Response to anonymous referee RC1 on "Assessment of Sub-Shelf Melting Parameterisations Using the Ocean-Ice Sheet Coupled Model NEMO(v3.6)-Elmer/Ice(v8.3)" by Favier et al.

April 26, 2019

In the manuscript, Favier et al., introduce the coupled model NEMO-Elmer/Ice and use it to assess the perfomance of sub-shelf melt parameterisations. This is done for an idealized setup of an ice-sheet and ocean system and using one coupled model. Although generalisation of the results to different geometries should be made with care, this study is of great interest for ice-sheet modellers relying on such parameterisations and reveals the advantages and disadvantages of the individual parameterisations ranging from simple linear parameterisation to the more complex plume and box model parameterisations.
We thank the reviewer for this positive comment

I have a few major comments on the manuscript:

Major comments:

• Figures 5 and 6: It is currently difficult to follow the reasoning of the main results because the figures are hard to read. The different model results are tricky to distinguish and partly not visible at all. My proposition is to split both figures into two figures each, one containing the information from the simple parameterisations and one from the plume and box model parameterisations. Some small changes could help to improve the understanding: (a) increasing/decreasing the dash length for the BME parameterisations with increasing number of boxes and for PME with increasing numbers, (b) if possible increased linewidth and legend font size would be great, (c) the ±50% range of ocean model results could be indicated in grey. In some cases, data is missing, e.g., in Fig. 5 panel (C) M+700 is missing after 70yrs, in panel (E) Mlin is hard to see, in panel (F) BM5,500 stops after 50 years. In Figure 6, panel (B) BM2,700 stops after 60 years, in (D), Mlin,700 is missing after 80 years and in panel (E), Mlin (red) is not visible.
We agree with the reviewer and have split Fig 5 and Fig 6 accordingly. We have also followed the other recommendations and added the missing results.

• Plume parameterization: More detail is needed: the explanation of why the melt rates show this patten of no melting near the grounding line and then increase towards the calving front without a melt-peak and decline afterwards is unclear to me, see also comment on page 17, lines 6-9. Given that its effective grounding line depth is always the central grounding line, I would expect the PME3 parameterisation to yield results along the center line y = 40km that are similar to a line plume model. In Figure 8 (g) of Lazeroms et al. 2019 (doi:10.1175/JPOD-18-0131.1) melt rates calculated with a comparable plume parameterisation and with a full plume model are shown for PIG with a melt rate peak around 15km and a decrease afterwards. Is this pattern different from the pattern you find here because of the higher T0 value used (1 degree at depth versus -1 degree)? If yes, how could this be improved?
This is a very interesting remark as it highlights the effect of the different choices in the implementation of the plume parameterisation. The PME3 accounts for the slope between the grounding line and the draft point as the effective angle, while the approach of PME1 (that is developed in Lazeroms et al, 2018, and also in the Lazeroms et al, 2019) accounts for the local slope. The two curves are thus difficult to compare, and also because they are the result of different environmental conditions of geometry, ambient temperature and salinity. In that sense, Fig. 8g of the Lazeroms et al, 2019, may not be the best to be compared with. Here below, you will see Fig. 1, which represents PME3 and PME5 (Fig C1 and Ap. D of the paper). The difference between PME3 and PME5 is in the definition of the effective slope, the local gradient being considered in PME5 instead of the slope between the grounding line and the draft point. We have chosen to represent the $Cold_0$ in Fig. 1 because the temperature of -1.5 is closer to -1.9 used in Lazeroms et al, 2019 (maybe you can consider their Fig. 5d to be compared with). To keep the paper clear, we have decided not to give those technical details

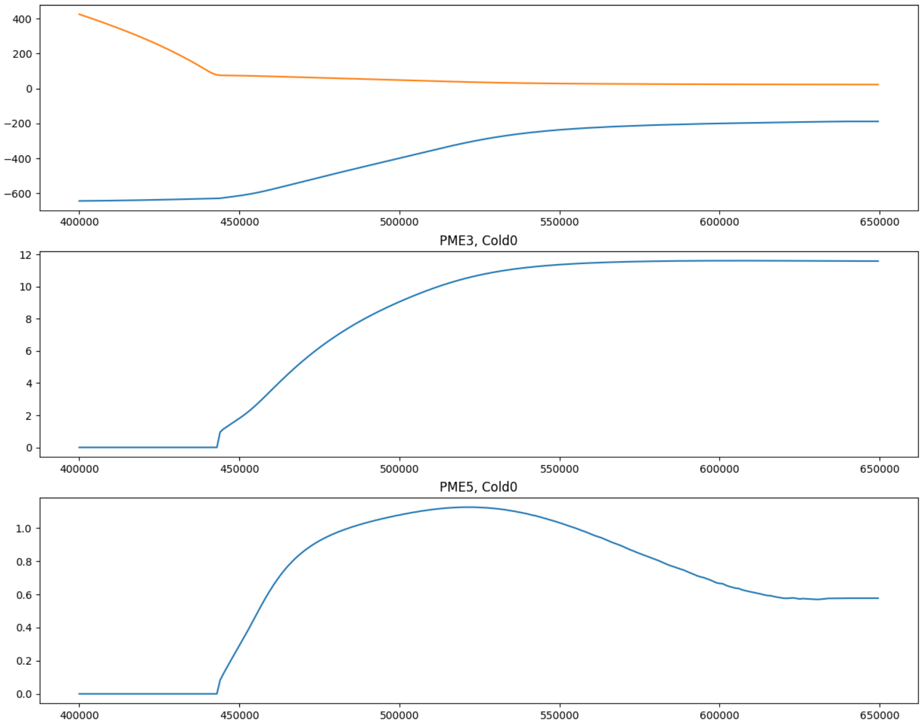

Figure 1: Melt rates obtained at $t = 0$ by the PME3 and PME5 parameterisations forced by the $Cold_0$ scenario. PME3 uses the slope between the draft point and the grounding line to calculate the effective angle, while PME5 uses the local gradient to do so. Ice shelf geometry in the top raw, PME3 in the middle raw and PME5 in the bottom raw.

in the paper. And, to conclude, we do have a melting peak in our simulations but the melt rates distribution will depend on the way the effective slope is accounted for, the temperatures, the thickness at the calving front, among other things, which for us opens the road to further investigations.

Minor comments

• page 4, line 12: Please check the reference to Schoof 2007, 'Ice sheet grounding line dynamics: Steady states, stability, and hysteresis'.
We have corrected the reference

• page 8, line 19-20: Please clarify which formulation you mean.
We mean when T_o and S_o are depth dependent, which we have clarified in the text.

• page 11, line 31: It would be great to have here a short explanation what the second parameterisation is about.
To clarify the text, we now only mention that the methods related to the plume parameterisation are all based on different calculations of effective values for the grounding line depth and the basal slope. All the specific details to each method are now in Ap. D only.

• page 12, line 5: Please add α in formula (7) to make it easier to understand its purpose, e.g., in Table 3.
Done

• page 12, line 9: You could refer here additionally to the Appendix where you explain the effective grounding line depth.

Done

• page 12, line 24: Please clarify that you explain the calibration of the coupled runs.
The initial melt from which start parameterised and coupled simulations is actually defined with an ocean spin-up (like depicted in Fig1) that is also the starting point of coupled simulations. We have rephrased this part to make it clearer

• Figure 3: Warm1 profile is missing in Panel (E). Generally, the details of this figure are hard to see. Could you maybe increase linewidth? And make the color schemes more intuitive by , e.g., using blue for the "Cold" scenario?
The Warm1 and Warm2 profiles are actually equal in panel E. We now mention it in the caption. For the rest, we have followed the suggestions of the reviewer.

• page 15, line 20: Maybe add the missing plots in a supplement.
We already have a lot of figures so we prefer not. Moreover, the parameterised melt that we don't show here are relatively easy to plot, as opposed to the more complicated pattern that we already show in Figure 4.

• page 15, line 30: The pattern in the TYP-10m experiment looks different from the other coupled runs as it shows melting at the opposite margin of the ice-shelf - why could this be the case?
It is not clear why the melting pattern of "TYP-10m" is different. The pattern suggests that more melting occurs when the warm water mass enters the cavity in TYP-10m (lower part of the plots), so that no more heat is available to significantly melt the ice along the outflowing jet (upper part of the plots). TYP-10m has a thinner TBL but stronger Γ_T coefficient. It is possible that the higher Γ_T coefficient make the exchange more efficient when the water masses enter the cavity, but the thinner TBL limits the amount of ocean heat immediately available for ice melting, so no more heat is available along the outflowing jet. Preliminary results of the ISOMIP+ intercomparison (Asay-Davis et al. 2016) indicate that both patterns are found across the existing ocean models. We have not added this discussion to keep the manuscript relatively short and with a clear focus.

And what causes the wave-like pattern in the basal melt rates of the coupled model?
The wave pattern is commonly found in the z-coordinate models taking part to ISOMIP+ (preliminary results). This is possibly related to the partial steps used for the upper levels, which makes that TBL averages can average one or several levels depending on the thickness of partial cells. This is an aspect of z-coordinate models that will need to be investigated.

• page 15, line 33: 'occurs' instead of 'falls'?
Changed

• Figure 4, Appendix D4: It's not clear why there is no melting in the area $y < 40$m for PME4: the algorithm (as described in the Appendix) would identify the closest grounding line point as the effective grounding line depth for points in this region. An example is shown in Figure E1 (C, example 2). I guess that those points are excluded based on the criteria for PME1?
For $y < 40$m, the melting are either 0 or low because either the unique direction found by the algorithm leads to a higher grounding line than the draft point, or the slope is low, respectively

• page 16, line 3: 'are similar by construction'.
Changed

• page 17, lines 6-9: Please clarify: I do not understand why a plume rising from only a limited number of directions reduces the melt rates, since, as explained in the Appendix and in Lazeroms et al., 2018, the effective grounding line depth is calculated as an average and similarly the effective slope is an average value (or the local gradient)? Also, I would expect the central grounding line point to be generally part of a 'valid' direction, since it is the deepest point of the ice shelf - how can then the melting at the 'inner sides' of the ice shelf increase, because the plumes can emerge from 'more deeper portions'? And third, it is not clear to me how a 'combination' of more plumes can generate higher melting towards the ice front? Shouldn't in this case, because plumes can emerge also from shallower grounding line regions, the effective (average) grounding line depth be shallower than close to the grounding line? Wouldn't in this case the thermal driving be also lower (WARM profile)? Then the higher melting must relate to the plume scaling and the dimensionless melt rate curve $M^{(}X^{)}$ or $g(\alpha)$?
You are right, the melting quantity doesn't depend directly on the number of directions. We have changed the sentences accordingly

• page 17, line 25: I think 'latter' and 'former' are switched.
Right, changed

• Figure 5: What causes the variations in the coupled model run in basal melt fluxes in comparison to the parameterisations?
These variations can also be seen in Figure A1. Given the absence of atmospheric and sea ice forcing, these variations must be related to an internal mode associated with the geometry of the closed ocean domain. For exemple, the 2-3 year period could be the typical advection time around the domain. In any case, this is very specific to the MISOMIP geometry and has not been investigated further.

• Figure 5 B: Some parameterisations show a decrease after ≈ 70 years. Is this because ice-shelf area is lost?
Yes, this is one of the reasons. This is why we set up the performance indicator at 50 years. We already mentioned this point in the first paragraph of the discussion

• page 20, line 18: Mquad,700 does not seem to do well for Warm3.
Right, modified

• page 20, line 26-27: '.. reflects the increase in thermal forcing compared to the depth dependent forcing.' I do not understand your statement here: the thermal forcing for 500m depth is lower at depth and higer towards the surface and seems on average to be comparable to the thermal forcing in the depth-dependent parameterisation (Figure 3)?
We have rephrased. The main idea here is that more thermal forcing does not mean more SLC, because the initial calibration step will result in a lower multiplicative coefficient (γ_T or α for the PME_i parameterisations).

• page 20, line 27-29: 'However, if the given...'. Please clarify: how does this statement relate to the result that in the 'Cold0', 'Warm3' experiments, the SLC for 500m is higher for all parameterisations while in the 'Warm0,1,2' all parameterisations using 700m have higher SLC?
We have removed this sentence

• page 21, line 24, Figure 7: Please indicate that the RMSE is calculated by summing the deviations of SLC over all experiments (if this is true).
The way it is calculated is written in the previous line in the main text

• Figure E1: If one doesn't know that the difference between PME1 and PME2 is how the calculation of the effective angle, it's confusing that Panel (A) shows both parameterisations.
Yes, we have added a sentence in the caption to recall that difference

• Appendix A: Please explain ub m and ub.
This was a typo. This is u_b in both case, but one is to the power of m

• page 27, line 13: Please define θ
Done here and also in the main text

• page 29, line 3: I think with 'checkerboard noise' you refer to Fig. 4 ?
Right, modified

• page 30, line 11: Since the formulas are not complicated, it would be helpful to add them here.
We have done as suggested.

Technical issues

• page 1, line 20: 'ice mass loss' and 'ice-shelf thinning' are exchanged?
Indeed, modified
• page 2, line 3: 'lowering of grounded ice surface'?
Modified
• page 4, line 4: 'controlled by Glen's flow law'.
Changed
• page 4, line 9: switch 'Seroussi and Morlighem, 2018' and 'equivalent to the SEP3 method in'.
Right, modified

- page 7, line 23: ‘this’ too much.

Modified
- page 11, line 17: ice-shelf basal slope θ

Done
- page 11, line 25: Appendix C.

Done
- page 12, line 25: Figure 3.

Changed
- Figure 3: Panels (C), (D), (E) are switched to (D),(E),(F).

Done
- page 31, line 4: anti-clockwise

Done
- Figure E1: ‘used in the present paper’.

Done
- page 23, line 34: ‘Ekman pumping’.

Done
- page 24, line 33: ‘multiple’.

Done
- page 27, Appendix B should be Appendix C.

Done
- page 31, Figure E1 should be D1.

Done
- page 32, Sections E1 and E2 should be D5, D6.

Done
- page 33, Appendix F should be E.

Done
- page 33, Figure G1 should be F1 and ‘Nico-’ can be deleted in the title.

Done.
- page 34, Appendix G

Done.
- page 36, line 20: ‘received’ too much.

Removed