Geoscientific

Model Development

Discussions

[GMDD]

Interactive

comment

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

Anonymous Referee #1

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In this manuscript the authors describe a new regional model configuration (WAOM) that encompasses the entire Antarctic continental margins, including the cavities beneath Antarctica's floating glaciers. The scientific focus of the model is on simulating ocean-driven melt of Antarctica's ice shelves. The manuscript describes the set-up and integration of the model at three different resolutions (10km, 4km and 2km grid spacing), the highest of which is ostensibly capable of fully resolving tidal and fine-scale processes that contribute to the circulation and stratification on the continental shelf. The authors evaluate WAOM using an ocean state estimate and independent modeled/measured estimates of Antarctica's ice shelf melt rates, focusing on the highest-resolution solution. They concluded that WAOM acceptably reproduces the observed ocean stratification and ice shelf melt rates, and therefore is a suitable tool for address-

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ing scientific questions related to mechanisms of ocean-driven melt. They discuss shortcomings and sources of biases in the model, particularly emphasizing WAOM's lack of an active sea ice component, and discuss future development and scientific goals for the model.

My high-level evaluation is that this is a significant model development that is worthy of publication in GMD. As the authors note, this is the first ocean model with (borderline) tidal- and eddy-resolving resolution that includes all of Antarctica's ice shelves, and therefore offers insights into the role of these processes in modulating Antarctic ice loss at the continental scale. The description of WAOM is appropriate for a model definition manuscript, and configuration and analysis scripts are provided (remotely) to allow for complete reproducibility. The manuscript and figures are very clearly composed.

Below I have provided a series of comments and suggestions on the manuscript for the authors' consideration. My most significant concern pertains to their validation of the ocean state: 1. Offshore, the model is evaluated against the Southern Ocean State Estimate (SOSE), which contains biases of its own, particularly close to the Antarctic margins (see e.g. Dotto et al., 2014, Ocean Sci.). It was unclear to me why the authors chose to SOSE rather than directly against measurements. 2. On the continental shelf the authors perform only a qualitative evaluation of the ocean state, rather than performing direct comparisons. I was surprised by this, because the ocean state on the shelf is critical in determining the circulation and melt rates in the ice shelf cavities. Based on this, I would argue that the hydrography on the continental shelf should be the most closely scrutinized aspect of the model state. Previous studies have compiled measurement from the continental shelf from all around Antarctica in order to compute trends in shelf properties (Schmidtko et al., 2014, Science), characterize different dynamical regimes on the continental shelf (e.g. Amblas and Dowdeswell, 2018, Earth Sci. Rev.) and evaluate models (e.g. Morrison et al., 2020, Sci. Adv.). In my opinion, the manuscript would be strengthened significantly if the authors used one of these datasets to map biases in shelf properties in WAOM.

Comments/questions:

L4-5: How significant is lack of interannual variability? Is it possible that some of the biases in the modeled shelf stratification and melt rates occur because the model excludes anomalous years, e.g. years with particularly strong/weak winds or warm/cool atmospheric temperatures?

L61: "beyond the scope of this study" doesn't really mean anything. If it's not in the study then it's beyond the scope by definition. It would be more helpful to state why an evaluation matrix was not pursued.

Also is model tuning really "rigorous"? A right answer for the wrong reasons is not necessarily better than a slightly wrong answer based on well-grounded physical parameter choices.

L93: Horizontal grid sizes in the form M x N would be more relatable than total number of computational cells.

L98: Are the authors referring to the scheme of Shchepetkin and McWilliams (2003)?

L100, L304-311: How wide does the "vertical cliff" at the ice shelf face become with this smoothing? Does this bias the model toward more Mode 3 ice shelf melt?

L102: Is the algorithm applied to both the ice shelf draft and the bathymetry, or only to the water column thickness?

L105: Could the authors elaborate on "one of the smallest modifications possible"? I don't understand why there would be a hard limit on the ice shelf thickness - only an increasingly severe time step constraint as the thickness approaches zero.

L115-118: I presume that the authors impose heat and salt fluxes, rather than just buoyancy fluxes.

L116, L399-400: Imposing surface wind stresses directly with no accounting for sea ice is a significant caveat. For example, recent Arctic-focused work shows that sea

ice plays a significant role in modulating the stresses felt at the ocean surface, and the ocean surface currents (see Meneghello et al. 2018, GRL and other subsequent papers from John Marshall's group).

L117-118: Please explain why not using a sea ice model is more likely to capture polynyas? This is counter-intuitive.

L119-122: The authors have quite a few rather ad hoc changes to the surface forcing that warrant further explanation. Why do the authors reduce the positive heat flux into the ocean by half? Is this to simulate the sea ice albedo effect? I understand the physical motivation for changing the brine rejection and restoring surface temperatures from below freezing, but it seems inconsistent to do so when all other fluxes are fixed. How do the authors gauge that one month is a "long" time scale for surface relaxation?

L125-126: I am skeptical about the claim that the model state at the boundary is primarily dictated by the interior. Surely this is not a desired outcome, as remotely-formed water masses (especially CDW) need to be supplied by the open boundary conditions.

Fig. 2: It looks like the melt rate drops instantaneously upon re-initialization with a 4km grid spacing, and again with a 2km grid spacing. Is there some geometrical impact of the grid refinement that causes this, or is the adjustment time scale just shorter than the monthly frequency of the model output that was used to create the plot?

Eq. (1): Shouldn't the denominator be $1/N$. Also, if Z_j^m and Z_j^o are complex variables (Z is the complex amplitude), then shouldn't the complex magnitude be taken before squaring (or equivalently multiplication by complex conjugate).

Eq. (2): Shouldn't the denominator be $1/(4N)$? Also, shouldn't the Z_j^m and Z_j^o be indexed by k as well, to distinguish tidal components.

L175-176: Is a 2005 tide model still considered state-of-the art?

Table 2: I think I understand how the stated quantities (e.g. RMSD phase in deg) are related to equations (1) and (2), but the naming convention and formulation of

equations (1) and (2) make this much less clear than it could be.

L190-191: Should we expect the model state to asymptote under grid refinement? After all, various model parameters (e.g. viscosities/diffusivities) implicitly change with the grid, as does the bathymetry.

Fig. 4: Is 10km->4km grid spacing a 250% increase in resolution? If the resolution is defined as the number of grid points per km, then the resolutions are 0.1, 0.25 and 0.5 km^-1. So 10km->4km->2km grid spacings correspond to resolutions of 0.1->0.25->0.5 km^-1, so the resolutions have increased by 0%, 150% and 400% relative to the 10km grid. Or the resolutions have been multiplied by 100%, 250% and 500% relative to the 10km grid. Please pick a consistent convention!

L215-222: I was initially confused by the authors' explanation of the RSBW and WSBW salinities, which the attribute to the (ECCO2-sourced) open boundary conditions. I would have hoped that water masses formed within the domain would not depend on the boundary conditions. That said, the model boundary cuts right through the Weddell Gyre, so perhaps it is reasonable to have inflow of some bottom waters. Perhaps the authors could clarify this in the text?

I am very confused by the waters with salinities reaching 34.8 in Fig. 6. There is no other water anywhere in the model domain with such high salinities, and these high salinities are only found at great depth, far from the surface. So, what is the source of the very salty bottom waters?

Fig. 6: I suggest adding more lines/arrows to indicate water mass locations more precisely. The CDW label looks to be much too fresh (e.g. compare with Fig. 7), and AABW is labeled at a lower density than CDW!

Also, I presume the portions of T/S space labeled "Weddell Sea" and "Ross Sea" are actually the Filchner-Ronne Ice Shelf cavity and the Ross Ice Shelf cavity, respectively.

L235: Are the transects all annually-averaged? Are there significant deviations in the

agreement between SOSE and WAOM in different seasons?

Fig. 7: Why does the grid spacing in WAOM appear to be so coarse? The model grid spacing is 2km but the data points in this figure appear to be spaced 50-100km apart.

Fig. 8: Given that the authors have just compared a few transects here, why not align the transects with WOCE transects? Then they could include a third column of panels showing the WOCE measurements for additional reference.

Also, often these transects are visually very similar, especially with these colormaps. Difference plots would be more revealing with regard to the model biases.

L421: Has the model equilibrated in the higher-resolution cases? I don't see how the authors can judge this from just a one-year time series in the 2km simulation. I think it would be fairer to say that the model has equilibrated at 10km grid spacing, and has been continued from that equilibrated state at higher resolution.

L433: It's really the "surface" stress that is uncertain: the wind stress is relatively well constrained by reanalyses, whereas the ice-ocean stress is much less well constrained.

Table C2: Please specify which relaxation parameters pertain to the open boundaries vs the ocean surface.

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