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Geoscientific Model Development Discussions

Interactive comment on "The Whole Antarctic Ocean Model (WAOM v1.0): Development and Evaluation" *by* Ole Richter et al.

Anonymous Referee #2

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This is an interesting manuscript that describes some of the more technical aspects of a new circum-Antarctic configuration of ROMS. The novel features are that it includes sub-ice-shelf cavities and tidal forcing, and it is run at high resolution. The subject matter certainly fits the remit of GMD and I think that it could make a valuable contribution to the growing literature on modelling the ocean circulation beneath the Antarctic ice shelves. However, I have a number of concerns with the way the model has been set up and validated, and I think the authors should clarify the reasons for the approach they have taken and provide a more critical evaluation of their results.

Questions: 1) While terrain-following coordinates have a number of advantages, their performance over steep topography can be an issue. The authors apparently deal with this critical point in lines 95-106, but nowhere do they state the extent to which the

topography of the ice front and continental slope have been modified by smoothing, or the magnitude of any residual pressure-gradient errors that they would expect after the smoothing process. This is an important point, because in Figure 7 there are some very obvious artefacts in the model results that appear to be associated with topography. Could similar artefacts be affecting the properties and circulation at the continental shelf edge and at the ice fronts? If so, they could have a major impact on the results that have been presented.

2) The surface forcing is unconventional, in that heat and freshwater fluxes are applied rather than derived from atmospheric variables driving a physical parameterisation of the atmosphere-ice-ocean exchange. This has the advantage of removing the need for a sea ice model that can introduce biases, but has the potential to introduce its own biases. Presumably that is the motivation for the rather ad hoc adjustments made to the fluxes, described in lines 119-124? Those adjustments really should be more carefully described and motivated. Furthermore the use of wind stress without a dynamic sea ice model is questionable. Wouldn't it be more consistent to derive ocean surface stress from ice motion observations? While wind stress may be a reasonable proxy for surface stress where the ice is in free drift, free drift is a poor assumption in many of the regions of interest in this study, particularly the Weddell Sea and almost all near-coastal regions.

3) It is not obvious why the model has been forced with one year of data repeated, especially when that one year is characterised by "a paucity of observational data" (lines 10-11). Why not choose a year with more data? On lines 130-132 the authors state that the strategy of using one year allows them to integrate the model to quasi-equilibrium, but does it really reach that state in only 8 years? I think the authors should show some further diagnostics, such as domain-averaged temperature, salinity, KE, etc, to justify that statement.

4) It is also not clear to me why the authors have used one model (ECCO) to supply initial and open boundary conditions, then compared the results to a second model

(SOSE). Comparing with observations (however limited) would give some kind of indication as to how realistic the results are, while comparing to the same model (ECCO) would inform us about how the differing model architecture and surface boundary forcing impacted the model evolution. Without knowing how ECCO and SOSE differ, it is not clear what conclusions can be drawn from the comparisons that are made between WAOM and SOSE. The problem is highlighted by Figure 6, where WAOM and SOSE look completely different, particularly at depth. Is that because ECCO and SOSE are very different, or are the differences produced by the surface forcing? If it is the latter, how can the deep ocean have been so extensively modified in only a few years? It tends to suggest that the model is much too prone to deep convection, which is quite a common problem in Southern Ocean models. But does that happen everywhere, or is that all a product of those model artefacts that appear over steep topography (see 1 above)?

5) Figure 6 raises some other serious questions about the results. The ISW and RSBW/WSBW mixing lines appear to point to a water mass that apparently doesn't exist (HSSW). That suggests that they were formed from HSSW that was prescribed in the initial conditions, but has since been used up and not renewed. If that is the case, it points towards a model that is not in a quasi-equilibrium state (question 3 above), but is still in the process of evolving from initial conditions to some other state. Similarly the water masses at 1000 m depth are much too warm, suggesting that the original CDW prescribed in the initial conditions has also vanished and has been replaced by something quite different. That suggests some issue with the surface fluxes (question 2 above). It also suggests (again) that a longer integration would see the model continuing to evolve to a different state. If deep waters as warm as 3-4 deg C found their way onto the continental shelf, ice shelf melt rates would be much higher.

6) But perhaps the error is just in the plotting of Figure 6. The deep ocean stratification suggested by the trajectory of points from RSBW/WSBW to the (poorly placed) AABW label appears to be much too strong. I find it hard to believe that the whole domain

can have been so extensively altered in 8 years of integration (assuming the initial conditions taken from ECCO looked something like the SOSE results). Has some error been made in converting temperature to potential temperature, or density to potential density?

7) The problems with the water mass structure apparent in Figure 6 are also seen in Figures 7 & 8. On line 234 the authors state that "The stratification of WAOM agrees well with SOSE for the off-shelf ocean", but I would disagree with that. In most transects the main pycnocline is not well represented, either in strength or depth, and the mid-depth salinity maximum appears to be absent (worryingly consistent with Figure 6). While, this is far from being the only model to get the stratification wrong (it is notoriously hard to get right), I think the results presented here warrant more than the glib statement that they agree well with SOSE. In Figure 8 the absence of a source water mass for all the colder forms of ISW is again apparent, while ISW seems much too prevalent in the Amundsen and Bellingshausen seas, where it is hardly ever observed. Cooling waters too efficiently in the cavities hints at a problem with the balance of heat fluxes into the boundary layer (KPP) and at the ice-ocean interface. But again, some of these issues might arise because of the plotting, which makes the stratification look very odd. The densest waters are shaded blue (Figure 8), implying they are at the surface. How is that possible?

8) Throughout the paper there as in implication that higher resolution is intrinsically-better, but improvement in the model results with increasing resolution is never actually demonstrated. The implication of the discussion around Figure 4 (lines 184-191) is that at higher resolution still, shelf water temperatures and melt rates would drop further. However, at 2 deg resolution the mean melt rate has already dropped below the observed value and the 4 deg resolution simulation is arguably the best according to that single metric. On a related note, I don't understand what the authors mean by "convergence" in that discussion. This term normally refers to the ability of a numerical code to reproduce an analytical solution as the grid size tends to zero. What solution should

the model “converge” to in this case? The authors describe some features, cooling of the Bellingshausen Sea (line 196), for example, that appear to be worse in the higher resolution runs.

9) Again in Figure 10, it is not entirely obvious what has been gained by the addition of tides and the use of high resolution. Results are different from previous studies, but not obviously any better. The spatial distribution of re-freezing is rather poorly captured (Figure 11): almost nothing on Amery and Larsen C ice shelves, and very little in the central Ronne Ice Shelf. Many of the early (admittedly) regional models did much better, despite being run at much lower resolution. That again points to the fact that increasing resolution does not necessarily improve results. I agree that a higher resolution model has the potential to improve the representation of reality, because it can resolve more processes, but no model can resolve every process, and the key to getting things right at any resolution is knowledge of how best to parameterise whatever remains in the sub-grid-scale. Arguably the authors have done a better job at that with the 4 km version of WAOM than with the 2 km.

In summary, while this circum-Antarctic model has the potential to be a useful addition to the growing collection of such tools, I feel the authors should do more to critically evaluate their results and explain the impact of including extra processes and using finer resolution. The main issues at present that make the model results questionable for the applications that the authors have in mind are the problems with water column structure (that may be related to sigma-coordinate problems over steep topography) and the curious water mass properties (Figure 6). If the latter are not due to mis-plotting, then it suggests some serious issues with the model (potentially associated with the surface flux forcing and/or the sigma coordinates). Those really need to be sorted out before the model can be considered fit for purpose.

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