Reviewer’s comment on

“The Whole Antarctic Ocean Model (WAOM v1.0): Development and Evaluation”

by O. Richter and coworkers

submitted to Geoscientific Model Development (Discussions)

General comments

In this paper, the authors report on the development of a new, high-resolution model of the Southern Ocean including its ice shelf cavities. The inclusion of tides represents a major feature of this model and a significant progress in scientific model development. The paper discusses model design und the evaluation of results.

The paper is well written and presents a lot of useful information. Figures are clear and well crafted. I recommend to accept the paper pending revisions guided by the following specific comments. Note that numbers 12 and 19 are a bit more substantial.

Specific comments

1. Throughout the text, I felt the urge to add a significant amount of hyphens in composite terms like “eddy-scale circulation”, “large-scale models”, “present-day conditions”, “eddy-resolving horizontal resolution”, “nearest-neighbour method“, „Spin-up procedure“, „depth-averaged temperature“ etc. I trust this will be handled by the copyeditor at one of the final stages of publication, but I also encourage the authors to revisit these composits.
2. l. 23: To the list of papers trying to predict future changes, you may want to add Timmermann and Hellmer (2013).
3. l. 27: It may be worth mentioning that several coupled ice sheet--ocean models exist already: Timmermann and Goeller (2017) with global ocean (but regional ice sheet) is one example, your co-author KA Naughten runs another.
4. l. 29: "augmented" (like an add-on) does not quite match the fact that at least some of these models were designed with ice shelves included right from the start. Two of the early, pioneering models of this kind were Beckmann et al. (1999) and Timmermann et al. (2002).
5. l. 47: instead of “usually”, I would find “often” or maybe even only “sometimes” more appropriate.
6. l. 60: The statement that the model “includes all the model physics of state-of-the-art regional applications.” seems a bit daring to me, given that sea ice (which is commonly regarded as part of the ocean) is only roughly approximated in this model.
7. l. 119: “[polynyas] are critical to resolve accurate ice shelf melting in cold regimes”: That's what people say. In fact, it is quite en vogue to stress the importance of coastal or flaw

polynyas. And it is not totally wrong at all. If you do the budgets though (let's say: for the continental shelf of the southern Weddell Sea), it turns out that the leads in the (vast) pack add up more salt flux through sea ice formation than the (comparatively tiny) coastal polynyas. What does make coastal or flaw polynyas important indeed is the fact that they are persistent and stationary. See, e.g., Haid and Timmermann (JGR 2013). So that statement is not totally wrong, but maybe a tad on the simplifying side.

8. l. 149: “while Bedmap2 ice thickness data is mostly based on laser altimetry data from 1994 to 1995” Are you sure this is true? Bedmap2 is much younger than this. Please double-check.
9. l. 166: I believe it should be Z = H (cosG+i sinG) (with brackets)
10. l. 183 etc: In the “Resolution effects” section, I think it would be very useful to not only discuss resolution-caused changes, but also whether these bring modelled hydrography closer to or further away from observations. Maybe this is easier if this section is moved to the end of the chapter? No preference, just an idea. Judging from Fig. 4, I am not convinved that I find the statement "the model solution [....] converges with increasing resolution" fully justified. We are indeed far away still from an asymptotic behaviour. Which is probably true for the vast majority of models in use today, so I am not criticizing the model here.
11. l. 207: You may want to finish the sentence with “and the representation of narrow troughs at the continental shelf break (Nakayama et al., 2014)”
12. l. 220-222: This passage is not fully convincing. Stronger water mass transformation would (in my view) go via more or saltier HSSW - which (according to their statement a few lines above, and consistent with Fig. 6) is not what the authors find. How does the model form WSBW with S>34.8 if no HSSW with at least the same salinity exists? There has to be a source somewhere, and I do not agree that finding this source can be beyond the scope of this study.
13. l. 225-227, particularly with regard to “Which of the models is more accurate close to the surface and what is causing the differences is not clear.”: A purely observation-based data product (like the World Ocean Atlas) might help.
14. l. 232/233: “The z-like signature of ISW in the Ross Sea is likely caused by continued mixing of ISW from one ice shelf inside the cavity of another ice shelf downstream and this further supports the presence of ice shelf teleconnections.” I think the statement here could/should be more precise. The idea is that these patterns are signatures of meltwater originating from ice shelves upstream from Ross Ice Shelf, right? So, this would be meltwater from the Amundsen / Bellingshausen Seas? I am not sure whether this explains the structure in the Ross ISW, but the idea of a teleconnection between this and those is supported by the findings of Nakayama et al. (2020).
15. l. 239-248: To me it seems as if this whole paragraph calls for a model-to-data comparison instead of (or in addition to) model-to-model.
16. Figure 9: Having the ice shelf on the left of the plot in the section AND in the map would be nice.

17. Figure 9 again: Is the very sharp front in the (c) panels a simulation result or are we too close to the open boundary / sponge layer here?
18. l.283: I think it should be “in agreement or close to FOR others”
19. l.310: The finding that strong ice-shelf basal melting near the ice front in this model is a widespread feature needs some discussion in context with numerics / sigma coordinates. Is there any risk that the particularities of terrain-following coordinates create a certain tendency / bias here? If mixing is not carefully controlled and ideally rotated to density surfaces (instead following lateral coordinate lines), a spurious exchange between the open-ocean surface and the ocean in touch with ice the shelf base near the ice front may be something to keep an eye on, I think.
20. l. 313, “accuarate polynyas”: Whether these are better in terms of giving the correct buoyancy flux than a prognostic sea-ice model may still be debatable. So, compared to resolution and tides, this may be a weaker point in the list of strengths of this model. Personally, I would concentrate on the "real strengths", with tides probably being the leader here, followed by resolution, and tune down the enthusiasm about the model’s approach to sea ice processes. This may be a matter of scientific taste though and I do not insist that the authors follow my suggestion.
21. l 330: It is SUCH a pity that results from coarser resolution or deactivated tides are not shown!
22. l. 334, “spuriously low conversion rate of heat into ice shelf melting”: This point I don't see, because even if you have a spuriously low ocean-to-ice heat flux, the transport of warm water onto the continental shelf is still the same, isn't it?
23. l. 355-357: I will shamelessly advertise RTopo-2 here. That said, it is highly unlikely that everything in RTopo-2 is perfect.
24. l. 359: In the list of studies on interaction between sea ice and ice shelves, you may REALLY want to add Timmermann and Hellmer (2013).
25. l. 360-362, “This study, however, prioritises accurate polynyas by prescribing surface fluxes from sea ice observations. While this is likely to result in more accurate melt rates at the base of the ice shelves” : I am not sure I agree with this. Having the polynyas at the right places is a good step, but the fluxes computed from there are probably much less well constrained.
26. l. 367/368, “This design, however, has been chosen to simplify future efforts that aim to couple WAOM with models of Antarctic ice sheet flow”: This has just been said (two sentences back).
27. l. 406: “harness”: Sure? Maybe “harvest”?
28. l. 420: Limitations of using just one particular year over and over again as atmospheric forcing need to be discussed. Think of periodic modes of variability and how each of these modes is randomly sampled in one particular phase and then repeated over and over again.

19.08.2020, with apologies for the substantial delay,

Ralph Timmermann