

Authors' comments on the reviews of  
“Response to Filchner-Ronne Ice Shelf cavity  
warming in a coupled ocean—ice sheet model.  
Part I: The ocean perspective”

R. Timmermann and S. Goeller

August 10, 2017

First of all, we would like to thank both reviewers for their careful reading of the manuscript and their helpful and constructive comments. Your input is really appreciated.

In the following, we quote the reviewers' comments in *italic* typesetting, followed by our replies. New text added to the manuscript or modified from the original manuscript appears in **bold**.

## Anonymous Referee #2

### Specific comments

*There is no comparison between the model and temperature observations, is it because there are no observations in this region or because such a comparison has already been done with previous versions of the FESOM? I imagine it is a big challenge to have a realistic ice shelf thickness and position of the grounding line because it might be highly sensitive to mean biases of ocean temperature and most ocean models have up to a few degrees of ocean temperature bias. What is the reason here for the good position of the grounding line? Is it because RAnGO has very little temperature bias or because there is room to tune the melt rate relations or because temperature is not so important in the melt rate or grounding line position change have a long time scale? It would be interesting to discuss this point in the manuscript.*

Data are indeed scarce for the Weddell Sea continental shelf. CTD sections close to the ice shelf front (see, e.g., Makinson and Nicholls, JGR 1999) indicate that most of the water column is at a temperature near the surface freezing point (i.e. between  $-1.8$  and  $-1.9^{\circ}\text{C}$ ), with a few warm (up to  $-1.4^{\circ}\text{C}$  in an episodic inflow of MWDW west of Berkner Bank) and cold (down to  $-2.2^{\circ}\text{C}$  in the ice shelf water plume on the western flank of Filchner Trough) exceptions. For an ocean model coupled to a sea-ice model, this is a comparatively easy situation: As long as the atmosphere is cold enough, the sea ice model together with a parameterization of convection will make sure that the bulk of subsurface water masses on the continental shelf will not deviate far from the surface freezing point temperature. The typical temperature bias for subsurface water masses in present-day climate FESOM and RAnGO simulations for the region discussed here does therefore not exceed  $0.2\text{ K}$ .

We added the statement **For present-day climate, the model yields ice shelf basal melt rates, ice thickness, and grounding line location in good agreement with observations.** as the second sentence in Discussion and Conclusions.

*How realistic is the little increase of ice shelf area during the A1B scenario? One could expect that calving would also increase and the area of ice shelf could reduce. How is calving modelled and how dependent on ice shelf thickness is it?*

To some extent, the moderate grounding line retreat in RAnGO is clearly a

matter of time scales. Considering that the warm water inflow starts towards the end of the 21st century in the RAnGO A1B simulation, there is little more than one century for the ice sheet to respond to the changing situation. First response is a thinning of the ice shelf, then with some delay the thinning of the grounded ice starts. The magnitude of the delay will be shown and discussed in Part II of this paper.

Note that previous studies modeling the impact of a warm water inflow underneath the FRIS with a one-way (ocean-to-ice) coupling have found a similar increase of ice shelf area (Mengel et al., Nature Climate Change 2016), so we are not 'out of bounds' here.

Similar to many ice models, calving in RIMBAY is implemented in the following way: If the ice thickness at the ice shelf front drops below a predefined threshold (100 m in our model runs), the ice shelf at this location is considered to have become unstable and have calved. Consequently, the ice will be removed and the grid cell assumed to be open ocean. At all other places, we keep the position of the ice shelf front constant and regard the mass flux through this front as a continuous calving process. In our model runs, ice shelf thickness never drops below the 100-m threshold, so that the calving-event criterium never applies.

*Also there is no comments about the feedbacks with the ice sheet. I understand this will be discussed in detail in a following paper but it would be nice to say a word also here. The thickness change in the ice shelf depend both on ocean melt rate and on ice inflow from the ice sheet, there are probably feedbacks between these because it is said in the manuscript that an increased vertical slope of the boundary between ocean and ice shelf increases ocean currents along the ice shelf which increases the melt rate.*

There are many ice streams feeding into the Filchner-Ronne Ice Shelf and they all have very different properties (ice velocity, ice thickness, bed slope gradient underneath the ice stream, lateral existence of buttressing mountain ranges, accumulation, proportion of ice mass above floatation where grounded, basal melt rates where floating), so it would be an oversimplification to discuss the feedbacks as a side note or in a 'rule of thumb' way - and more than that would go far beyond the ocean focus of this manuscript. In Part II of this paper, we compare all the relevant ice streams by their properties and discuss why only the Institute and the Support-Force Ice Stream show a thinning of the grounded ice and others do not.

*How does the discussion of section 3.3 relate to the concept of Marine Ice Sheet Instability? Is there locations of reverse slope bed in this region? I expect that the conclusion of this study that to first order using a model with constant ice shelf topography is fine, would have been very different if the ice sheet had been on a reverse slope bed. A discussion of this point in the discussion and a comparison with other ice shelves would help put the results of this manuscript in context.*

This as well is something we promise to adress in Part II of the paper.

*The ocean part of the RAnGO model is only in contact with an ice sheet model in the Filchner-Ronne region, how is the interface between the ocean and the ice shelves modelled in ocean regions? In an A1B scenario one would expect that freshwater would be entering the ocean in other regions, could this have an effect on the local ocean circulation?*

All other ice shelves in the coupled model are treated in the classical way with fixed geometry, so their meltwater flux responds to changes in ocean climate the same way as in fully uncoupled FESOM. Weddell Sea inflow in our setup has very similar properties between the FESOM and RAnGO simulations, so that differences in FRIS melt rates can be safely attributed to the representation of ice dynamics in this sector.

Timmermann and Hellmer (2013) discussed the effect of increased meltwater flux from FRIS on basal melt rates for Larsen C ice shelf as an example of nonlocal interaction between different ice shelves. In principal, the same mechanism is active in RAnGO, but the difference in FRIS melt rates between FESOM and RAnGO is not big enough to affect the evolution of Larsen C melt rates in any significant way.

We added the statement **All other ice shelves are modelled with fixed geometry.** towards the end of Section 2.1 (RAnGO: Overview).

## **Small comments**

*p.3, l.4: “Is” should be “it”*

Done.

*p.4, section 2.4, the basal melt rates are averaged yearly, is there no seasonal cycle and would it influence the ice model?.*

A substantial seasonal cycle of basal melt rates is evident only close to the ice front, where some increase in melting is driven by summer warming of surface water. This, however, is far away from the grounding line.

Furthermore, the ice velocities at the transition between grounded and floating ice within the ice streams are about 500 m/yr on average. At the Filchner Ice Shelf front, ice velocities reach a maximum of about 1500 m/yr. If we assume the seasonal cycle of basal melting at any location to be (first order) sinusoidal, we obtain a horizontal ice thickness variation with a wave length of 500 m at the ice streams and 1500 m at the Filchner Ice Shelf front. Thus, these fluctuations are about one order of magnitude smaller than our horizontal model resolution. The application of a yearly averaged melt rate is therefore clearly justified.

*p.8, section 2.6, why does it take so much time to build the 3D grid? The surface grid is already computed, which steps are left? In a typical free surface ocean model with sigma coordinate like ROMS, the sigma coordinates adjust vertically at each time step, this is a very fast process.*

Indeed, if the variation is only in ice shelf draft or water column thickness, i.e. in adjusting the depths of coordinate levels, that's very fast. The time-consuming step is necessary (only, but in fact every time) if the surface mesh changes. We have a precomputed mesh that is adjusted to the actual cavity geometry by cutting off unneeded elements after each coupling step. This is a fast process as well, but it is only applied to the surface (triangular) grid. The time-consuming bit is the creation of the three-dimensional (tetrahedral) grid under that new surface mesh, because it does not correct the existing tetrahedral grid but creates a new 3D mesh even if only one surface element has been added or removed. We now have a more efficient machinery for this step in the pipeline though.

*p.9, l.5-6 why is figure 7 referenced here? It does not show the in situ freezing temperature.*

The citation did not refer to the freezing point but to the statement of the full sentence. We now reference the two panels (melt rates and ice draft) separately.

*p.11, l.10 "my" should be "by"*

Done.

*p.18, l.20-21, if some work does this parameterisation it would be helpful to reference it here.*

The classical case here is the parameterization of Beckmann and Goosse (2003), used by, e.g., Mengel et al. (2016), and many others. The sentence now reads **We conclude that parameterizing ice-shelf basal melt rates as a function of ice thickness, like in the widely used scheme suggested by Beckmann and Goosse (2003), is not necessarily a good approximation to the governing processes.** This also acknowledges the fact that BG03 is very convenient for long-term ice model spinups and may well be an appropriate choice there despite its limitations.