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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 #2

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This paper presents a circum-Antarctic ocean-ice model and investigates the sensitivity of the results to the inclusion or exclusion of two key components of the cyrosphere: sea ice and ice shelves. Such studies have potential uses, either to address the academic question of what impact the cryospheric components have on the Southern Ocean circulation, or to evaluate potential biases in other models that may not have included those components for reasons of computational efficiency. However, I found this particular study somewhat lacking, both in the design and description of the model experiments and in the analysis of the results. The main conclusion seems to be (unsurprisingly) that to simulate the Southern Ocean correctly requires the inclusion of the cryospheric components. However, we are given little insight into the key processes by which the cryosphere modifies the ocean circulation (e.g. what actually causes the big differences in the large-scale circulation (Figures 7 and 8) and water masses (Figure

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Interactive Discussion

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9)) and little understanding of the biases that may be introduced into models that make use of simplified parameterisations of the cryosphere. It seems to me that to address the weaknesses of the paper requires a rethink of the experiments and a major rewrite, and I would not recommend publication until that has been completed.

The main problems are with the model experiments. These are:

M1: a control run with all components of the cryosphere switched on. This run seems fine. The resolution is a little course by present day standards, but that should not be an issue for the results that are presented.

M2: a run with the ice shelves included but their thermodynamic interaction with the ocean turned off. This seems an interesting experiment, and one that I have not seen done before in anything other than an idealised set-up. However, the results it produces puzzled me. I assumed that there were no heat and salt fluxes beneath the ice shelf (and this certainly seems to be the implication of most of the text), so could not understand how the waters beneath the ice shelves became cold and salty. Then I came to the penultimate paragraph of the paper, which implies that heat and salt fluxes are being applied as if this were open water. Actually, the authors sound unsure themselves, but if open water fluxes are being applied, the similarity between the results of M2 and M4 can be explained. 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 fluxes are applied with the ice shelf in place, are they actually being applied in M1 in addition to the ice-shelf-ocean thermodynamics?

M3: a run without the ice shelf. This could have been done in two ways: treating the ice shelf covered area as land, or treating it as open water. While the choice is arbitrary it will surely have a large impact on the results. The former (ice shelf becomes land) is what has been done in every other model I am aware of (except one), so this would be the obvious choice if the question to be addressed was what the potential biases are in models that have been run without ice shelves. However, the authors choose the latter

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(ice shelves become open water). The problem with this option is applying surface fluxes. 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.

M4: a run without ice shelves or sea ice. The foregoing remarks about surface fluxes over regions that should be ice shelf covered are equally relevant here. However, this run has even bigger problems. Applying Southern Ocean surface fluxes to a model that has no sea ice and no means to prevent the ocean supercooling is simply unphysical. Apparently temperatures as low as -15C occur and presumably the lack of stratification and resulting high surface salinities (Figures 4 and 5)are less a result of not having sea ice melt/freeze than the fact that the water column overturns everywhere because of the extreme temperatures (I wonder how the equation of state copes with these unphysical temperatures)? Since no model has ever been run like this before (I assume,) I wonder what insight the authors hoped to gain? Surely the only sensible way to run without a sea ice model is to introduce a simple parameterisation of the thermodynamics (making sure the heat flux is appropriately partitioned between a sensible heat flux that can cool the water to the freezing point, but not beyond, and a latent heat flux, which is converted into an appropriate salt flux). While you could argue that this is a basic thermodynamic sea ice model, anything less will give you unphysical temperatures. In the sea ice zone you simply cannot ignore the thermodynamics. You can beneath the ice shelf because it insulates the ocean completely from the atmospheric fluxes (at least it should!). I guess you could do the same for the sea ice, but this would be something more than an experiment without sea ice. It would be an investigation of the impact of removing buoyancy forcing almost entirely and simply having the wind acting on the Southern Ocean.

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Detailed comments:

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”.

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, C08043; Timmermann et al., Ann. Glaciol., 53(60), 303-314, 2012).

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 modified the formulation of Mellor and Kantha to ensure that it is conservative, hence the citation to Jenkins et al (?).

Page 4043, lines 8-9: Here the authors explicitly state that all atmospheric fluxes 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).

Page 4043, lines 14-18: Other authors have reported problems with the BEDMAP datasets. Did you need to do any editing to ensure that cavities of the correct shape appeared in the domain with non-zero water column thickness?

Page 4043, lines 18-20: Does smoothing of the topography smooth the ice fronts? Is this necessary anyway for the sigma coordinate? What limits were set on the acceptable steepness of the topography?

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

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

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

Page 4044, lines 27-28: This again implies that with the ice shelf thermodynamics off, there were no heat and salt fluxes applied to the ocean beneath. Is that true? Figures and the comments at the end of the paper imply otherwise.

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

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

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

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

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.

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.

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?

Page 4051, lines 14-16: I think the fact that the sea ice concentration differs little

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between the experiments is an indication of how closely linked sea ice formation is to the surface atmospheric forcing.

Page 4053, lines 7-14: I don’t really follow this. Are you saying that there were fluxes 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 clarified.

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