text. *Based on the comments from Xylar Asay Davis and your, we decided to expand the Tbl acronym in the text and we define for the section 2.2 the acronym H_{TBL} for top boundary layer thickness.*

- 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). *DONE*

- Section 2.3, 3rd paragraph: I would replace "equilibrium depth" with something clearer like "floatation depth" if it's what the authors mean. *By equilibrium depth, we mean the depth where the plume density equal the density of the ambient water. So, we think equilibrium depth is the correct word. "Floatation depth" could lead to confusion with the base of the ice shelf. Precisions are added into the text: "… thus an overturning between the grounding line depth and the equilibrium depth (the depth where the density of the plume is equal to the density of the ambient water)"*

- Section 2.3, last paragraph: expand "fwf". *DONE*

- Section 3.1: please add some information about the initial state and T,S restoring if any. We add the information in the first paragraph of section 3.1*: "The water is initially at rest and has a potential temperature of -1.9◦ C and a salinity of 34.4 PSU. No restoring is applied to either the temperature and salinity.*"

- 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. *DONE, fontsize was also changed to ease the reading and extra simulation point added.*

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

The refreezing occurs only at the ice/ocean interface. Furthermore, the properties at the ice/ocean interface in case of freezing (drag, exchange coefficient …) are the same as under melting conditions. Text in section 2.2 has been modified. New text is:

". Parameter values used in Eqs. 7-12 are defined in Table 1. Hereafter, Eqs. 10-12 are referred to as the "three equation" ice shelf melting formulation. At the differences of more sophisticated model (Galton-fenzi et al., 2012), the parameter used in the "three equation" formulation are not dependent of the surface state (freezing or melting) and the freezing only occurs at the ice/ocean interface."

- 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.

It could be, but it does not explain why the sensitivity of overturning and stream function are weak. No text change.

- 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.

The internal energy wave used in the parametrisation is derived from a barotropic model of the tides utilizing a parameterization of the conversion of barotropic tidal energy into internal waves. Under the ice shelves, the internal energy wave map is set to 0 by simplicity.

The new text: "The geothermal heat flux is assumed to be constant and set to 86 mW/m^2 (Emile-Geay and Madec, 2010), while the internal wave energy used in the tidal mixing parametrisation (0 under the ice shelf by simplicity) is derived from the tide model FES 2012 (Carrère et al., 2012)."

- 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?

There is inter-annual variability in our model, that is right. Figure 2 show clearly that the differences between run are larger than the inter-annual variability (R_noISF vs R_ISF or R_PAR) or that the inter-annual variability is very similar (R_ISF vs R_PAR). In the first case the signal we are looking at is much larger that the interannual variability. In the second case, this means that there is no "first order" variability at the interannual time scale. This does not rule out the conclusion we made on the performance of the simple parametrisation (R_PAR) compare to the standard case (R_noISF). No change in the text.

- 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).

We agree. The text has been changed to make it clear.

'Finally, results from R_MLT are used to evaluate the modelled ice shelf melting in our circum-Antarctic configuration using the "three equation" ice shelf melting formulation.'

"To compute melt rates for other oceanic states interactively, and eventually to couple the ocean model to an evolving ice sheet model, requires the "three equation" formulation for ice shelf melting. Next, we evaluate the ability of the described circum-Antarctic configuration with the "three equation" ice shelf melting formulation to modelled ice shelf melting."

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

Yes it could. Precision on the period of the climatology used and on the model year has been added in Figure 9 caption. New caption is: "Profiles (year 10, 1988) in Pine Island Bay in R_noISF (blue), R_ISF (red) and R_PAR (green) of a) salinity and b) temperature. Climatology from 1994 to 2012 (Dutrieux et al, 2014) is in black."

- Section 5.6.3: a reference to Millan et al. (GRL, 2017) could be included to highlight uncertainties in bathymetry and ice drafts. *We add a sentence to mention that bathymetric features are missing in the BEDMAP2 data set.*

The new text is : "The most recent bathymetry and ice shelf draft reconstruction of Amundsen Sea (Millan et al., 2017) shows large missing features in the BEDMAP2 data set. In BEDMAP2, for many ice shelves, there are only indirect observations of ice draft, based on satellite surface elevation data, while the sub-ice bathymetry is often poorly constrained. For some ice shelves (Getz, Venable, Stange, Nivlisen, Shackleton, Totten and Dalton ice shelves, for some of the thickest areas of the Filchner, Ronne, Ross, Amery ice shelves and for the ice shelves of Dronning Maud Land), the *floatation needs to be enforced by lowering the sea bed based on nothing more than extrapolation of cavity thickness from surrounding regions of grounded ice and 100 m thick cavity. Consequently, more data are needed for effective modelling (Fretwell, et al., 2013), because cavity geometry has a major impact on the simulated melting by controlling the water mass structure and circulation within the cavity (Rydt, et al., 2014)."*

- Section 5.6.3: Makinson et al. (GRL 2011) estimate that tides double the net melt rate underneath Filchner and Ronne Ice Shelf. *The citation we used in the text in section 5.6.3 and 5.6.1 was wrong. Instead of Makinson et al., 2012 we now used Makinson et al. (2011) (Makinson, K., Holland, P. R., Jenkins, A., Nicholls, K. W., and Holland, D. M.: Influence of tides on melting and freezing beneath Filchner-Ronne Ice Shelf, Antarctica, Geophys. Res. Lett., 38, L06601, doi:10.1029/2010GL046462, 2011.)*