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Geoscientific Model Development Discussions

Interactive comment on "A numerical study of the Southern Ocean including a thermodynamic active ice shelf – Part 1: Weddell Sea" *by* V. Meccia et al.

Anonymous Referee #1

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GENERAL COMMENTS

Review of "A numerical study of the Southern Ocean including a thermodynamic active ice shelf - Part 1: Weddell Sea," by V. Meccia, I. Wainer, M. Tonelli and E. Curchitser.

In this paper, the authors give a description of a coupled ocean circulation/sea-ice/ice shelf model (using the Regional Ocean Modeling System) setup in a circumpolar domain for the entire Southern Ocean with enhanced horizontal resolution in the Weddell Sea. Four experiments are performed: a full simulation, a simulation with the mechanical effects of ice shelves but no ice shelf/ocean thermodynamic fluxes, a simulation with no ice shelves at all and a simulation with no ice shelves and no sea-ice. The results of the four simulations are compared against each other to show the importance of the sea ice model and the ice shelf/ocean fluxes in accurately simulating the

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circulation and water masses in the Southern Ocean.

The inclusion of ice shelf/ocean interaction, along with sea-ice processes, is likely to be critically important in estimating the Southern Ocean response to future climate change. The results from these experiments are interesting (especially after a recommended change to experiment M2... see below) and a description of this model is well worth being included in Geoscientific Model Development. However, I believe much remains to be done to this study before it can be used to show the importance of the different cryosphere/ocean couplings and that this is a "promising tool for analyzing the Southern Ocean response to future climate change scenarios."

The authors state that their main goal is "identifying the relative importance of the cryosphere's components and their interactions with the ocean in the Southern Ocean" (pg. 4045) and especially the Weddell Sea. However, I don't believe that two (M2 and M4) of the experiments are setup properly to show this clearly:

1) M2: When first looking at the results from this experiment I was surprised at how much of a difference there was from just turning the ice shelf thermodynamics off (M2 vs. M1), especially since "Below the ice shelves, the atmospheric contributions to the momentum and buoyancy fluxes are set to zero" (pg. 4043). Looking at the temperature and salinity cross sections (Figures 5 and 6), I could not figure out how one could get temperatures well below the surface freezing point underneath the ice shelf (Figure 6) with no ice shelf thermodynamics or why the water in the ice shelf cavity was so salty. The cross sections show that on the open shelf, the sea-ice code is preventing the surface water from cooling well below surface freezing (as opposed to case M4) and I could not think of another heat sink that could get the ice cavity water that cold.

After going through the entire paper comparing the results of M2 to the other simulations, the authors finally mention at almost the end of the paper (pg. 4053) that "maybe" the heat and salt fluxes underneath the ice shelf "are being computed as if it was open water," contributing to the super-cooled temperatures and that the frazil ice

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parameterization in the code is producing the high salinities.

This seems like a big deal to just casually mention at the end of the paper. First of all, why "maybe"? Can't the authors look at the model code and see if this is the case? Second, if this explanation is correct, then experiment M2 is not going to allow "the evaluation of the importance of the heat and freshwater fluxes between the ocean and the ice shelf" (pgs. 4044-4045), but instead is going to show the rather unrealistic effect of ocean polynyas the size of ice shelves. It does not show the importance of the thermodynamics at the base of the ice shelf with respect to a perfectly insulated ice shelf base. M2 probably way over does the High Salinity Shelf Water formed, which will certainly have a huge impact on the overturning circulation (as shown in Figure 8 for M2).

The authors really should find out for certain if case M2 is acting as if there are open polynyas in the ice shelf locations. If so, then I think they should rerun case M2 with a properly insulated ice shelf. If the authors do not wish to rerun the simulation, then at the least either case M2 should be removed or the sections related to it rewritten explaining what it really represents. If case M2 is not acting as if there are open polynyas, then I think there needs to be a better explanation given as to why "very cold and salty waters result if only the ice thickness is included" (pg. 4053).

2) M4: Using a bulk flux formulation with no sea-ice leads to totally unphysical temperatures on some shelves (-15C, pg. 4046) that I suspect the equation of state in ROMS does not handle very well and who knows what densities are calculated. Once this water spills off the shelf, the circulation will be totally "wrong" as the authors show. However, I wonder if this test overemphasizes the need for a sea-ice model? Southern ocean models have been run before without sea-ice (including the referenced Matano et al. 2002 model) and have performed much better than case M4 because the surface fluxes were computed differently (usually relaxation to surface T and S).

I have several other comments and suggestions (listed below), but most of these are pretty minor. My summary recommendation is to return the manuscript to the authors for major revision including re-running experiment M2 if it is found that the heat and salt fluxes under the ice shelf are being computed as if it was open water.

SPECIFIC COMMENTS

4041/22: I think a more up to date reference for ROMS (e.g. Haidvogel et al., 2008) should be used.

4041/22-24: The time discretization given here is not quite correct for the 3D equations, see Section 5 and Table 1 of Shchepetkin and McWilliams (2009).

4042/6: Why are figures 1c and d presented before 1a and b?

4042/6-7: Any issues with having a 15-1 ratio in the grid size over the model domain? I know the BRIOS people were not worried about it, but their ratio (longitudinally) was only 4.5 to 1.

4043/1-3: I don't think the Jenkins et al. (2001) reference is appropriate here as it is more about an additional term needed for meltwater advection in the boundary conditions at the ice-ocean interface (for conservation reasons) instead of how the salt flux under the sea-ice is computed in a sea-ice model.

4043/13-14: By "depth below mean sea level of the ice shelf thickness", do the authors mean that the depth of the bottom of the ice shelf (meters below MSL) is set as the entire thickness of the ice shelf or just the portion of the ice shelf that is below MSL (most of the thickness, but not all)?

4043/16-18 and Figure 1b: The ice thickness for the Amery seems much too low. From Figure 1b, it looks to be < 100 m everywhere, while it should be > 400 m over most of the shelf and > 200 m over almost the entire shelf. Too much smoothing perhaps?

4043/25-28: Were any velocities or surface elevations used on the northern open boundary?

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4044/8-9: I think Fairall et al. (1996) is COARE2.5. Doesn't ROMS now use COARE3.0? If so, I think the reference should be Fairall et al. (2003).

4044/14-19: I think it would be helpful to include the "standard" (for Southern Ocean models anyway) diagnostic of the Drake Passage transport in order to see if the mean model ACC transport is maintained over the 100 year simulation.

4045/19-12: Why not just use the last 9 years of all four simulations?

Section 3, general comment: Since the purpose of this paper is to identify "the relative importance of the cryosphere's components and their interactions with the ocean in the Southern Ocean," I think it would be useful to compare the freshwater budget (or salt budget, since the surface flux in ROMS is a salt flux) by source (i.e. open water surface flux, sea-ice surface flux, ice shelf basal melt, surface relaxation term, northern open boundary) for the different experiments.

4046/2-17: Since all the parts of Figure 3 look pretty much the same (i.e. the comparison looks pretty good, especially since the minimum value of the scale is -1.0 and thus the really cold temperatures in M4 do not stand out), I think it would be helpful to do a more quantitative comparison (at least compute the RMSE with respect to the satellite data) to help differentiate between the experiments.

4046/18-25: Have the authors looked at a comparison of salinity deeper in the water column? I suspect M1 and M2 wouldn't look nearly as comparable as they do at 10m (see your different cross sections), at least on the Ross and Weddell shelves, helping to explain the big difference in the overturning shown later.

4047/22-26: This explanation makes no sense if the comments on pg. 4053 about the heat and salt fluxes are correct.

Section 3.2: Can the authors add an estimate of the Weddell Gyre transport in order to compare with observational based estimates? I guess it would be difficult to do for experiments M2 and M4, but I think it would be illustrative for M1 and M3.

4050/10-14: I think it’s important to also mention the interactions of the water with the base of the ice shelf when discussing the formation of ISW.

4050/20-22: If the authors want to show WSBW along with all the other water masses, why not do a T-S diagram over the entire Weddell sector instead of just along the 40W line?

4051/9-18: The winter extent comparison looks really good. I know there’s no good observational data to compare against, but how does the ice volume differ between M1, M2 and M3?

4051/18-22: I think this is misleading given the comments on pg. 4053 about experiment M2.

TECHNICAL CORRECTIONS

4043/6: Recommend changing "along" to "during".

4044/26: Recommend changing "they" to "the ice shelves".

4049/18: "fluxes is" should be "fluxes are".

Interactive comment on Geosci. Model Dev. Discuss., 5, 4037, 2012.