Response to the second revision of “The Whole Antarctic Ocean Model (WAOM v1.0): Development and Evaluation” by Richter et al.

We thank the editor and reviewer for their remarks. Our response is in blue text. Our line numbers refer to the marked up manuscript.

Response to Review

In this revision the authors have substantially improved the manuscript, particularly via an overhaul of the evaluation of the simulated hydrography on the continental shelf. Unfortunately, this improved evaluation reveals serious biases in the continental shelf water masses. Most notably, there is an almost-complete absence of High Salinity Shelf Waters at sites such as the Ross and Weddell continental shelves, and a lack of Circumpolar Deep Water (CDW) circulating in the Amundsen and Bellingshausen Seas. Although the authors argue that the model is ready for process-oriented studies, my perspective is that the water mass biases make its applications rather limited: the model cannot be used to study dense shelf water formation, and is not well suited to studying CDW-driven melt of Antarctica’s most rapidly-melting ice shelves. These biases likely stem from the authors’ prescription of the ocean surface fluxes, rather than the more conventional approach simulating the evolution of the sea ice under a prescribed atmosphere.

That said, the authors are open about the model’s biases and readiness for addressing science questions, and they identify various experiments and analyses for which the model could be used in its present state. Thus, while I think the community would be better served by the authors publishing a further improved version of the model in which the major hydrographic biases had been somewhat ameliorated, I do not see this as a barrier to publication of the model in its current state.

We would like to highlight that the reviewer agrees that the development step that WAOM v1.0 represents is worthy of publication.

Below I have included another round of comments and questions for the authors. Of these comments, the most significant pertain to the equilibration of the model at 4km and 2km resolutions. As many of these comments may require substantial changes to be made, particularly to the figures, my recommendation is that the manuscript be returned to the authors for further major revisions.

We now have included a statistical measure that shows that the model is near to quasi equilibrium at all resolutions. Further, we argue that the remaining model drift is acceptable for the purpose of this study. All comments have been addressed in detail below.

Comments/questions:

L29-32: This statement is correct, but don't all models fall into one of these categories? I am struggling to understand what exactly the authors are aiming to convey with this sentence.

No, there are also pan-Antarctic ocean models without any kind of ice shelf interaction (explicit or parameterised). We have included a clarifying sentence (here shown in bold, L32-34):

"Many ocean models with pan-Antarctic coverage have either been designed with cavities from the beginning (Beckmann et al., 1999; Timmermann et al., 2002; Hellmer, 2004) or augmented by an ice shelf component at a later stage (e.g. Timmermann et al., 2012; Kusahara and Hasumi, 2013; Dinniman et al., 2015; Schodlok et al., 2016; Mathiot et al., 2017; Naughten et al., 2018b). ***There also exist stand alone ocean models without explicit or even parameterised ice shelf interaction (e.g. Mazloff, Heimbach, and Wunsch 2010) and most earth system models used for state-of-the-art climate projections do not include an ice shelf component (e.g. Griffies et al. 2016; Dinniman et al. 2016)."***

L48: The authors state that model parameters are "often" calibrated, but then cite only a single study in support of this statement. Does this really constitute "often"?

We agree with the reviewer. We are not aware of another study that performed a calibration as extensive as done by Nakayama et al. (2017). We have reworded the sentence without changing its general statement (L51).

"Model parameters in regional studies ~~are often~~ ***can be*** *calibrated (e.g. Nakayama et al., 2017), but to approach similar efforts with large scale models, suitable Antarctic-wide observations need to be compiled first."*

L50: Again "often", this time with just two examples. If these were review articles then this would be reasonable, but these are just regular scientific studies.

We agree. We have rephrased the sentence without modifying its general statement (L54).

"For this purpose, previous studies have ~~often~~ utilised ice shelf melt rates derived from satellite observations and models of firn processes (e.g. Schodlok et al., 2016), and selected Southern Ocean quantities from observations and reanalysis products (e.g. Naughten et al., 2018b)."

L97-111: Have the authors tested the model with uniform stratification and no forcing to establish that their topographic smoothing and density Jacobian formulation of the pressure gradient force suppress spurious along-slope currents?

Yes, we have performed a zero-forcing experiment for the 10 km resolution version of the model and included a sentence about the magnitude of the spurious currents as compared with the forced simulation (L106-115):

"*We apply the Mellor-Ezer-Oey algorithm (Mellor et al., 1994), which is well established for bathymetry smoothing (Sikiric et al., 2009). We smooth the bathymetry and water column thickness directly until a maximum slope factor $r = (h_i - h_{i+1})/(h_i + h_{i+1}) \leq 0.3$ is satisfied for both. The ice draft is then redefined as the superposition of bed and water column thickness. ~~While this approach has been developed in regional studies~~* ***This is a well established procedure to minimise spurious currents in regional ice shelf-ocean configurations*** *(Galton-Fenzi et al., 2012; Cougnon et al., 2013; Gwyther et al., 2014).~~, the impact of these manipulations on the ice front representation and related processes (e.g. Mode 3 melting) is unknown.~~* ***An experiment at 10 km resolution with uniform stratification and no forcing produced negligible currents in most regions. The only spurious currents of note on or near the continental shelf are along part of the Amundsen-Bellingshausen shelf break, which may explain some of the discrepancy in hydrographic conditions in this region (described in Sec. 3.4).***"

We refer back to this point when discussion overly mixed conditions on the shelf (L413-417):

"[...] CDW enters the shelf, but gets mixed away too readily before reaching the ice (Fig. 9c). Indeed, WAOM is overly mixed in many regions (incl. the Bellingshausen Seas; see Fig. 12). ***Spurious currents from pressure gradient force errors may explain part of the discrepancy in the Amundsen-Bellingshausen Seas, but not in other regions (see Sec. 2.3).*** *[...]"*

and future work (L479-481):

"*To improve WAOM v1.0 (focused on accurate sub-ice shelf melting) future development should focus on reducing mixing [...] Finally, the sensitivity of stratification to different slope factors (Haney factors) should be explored. Spurious mixing at steep sloping topography (related to pressure gradient force errors in sigma coordinate models; discussed earlier) is sensitive to the degree of smoothing. Our smoothing procedure is similar to regional studies and the smoothing algorithm has been shown to perform well for a realistic, complex case without ice (The Adriatic Sea, see Sikiri´c et al., 2009).* ***However, spurious currents in our model are significant along the shelf break of the Amundsen-Bellingshausen Seas, possibly reducing the stratification on the adjacent continental shelf.*** *~~However, o~~ Other pan-Antarctic studies have chosen different routines and algorithms and do not report overly mixed conditions (Naughten et al., 2018b). [...]*"

L130: I suggest an alternative naming to "heat flux into the ocean". Based on the units and context, I infer that this quantity is some kind of restoring coefficient.

Yes, in ROMS it is actually referred to as surface net heat flux sensitivity to SST.

We have changed the text accordingly (L137 f.):

"Further, to avoid model drift, the surface ocean is relaxed to the solution from SOSE (Mazloff et al., 2010), using a ~~heat flux into the ocean~~ ***surface net heat flux sensitivity to SST*** *of 40 W m−2 ◦C−1 and … "*

Fig. 2: The 10km run looks, to my eye, to have reached equilibrium, but it would be appropriate to include some statistical measure of this. The 4km run looks like it may still be drifting away from the 10km run - have the authors tried checking whether there is any statistically significant trend in the difference between the 10km and 4km melt rates at the end of the integration period?

The drift in the monthly mean melting for both the 10km and 4km runs is less than 3% across the last year of integration. Further, Dinniman et al. (2015) uses a comparable model to perform sensitivity studies, which is generally more delicate than model development and evaluation. They are satisfied with a remaining interannual melt variability of 1%. Therefore, we rate our remaining model drift as acceptable for the purpose of this study, that is development and evaluation.
However, we acknowledge that 3% drift is not perfectly equilibrated, but rather *close to* steady state. We have changed our wording accordingly and included the above outlined statistical measure in the text.

L149 f.: "*Forcing with single year conditions captures daily to seasonal variability, while allowing us to run the model* ***close*** *to quasi-equilibrium with our given supercomputing resources."*

L155-159: *"The 10 km version of the model is integrated for 5 years, before the on shelf ocean ~~reaches~~* ***is near to*** *a quasi equilibrium and its solution is used to initialise the 4 km run. Analogously, the 4 km run is stepped forward in time for 2 years before the final 2 km simulation is initiated and integrated for another year and three months.* ***Interannual monthly mean melting at each resolution drifts by less than 3% at the end of the integration period, which we rate as acceptable for the purpose of this study.****"*

L530 f.: *~~"The model has equilibrated at 10 km grid spacing, and has been continued from that equilibrated state at higher resolutions (up to 2 km).~~ We have brought the model close to equilibrium at 2 km grid spacing."*

Additionally, I would strongly encourage the authors to consider whether other measures of shelf water masses, e.g. mean bottom temperatures or salinities, indicate that the simulations have reached equilibrium.

We thank the reviewer for this suggestion. Ice shelf basal melting, however, is a consolidated representation of continental shelf ocean conditions and the central quantity of this study. Like previous studies with comparable models (see, e.g., Dinniman et al. 2015; Kusahara and Hasumi 2013; Naughten et al. 2018), we are convinced that the evolution of ice shelf melting is sufficient to present the spin up of models of ocean ice shelf interaction.

L187: Why only the summer mean? The winter conditions are arguably more important for some processes, e.g. formation of dense shelf waters.

This is true, but we trust the observational products only for summertime where they would have primarily been sampled. This has been elaborated in the preceding paragraph (L184 f.):

"WOA18 is most accurate in summer, when sea ice has its minimum extent and the vast majority of observations are taken."

No changes have been made to the text.

Fig. 3: As the amplitude of tidal fluctuations varies widely around Antarctica, this figure may be more insightful if the error were normalized by the root-mean-square tidal height fluctuations at each tide gauge.

The reviewer makes a good point here. The key is to produce a figure that best demonstrates the performance of the model - with respect to what the model is designed to do. Modelled ice shelf melting scales approximately linearly with velocity, and thus relative errors of tidal height amplitude are most insightful.

We have replaced the original Figure with the following:

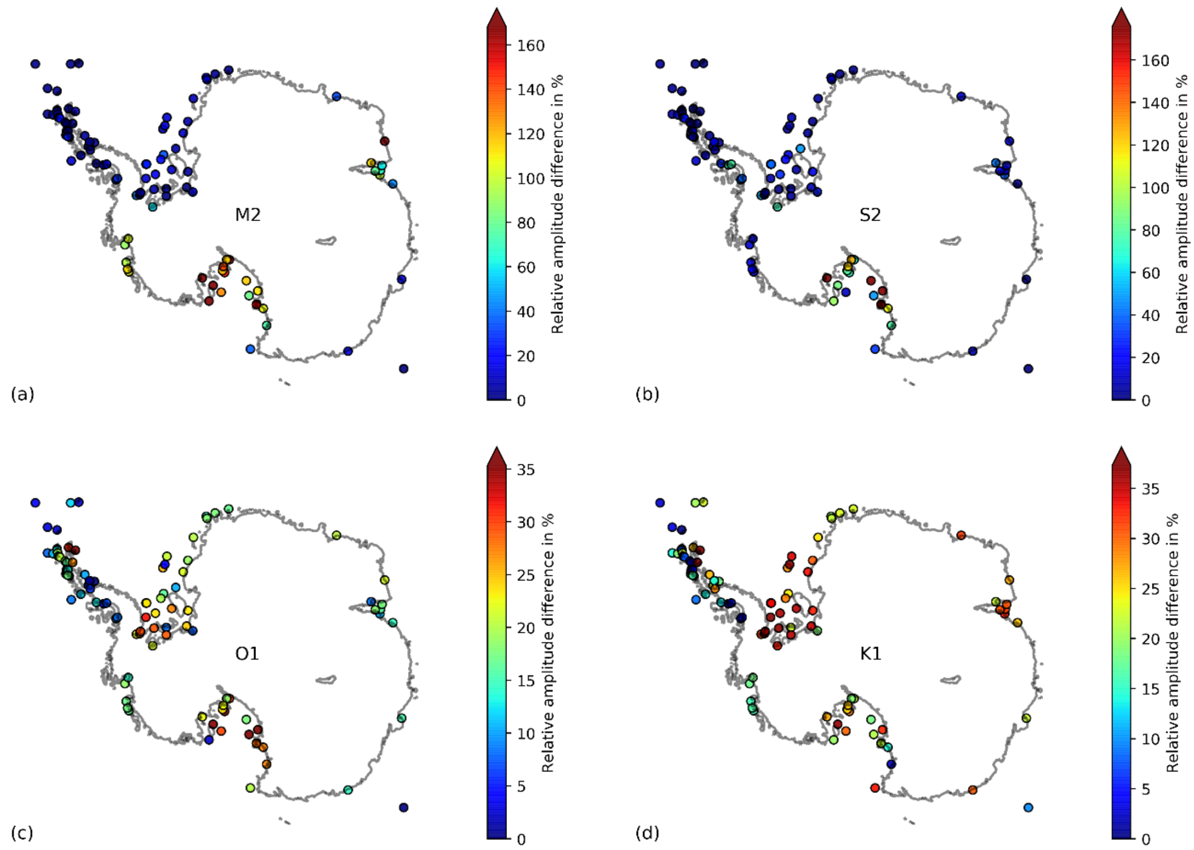


Figure 3: Spatial distributions of tidal height accuracy. Relative amplitude differences between the model solution and Antarctic Tide Gauge records ([H_WAOM − H_ATG] / H_ATG) are shown for the major tidal constituents (a) M2, (b) S2, (c) K1 and (d) O1. The colorbar has been truncated at the 95% quantile.

The respective interpretation has been included at the end of the section (L237-240):

*"**Figure 3 shows the relative differences in tidal height amplitude. WAOM systematically overestimates tidal strength of the semi-diurnal constituents in the Ross Sea with differences often exceeding 80 %. In contrast, diurnal tides are generally underestimated and deviations are more balanced around the coast. For the diurnal bands most stations feature differences below 35 %.**"*

As this bias is not reflected in the melt rates, these findings are not picked up in the discussion.

Finally, we have disregarded the original Figure 3 (absolute errors of complex amplitude differences) to the supplemental material as it only confirms biases, which are well known from barotropic tide models. We have modified the respective paragraph accordingly (L228-232):

"Table 2 summarizes the outcomes of the tidal height accuracy analysis. *The model has a combined RMS error of 20 cm, which is within the accuracy of 2D Antarctic tide models (assessed by King and Padman, 2005).* ~~*Table 2 summarizes the outcomes of the tidal height accuracy analysis, while Figure 3 shows complex amplitude differences for each of the four constituents at each tide gauge station. Most of the*~~ ***Similar to these models, most of our*** *bias comes from* ~~*the semidiurnal constituents M2 and S2 and*~~ *sites at the grounding line deep under the large ice shelves* ***(see Appendix Fig. D4)****."*

Fig. 4: This figure shows that the 4km and 1km runs are much more similar than the 10km run. Is this simply because the 10km run has been integrated independently of the others for much longer (the 4km run is branched off the 10km run at year 5, then the 2km run is branched off the 4km run in year 7, and analysis is performed in year 8), or due to changes in the resolution of shelf processes?

The impact of independent integration time does not matter when each of the model solutions has equilibrated. That is, if we would disregard our cascading spinup procedure and instead initiate the 2km model from the ECCO2 state and integrate it for 9 years, we would expect to derive the same state. Further, the remaining model drift in mean melting at 2 km resolution has an opposing sign to the one at 4 km resolution, showing that independent integration time does not govern our results.

No changes have been made to the text.

It would also be helpful to include a second x-axis, along the top of the figure, showing the actual model grid spacings.

We agree with the reviewer and have included this detail. Here is the updated Figure 4:

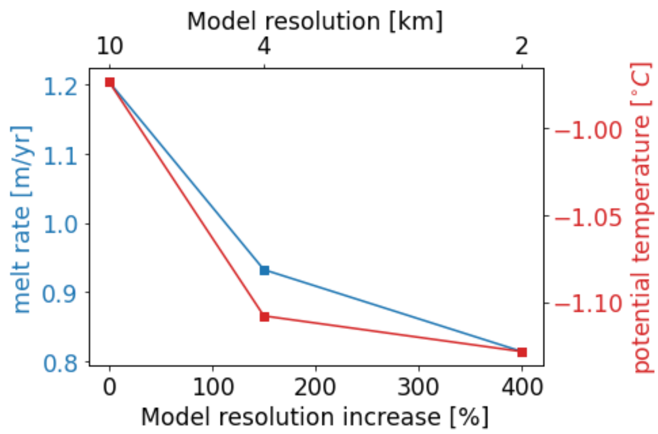


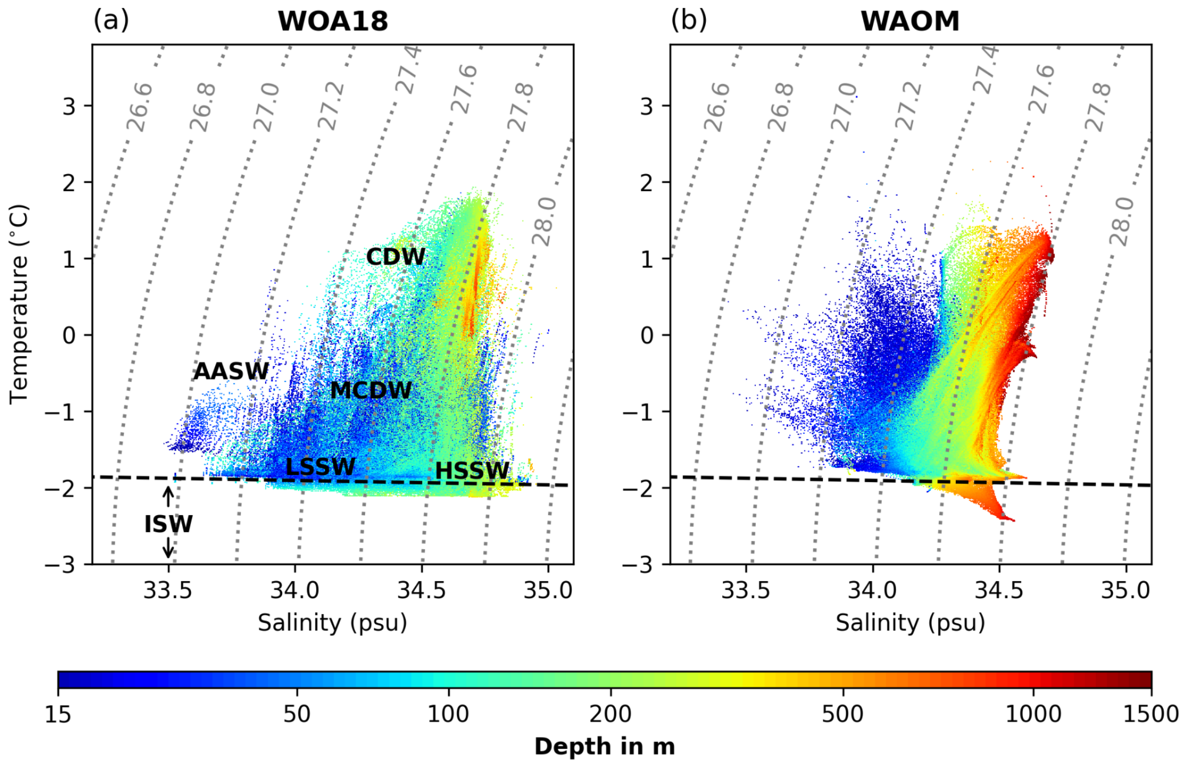
Fig. 8: These plots are difficult to compare because the data are presented as a straightforward scatter, causing many points to overlap one another and thus obscuring many details. I would recommend presenting these T/S diagrams as two-dimensional probability density functions instead. Information about the depths of water masses could be conveyed in separate panels via volumetric weighting of the depths contributing to each water mass bin.

We believe the reviewer's understanding about our visualization method is incomplete, despite an comprehensive and well placed description. Our TS diagrams are not scatter plots. We plot a grid of volume weighted averages with transparent grid cells where bins are empty. It might appear as a scatter with overlap, but, actually, no points overlap and no detail is lost. This kind of visualization has been used before and using it here allows for direct comparison (e.g. against MetROMS and FESOM by Naughten et al., 2018).

No changes have been made to the Figure. The method to produce these plots is outlined in detail in the caption of the figure and, hence, no changes have been made to the text.

Also, the "ISW" label doesn't seem to be indicating any water masses in the left-hand panel.

We now have included a double headed arrow to indicate that the ISW label refers to all water masses below the freezing point (dashed line). Despite more ISW in subfigure b, the ISW label is presented in subfigure a to have all labels in one place.



Also, “AntarcticSurface” -> “Antarctic Surface” in the caption.
We have corrected this mistake in Figure 8 and Figure D2.

L304-305: Aren’t the waters below the surface freezing temperatures ice shelf waters?
The reviewer is correct. We have modified the text from (L317-320):

“Ice Shelf Water (ISW) outside the cavities is only apparent in WAOM.“ to:

“Ice Shelf Water (ISW) in WOA18 remains within 0.25 degC below freezing and is apparent over a wide range of salinities (33.75 to 34.75). In contrast, WAOM features ISW with temperatures of more than 0.5 degC below freezing, but a narrower range of salinities of 34.25 to 34.6.”

This detail has not been picked up later and no further changes to the manuscript are necessary.

L307: Formatting of citation.
We have corrected this mistake (L322 f.).

L311: Format “degC”
We have corrected this (L326).

L325: “temperature salinity transects”
We have corrected this to (L340): “*temperature-salinity transects”*

L327: “hydrology” -> “hydrography”?

The reviewer is right, we did mean hydrography. The term hydrology also includes water on the land surface, while hydrography only refers to ocean water. We have corrected this mistake (L342).

Fig. 9: The color bars for the middle panels are mis-labeled, I think.
The reviewer is right. These are absolute values, no differences. We have corrected this mistake in the label.

Figs. 11-13: The biases in WAOM look to be quite severe: comparable to the variations in T and S in WOA. I suggest adding panels showing the T and S sections in WAOM to better visualize this.
We agree with the reviewer and have included the respective plots. To follow the reviewers' intent in emphasizing the bias, we show WAOM results on the same color scale as WOA transects, rather than desaturating the new plots. We also have modified the captions accordingly. Here are the new Figures and captions:

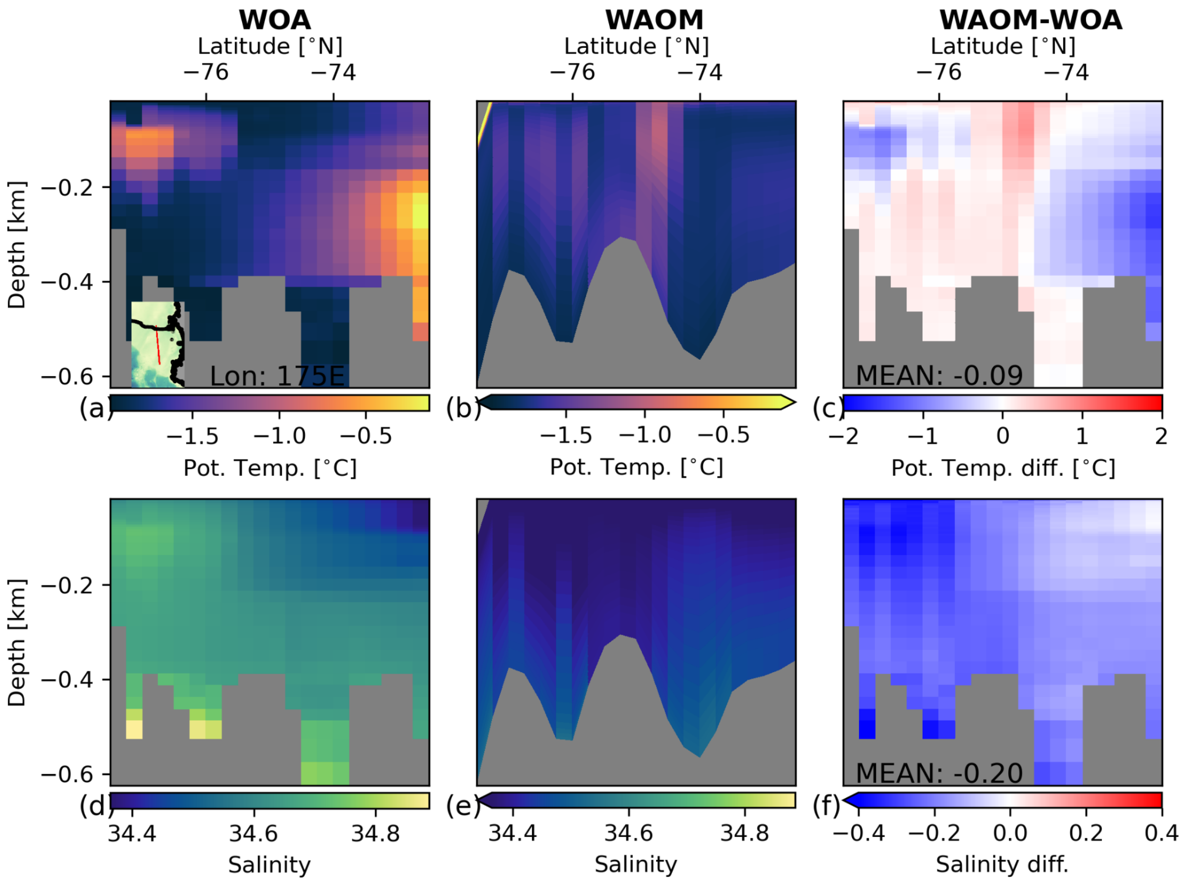


Figure 11. Temperature and Salinity transect on the Ross Sea continental shelf (175 ◦E) compared against observations. (a) and (d) are WOA18 2005-2017 summer mean temperature and salinity, ***(b) and (e) are 2007 summer mean temperature and salinity from our model (WAOM)****, and (b) and (d) are the perspective differences ~~to WAOM's 2007 summer mean~~ (WAOM - WOA18). ~~Prior to the comparison,~~ WAOM's data has been interpolated to the WOA18 grid using nearest neighbours* ***(for b and e in the horizontal; for c and f in the horizontal and vertical).***

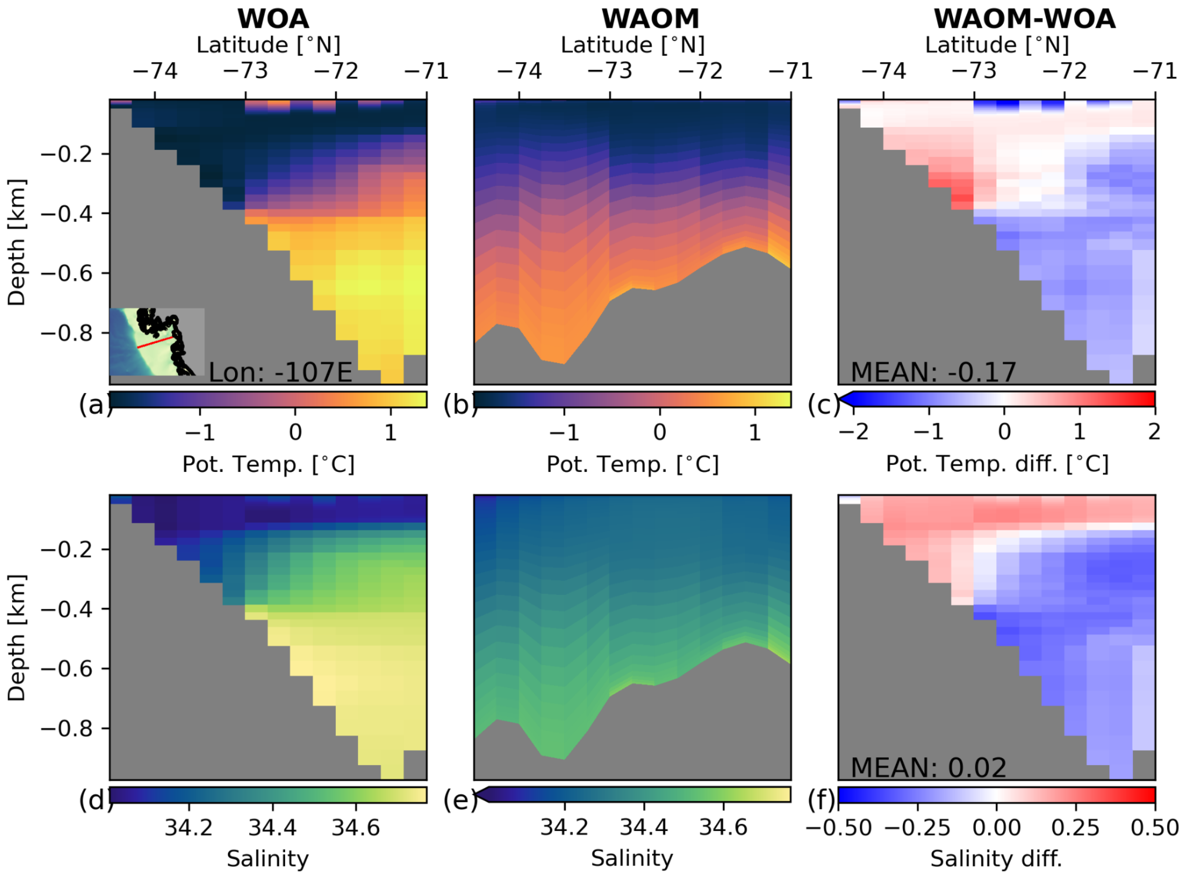


Figure 12. As Fig. 11, but for a transect across the Amundsen Seas along 107 ◦W.

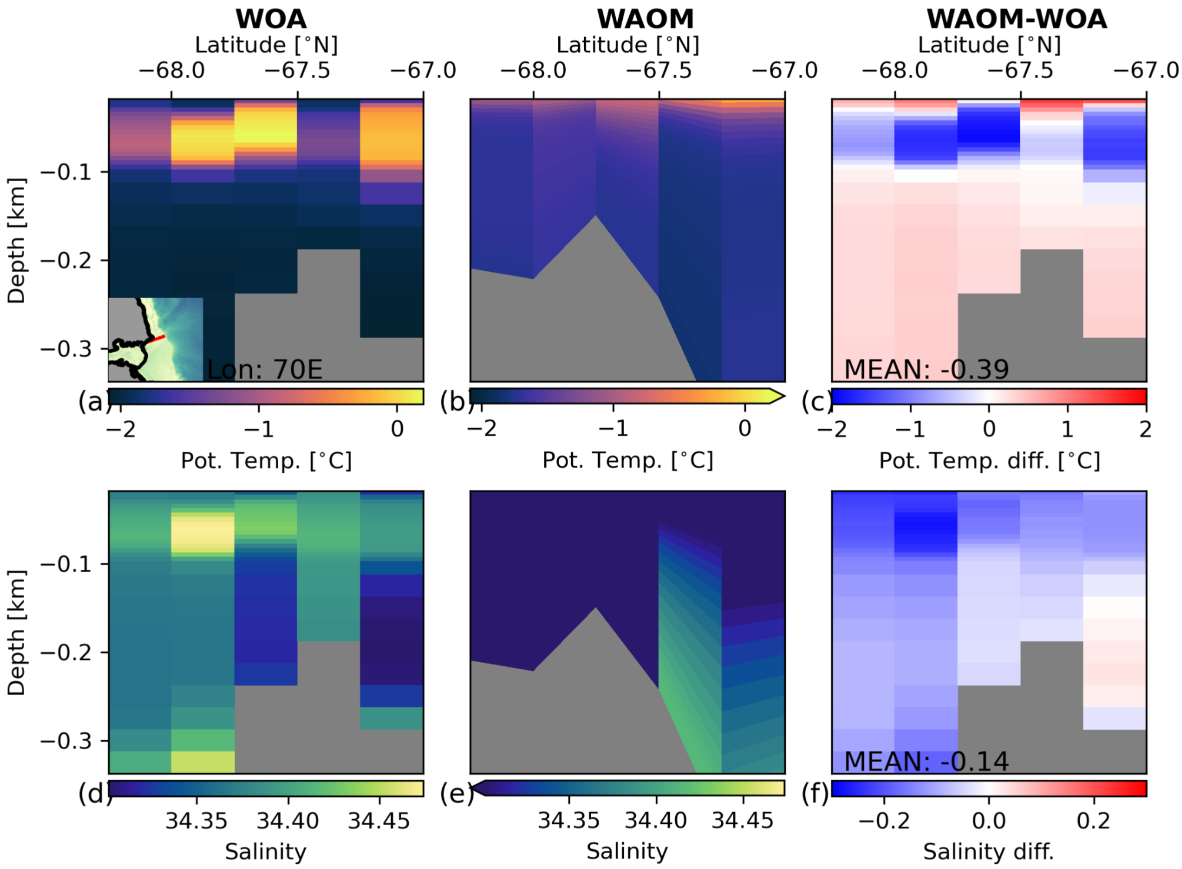


Figure 13. As Fig. 11, but for a transect in Prydz Bay (Davis Sea continental shelf) along 70 ◦E.

No more changes to the text were necessary, since the additional figures better visualize already discussed biases.

Fig. 15: Same comment as for Fig. 8.
We kindly refer to the same answer as given for the comment for Figure 8 above.

L367: “accurate” -> “accurately”
We have corrected this mistake (L383).

L384: citation formatting
We have corrected this mistake (L400).

L473-475: Has this issue been discussed earlier in the main text. I see it noted in Fig. D4, but not elsewhere.

The point the reviewer is referring to was discussed under L491-493, that we repeat here:

“In third place, the boundary effects in the Eastern Ross Sea should be addressed. Introducing a sponge layer is difficult, since tides are also forced at the open boundary. Instead we recommend an adjustment to the model boundary locations to avoid intersection with ACC jets at shallow angles.”

This issue has been introduced earlier in the discussion (L433-437):

"We also have reported CDW intrusions onto the continental shelf of the Eastern Ross Sea (see Fig. 9) and this is likely related to boundary effects. Where ACC jets cross the domain's boundary in shallow angles, artificial currents can arise. We have reduced these effects by making the boundary conditions outflow dominant (see Methods), but some artificial currents remain in the Ross Sea (see Fig. D3). We hypothesise that these currents drive CDW onto the shelf by affecting the slope of the isopycnals close to the shelf break."

No changes have been made to the text.

References

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