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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.

**V. Meccia et al.**

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Response to Referee #2

I would like to thank reviewer #2 for his careful review and thoughtful comments and suggestions. Our point-by-point response follows:

Answer to general comment

We agree with the reviewer that this paper is more technical and less innovative, but as far as we are concerned this is a first attempt into designing a Southern Ocean setup for ROMS that takes into account the ice-shelves. In order to be able to take advantage of including ice-shelves together with other capabilities in ROMS (such as the ecosystem and biochemistry modules) the simple experiments described are mandatory.



We also agree that the design of the experiments was a source of confusion and in fact, describing the setup and results for experiment M2 (ice-shelves with the thermodynamic interaction with the ocean turned off) served only to add uncertainty – we decided to eliminate the experiment M2 from the paper. It should also be clarified that once there is an ice shelf, we shut off the air sea fluxes.

"[. . .] However, this does not seem a very sensible experiment to me. The ice shelf should insulate the ocean from the atmosphere, and if open water ïňĆuxes are applied with the ice shelf in place, are they actually being applied in M1 in addition to the ice-shelf-ocean thermodynamics?"

The reviewer is correct that M2 was not a sensible experiment and thus stricken from the text and discussions.

"M3 [. . .] The authors choose the latter (ice shelves become open water). The problem with this option is applying surface ïňĆuxes. If you just take the atmospheric forcing as it stands you will over-estimate the sea ice formation that would take place in the absence of the ice shelves, because air temperatures are much lower over the ice shelf surface than they would be if that area were occupied by the ocean. The only way to deal with this would be to have some interactive atmosphere (maybe just a boundary layer model), but I assume that is not done here. The authors do not mention how they dealt with this issue."

M3 was run to address the effects of the ice shelf inclusion. This experiment uses the standard version of ROMS (no ice shelves whatsoever) where the sea-ice module is coupled to the hydrodynamic (ocean) model. The ice shelves region in this experiment can eventually be occupied by sea-ice. The atmospheric forcing is the normal year forcing (NYF – Large and Yeager, 2009, The global climatology of an interannually varying air–sea flux data set, Climate Dyn.). It consists of a repeat annual cycle of everything needed to force a coupled ocean sea-ice model. It is the forcing of the CORE experiments in GrifïňĄes et al. 2009, Coordinated ocean-ice reference experiments

(COREs) ; Ocean Modeling).

“M4: a run without ice shelves or sea ice. The foregoing remarks about surface ïňĆuxes over regions that should be ice shelf covered are equally relevant here. However, this run has even bigger problems. Applying Southern Ocean surface ïňĆuxes to a model that has no sea ice and no means to prevent the ocean super cooling is simply unphysical.”

The reviewer is correct and M4 is pretty much a proof-of-concept experiment where the intention was to show that one should not expect any reasonable result if no sea-ice model is coupled to the ocean model, which is what happens with intense overturning because of the high salinities and very low temperatures – an observation to this respect was included in the text.

Detailed comment

“Page 4039, line 9: I think it might be fairer to say that AABW is the “main water mass in the Southern Ocean responsible for the deep ocean’s ventilation”.”

“Southern Ocean” was added to the text

“Page 4041, line 5-8: While climate scale integrations of ocean models that include sea ice and ice shelves are not common, they do exist (eg. Losch, J. Geophys. Res., 113, Timmermann et al., Ann. Glaciol., 53(60), 303-314, 2012).”

We have included the references in the text:

Losch, M; 2008; Modeling ice shelf cavities in a coordinate ocean general circulation model ; J. Geophys. Res. 113

Hellmer, H., Kauker, F., Timmermann, R., Determann, J and R. Jamie; 2012; Twenty-first-century warming of a large Antarctic ice-shelf cavity by a redirected coastal current ; Nature

Timmermann,R., Wang, Q., and H. Hellmer; 2012; Ice shelf basal melting in a global



finite-element sea ice–ice shelf–ocean model; Annals of Glaciology pp. 303-314(12) .

“Page 4042, line 27, to Page 4043, line3: I think I know what you have done here, but it could be made more explicit. I presume you have modiïňĄed the formulation of Mellor and Kantha to ensure that it is conservative, hence the citation to Jenkins et al (?).”

The reference to Jenkings, 2001 was removed.

“Page 4043, lines 8-9: Here the authors explicitly state that all atmospheric ïňĆuxes are set to zero beneath the ice shelves, as they should be. But is this true for all simulations? If it is, I do not understand how you get such cold and salty water beneath the ice shelf in M2 (Figures 5 and 6) and I am confused by the statements in the penultimate paragraph of the paper (page 4053, lines 3-16).”

The reviewer is correct, M2 was a not thought out experiment and difficult (actually confusing) to interpret so it was stricken from the analysis and the text. Lines 3-16 were deleted.

We had to ensure we did not get 0 thickness and also smooth for pressure gradient errors. Remember we have a sigma model. Also more that the ice shelf has no tensile strength. It’s made up of free-floating columns of ice.

“Page 4043, lines 22-23: Can you elaborate on what you mean by “theoretical vertical proïňĄles of temperature and salinity”?”

It means that these initial conditions did not come from observations and were specified – this is reworded in the text.

“Page 4044, lines 11-12: Why did you restore surface salinities to observation? What impact did this have on the salt ïňĆuxes applied to the ocean model? Is there any restoring in the ice shelf regions? If so what data do you use?”

Surface salinities were restored to Levitus with a time scale of 180 days, there is no restoring under the ice-shelves. The surface salinity restoring acts as a correction to

the surface in the Southern Ocean, in particular E-P.

“Page 4044, lines 14-15: You do not state it explicitly, but I assume the spin-up run had all the cryospheric processes included?”

The reviewer is correct. The spin-up procedure had all the cryospheric processes included (experiment M1) this was added to the text.

“Page 4045, lines 3-6: What surface ïňĆuxes were applied to the areas formerly occupied by ice shelves?”

The fluxes are from the CORE - Large and Yeager, 2009. It consists of a repeat annual cycle of everything needed to force a coupled ocean sea-ice model.

“Page 4047, lines 23-26: This again implies that with the ice shelf thermodynamics off, there were no heat and salt ïňĆuxes applied to the ocean beneath. But if that is the case how do the sub-ice-shelf waters get so cold and salty?”

In fact, the experiment with the thermodynamics shut off (M2) was not well thought and the results are very difficult to interpret, we have removed it from the paper.

“Page 4048, line 3: I think it would be more accurate to say that the Weddell Sea contains “a climatological low atmospheric pressure centre”.”

The reviewer is correct, this was changed in the text.

“Page 4048, line 22: Shouldn’t the equation be a double integral rather than the product (?) of two integrals?”

The reviewer is correct, this was changed in the text.

“Page 4049, lines 3-6: It is a good idea to use the SODA dataset for comparisons, but I wonder why this was not done before. When you compare your results to Levitus you are comparing multi-year average model output to an observational dataset that has a strong summer bias.”



Levitus was chosen because it has been the standard observational data-set used for model set-up, initial and boundary conditions. Indeed the summer biases are strong which is the reason for introducing SODA which is reanalysis with assimilation of observations.

“Page 4050, lines 10-14: There is an error here. ISW is formed from the interaction of HSSW (and possibly LSSW) with the ice shelf, not the interaction of the two water masses.”

The reviewer is correct and this was changed in the text.

“Page 4050, lines 20-23: I do not understand your argument for the lack of WSBW on the 40W transect. Even if it does not form there (a point that I might dispute anyway), it circulates in the gyre, so should show up on all transects, shouldn’t it?”

The reviewer is correct and in fact closer examination does show WSBW, statement was stricken from the text.

“Page 4051, lines 14-16: I think the fact that the sea ice concentration differs little between the experiments is an indication of how closely linked sea ice formation is to the surface atmospheric forcing.”

The reviewer is correct and this observation was added to the text.

“Page 4053, lines 7-14: I don’t really follow this. Are you saying that there were ïňĆuxes applied to the ocean beneath the thermodynamically inactive ice shelf? Or are you Just saying what might happen if there were? This point needs to be clariïňĄed.”

The arguments in the end are indeed confusing and we decided to remove them from the text. No fluxes were applied to M2 experiment, and as mentioned before this experiment added more confusion than elucidation – we have eliminated the M2 experiment in order to make the paper more focused and objective.

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