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Geoscientific Model Development Discussions

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Interactive comment on "Assessment of Sub-Shelf Melting Parameterisations Using theOcean-Ice Sheet Coupled Model NEMO(v3.6)-Elmer/Ice(v8.3)" *by* Lionel Favier et al.

Anonymous Referee #2

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The authors present the first results from a newly developed offline coupling between the ice model Elmer/Ice and ocean model NEMO. They use the standardized MISMIP/MISOMIP intercomparison framework to assess the impact of ocean melting on ice dynamics for a range of (idealized) future ocean conditions, demonstrating the capabilities of their model. They then use these (sophisticated) coupled ice-ocean results as a benchmark to assess the performance of a range of (simplified) ice-shelf melting parameterization that are commonly used in a stand-alone ice models.

This is a solid piece of work, which recognises the importance and complexities of simulating ice-ocean interactions in Antarctic ice shelf cavities, and the need for care-

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ful model development and validation. The manuscript presents the first comparison between results from a coupled ice-ocean model and a comprehensive set of commonly used meltrate parameterizations, making this a timely and valuable reference for further research. The model development sections and appendices contain sufficient amounts of (technical) detail, the methods are sound and in line with previously published developments, and the experiments are explained in a comprehensive way. The authors demonstrate the capabilities of their new coupled ice-ocean model, and pave the way for further research. I highly recommend this work for publication in GMD, although I would like the suggest a few points for further clarification/improvement.

1) For the calibration of the melt calculations you use the WARM ocean conditions, but adopt different criteria to fix the exchange coefficients in the coupled (<melt>=30m/yr below 300m) and parameterized (<melt>=8.5mr/yr over the entire ice shelf) melt calculations. This -somewhat ad hoc- choice of calibration subsequently has large effects on the results (comparing Figure 7 and H1) and it therefore seems rather important. Could you please clarify why you do not adopt a universal calibration for all (parameterized and coupled) methods, e.g. <melt>=8.5mr/yr over the entire ice shelf and what would inform such a calibration? For a universal calibration, the initial differences in SLC between different melt calculations are fully attributed to differences in the spatial distribution of melt, at least in the WARM scenario, rather than spurious effects due to the calibration method.

2) On a similar note, I wasn't expecting the spread in total melting between different melt parameterizations (Figure 5) to remain more-or-less constant through time. I was expecting a divergence, with most parameterizations doing progressively 'worse' over time, compared to coupled simulations. Do you have any insights in to why that is?

I understand that the initial spread is defined by how sensitive the melt parameterizations are to the forcing for a given geometry, with the constraint that total melting is the same for all parameterization in the WARM (calibration) scenario. To disentangle the geometrical feedbacks and the initial sensitivity to ocean forcing, I think it would be

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instructive to see a plot similar to the panels in Figure 5, but for the WARM scenario. By nature of your calibration, all simulations should agree on the total melting at time 0, and (perhaps) divert for t>0 due to ice-ocean feedbacks. By imposing a common starting point, you will be able to unambiguously identify which parameterizations are ‘close to’ or ‘far away’ from the coupled simulations for that particular forcing.

3) About the RMSD criterion (Figure 7) that you use to assess the performance of parameterized melt rates compared to coupled ice-ocean simulations, I wonder if this criterion might be too simple and perhaps even misleading. As you explain, the spatial distribution of melt rates for a given geometry will, to a large degree, control the dynamic response of the glacier. However, this is not clearly captured by the RMSD criterion. In particular, you could identify parameterizations with a low RMSD as ‘performing well’, but the spatial distribution of melt could be totally wrong, and therfore the RMSD is low for the wrong reasons. As a result, this parameterization might not be suitable for other (more complex) geometries. Perhaps a simple RMSD criterion should be supplemented by a measure of how well the spatial melt distribution compares to the coupled scenario, making the assessment more robust. To achieve this, it would be instructive to see Figure 4 but after 50 years of simulations.

Finally, here is a list of some smaller comments, typos etc.

p1, l15 shelt → shelf

p4, l19 ‘floating nodes only’: please clarify if you impose melt for nodes in partially grounded elements

p5, l1 different ocean models will use different ways to parameterize a ‘boundary layer’ and calculate u_TLB. Perhaps you could be more specific here about u_TLB, unless this methodology has been published elsewhere?

p5, l17 do you always average over the entire coupling period, or do you use the final week/month/... of that period?

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p5, l22 ‘too thin to be captured by NEMO’: could you be more specific please? Do you impose a minimum water column thickness? Do you adjust your geometry to allow for this etc?

p6, l10-12 You say you have shown convergence with coupling timestep, but in Figure A1 all results fall on top of each other. To fully show convergence, you need to present results from eg 48 and 24 months, and show that they ‘converge’ to the solution for 12, . . . months. On a similar note, you present results for 1 particular scenario. In the caption of Figure A1 you say this is the COM-Ocean1 experiment, but I’m not sure if you mean Ocean1r or Ocean1ra? As the total melt goes down over time, I’m assuming this is a 1ra scenario with cold forcing? This could be important, as convergence might be harder to achieve in a warm ocean scenario?

p6, l17 As the calibration procedure is so important, perhaps you could provide a 1-sentence summary of the protocol from (Asay-Davis et al., 2016)?

p6, Table1 Of course this list of experiments is not/cannot be exhaustive, but it would be nice to have some more motivation for the choice of these specific experiments. Do they somehow represent end-members that allow you to put generous ‘error bars’ on the coupled results? For example, why are you interested in constant tidal velocities, knowing that Pine Island is not subjected to strong tidal currents? What defines T_CPL and why is it different for the different scenarios, given that you claim ‘convergence’ below 12 months. Why do you not consider changes in the drag coefficient, etc?

p8, l10 ‘inspired BY Jourdain et al. (2017) WHO ...

p10, l9 ‘to the local pressure’ → ‘ON the local pressure’

p12, l25 do you mean figure 2?

p14 Perhaps you can point out that you do not impose any ‘density compensation’ by changing the salinity for each temperature profile, so the density profile will be different in each scenario. Figure 3. The labels for the different sub figures got mixed up in the

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caption. I guess D should be C, E should be D and F should be E?

Figure 4. In the top row, one panel shows contours. Are they ice draft?

p17, l9 conbined → combined

p17, l20-25: How do you explain this difference in timescale between the initial melting pulse in the parameterized vs coupled case?

p17, l25: Do you mean 'lasts longer for the former, about 20a, than for the latter, about 5a'?

p17, l26: 'a melting minimum': This is not entirely obvious to me, it is certainly not 'universal', e.g. PME1 and BM5_500 seem to be monotonically decreasing?

p17, l30: surface → sea surface

Figure 5. Caption: 'Same as figure 6': please describe here, and refer in the caption of Figure 6 to Figure 5. The solid light grey curves are very hard to see, so I would consider using a darker shade or a different colour. Also, I would like to see these curves included in the figure legend. I'm also finding it impossible to distinguish between Mlin, PME1 and PME4 as they all print in pink. The same happens for Mquad and PME2. Perhaps you could consider expanding your range of colours, at the risk of making this figure even harder to interpret?

p20, l5-7 I'm a bit lost here, could you please reformulate this statement?

p21, l5-10: do results converge with a larger number of boxes, or is there an optimal choice for the number of boxes?

p21, l22 and Figure 7. It's unclear to me why you chose 50 years as a time for your performance indicator. As you have 100 years of simulations, why not use 100 years? Also, is there any way you can account for the difference in initial pulse, which lasts 20 years in the parameterized cases compared to 5 years in the coupled setup? This difference might severely bias your performance indicator, altough it is probably a constant offset.

p26, 27: Appendix B is used twice, please check the numbering

p31, l3: remove ‘the’

p31, l4: anto $\rightarrow$ anti and remove ‘until’

p33, Appendix F: I’m not sure where this appendix has been refered to in the main text

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