Review of: *A modeling study of the hydrographic structure of the Ross Sea*, by M. Tonelli et al.

Summary

This paper presents results from numerical simulations of the Southern Ocean (ocean + dynamic/thermodynamic sea ice + thermodynamic ice shelves), focusing on the model's ability to represent water mass characteristics in the Ross Sea, towards the goal of making sure that water mass modification processes over the Antarctic continental shelf is correctly represented in the Global Thermohaline Circulation (somewhat strangely denoted "THC").

The goal is sensible, and I appreciate the use of multi-parameter analyses to identify the primary water masses and their relative distributions in the Ross Sea based on their model. However, for a variety of reasons, explained below, I don't feel that the results are presently sufficiently well explained to publish.

General comments

1. It is never really explained what the authors mean by "The Ross Sea". Given its quoted size (section 1.1), it appears to be the continental shelf only, north of the Ross Ice Shelf. I assume that this is the region that the T-S scatter plots are prepared for (Fig. 3). This would have been much clearer if the first figure was what is now Fig. 2 (the model domain map), with a large inset or extra panel showing just the Ross Sea, defining the area for which Fig. 3 was created, and labeling all the features that come up in the text (e.g., Cape Adare and Cape Colbeck, and the RIS).

2. The principal conclusion from this study is that the model does a reasonable job of reproducing the distribution of water masses as determined by comparison with Orsi and Wiederwohl (2009; OW09). So, the paper should show this. At a minimum, compare T-S scatter plots in two panels on the same scale, the model run and either Levitus (your initial condition) or OW09 (preferably both). I accept that there will be biases, and this is a great chance to show these and speculate as to why they exist.

3. Along the same lines, the methodology for partitioning water mass contributions (as shown in Fig. 4) can be applied to real-world T-S, either Levitus climatology (i.e., your model's initial state) or OW09. Do the model biases relative to OW09 arise because OW09 is different from Levitus, or because your model shows that Levitus is not consistent with the CORE normal-year forcing and/or the model physics? At this stage of the analysis, explaining why the model drifts away from Levitus initial conditions is probably more useful than comparing with an extra validation data set (OW09) which has its own data limitations. It would really be interesting to see how different Fig. 4 panels were, between Levitus and the spun-up model.

4. There are now several circum-Antarctic models and many global ocean models, and a few Ross-Sea-only models. This new model is apparently doing a good job of

representing the coupled system, but you do not show that you are doing a better job than others, or why. I agree that it is valuable to validate models through volumetric analyses of water masses in a critical region (i.e., Fig. 3, modified to include panels for OW09 and Levitus), and most modeling papers do not use this method for validation. (At least, they don't include it in their publications.) But nothing in the paper really helps us know whether a specific feature of your model is important to getting the Southern Ocean state right. If you are doing better than other models (not yet shown), is it because of the model resolution or structure, the quality of sea-ice or ice-shelf physics, the use of terrain-following coordinates, or ???

5. I really don't understand the choice of the 165^{o}W transect (Fig. 4) to illustrate model performance. The water masses of the Ross Sea are spatially variable; almost all the really dense shelf water is in the western half, and the only significant outflows of AABW are from the Glomar Challenger and Drygalski troughs in the west. The most valuable result that you show in Fig. 4, once all the problems I had with the analysis (see next point) are sorted out, is the reduction in CDW fraction in the abyssal ocean (>3000 m depth) correlating with the slight increase in SW fraction. (You need a better color scale to make this clear.) However, this presumably represents the contribution of SW via AABW production in the *western* Ross Sea, circulating clockwise around the Ross Gyre to show to on this eastern transect. That is, I think there must be a better way to show what the model is doing with water masses than showing just the 165^{o}W transect; e.g., more transects, and/or maps of depth-integrated water mass volume

6. Given that there is no comparison of water mass distributions between your model results (Fig. 4) and either real-world data or other models, I have no way of knowing whether the Fig. 4 panels are reasonable. However, I cannot even accurately interpret them. There are several issues here:

a) Table 1 suggests that there are 4 independent seawater types involved in the fitting procedure. But (i) SW and ISW are not independent in your definitions (ISW is a subset of SW), (ii) ISW is not actually a high-salinity water mass, since it includes meltwater from the ice shelf, and (iii) you have to use exact definitions of sea water types to solve the equation set (1); you can't have open-ended inequalities.

b) Equation set (1) is really misleading. First, you must explain the extra terms R_{θ}, R_S, R_O, R_{mass} and, in fact, all terms in this equation set. Why are there "0" terms in the θ, S and mass equations? 'PV' is, presumably, potential vorticity, which you propose to use in future studies, but you imply that 'O' (dissolved oxygen?) is the way to look for conservation of PV?? Second, you need to explain, early, that you can only specify three water masses, then tell us exactly what these are. You cannot separate ISW from SW as defined in Table 1, so what do you use for the cold, salty end-member?

c) In Fig. 4, I get the sense that the sum of all water masses is not 100%. For a while, I thought this was because of the 'R' terms in equation set (1). However, maybe it is because you don't really solve Eq. (1) in the Tomczak and Large style, but instead just sort the measured T-S into water mass types? I think, when you explain clearly that you can only get three water masses, then you would change Fig. 4 to only show 3 panels that do add up to 100%. You might then show a new figure showing the distribution of ISW separately, not to imply that it comes from Eq. (1). Maybe the distribution of true ISW is a sensitive test of model performance since it certainly relies on, at a minimum, getting the ice shelf basal melting right, so it would be worth comparing modeled ISW (defined in a more physically sensible way) with OW09 (which probably does a better job of identifying ISW than Levitus does).

However, to repeat, Fig. 4 is only valuable if it is compared with something else. Are these reasonable distributions? (Compare with the same analysis of Levitus and/or OW09.) What do they really tell you? (Is ISW confined below 500 m on the continental shelf a reasonable solution? Is the volume of ISW reasonable given modeled basal melt rates for RIS? Part of the problem with ISW is the strange definition of it (S=34.78, same as SW). Is it closer to truth than, say, IPCC-class models that are readily accessible to look at?