Overall statement

We thank all three reviewers for their thorough and constructive comments. Overall, we are pleased to see that the reviewers agree with the importance of the model and that the development step it represents is worthy of publication in GMD.

Concerns are mostly related to our evaluation strategy for the ocean. We have substantially strengthened the paper in this respect, closely following the reviewers suggestions. The focus has been shifted to the on-shelf regions and model predictions are now compared against observational estimates from WOA18 and CTD bottom layer measurements (compiled by Schmidtko et al., 2014).

Reviewer 2 also questions if the model is “fit for purpose” at its current stage. We now communicate more clearly that WAOM v1.0 is just the beginning. The model has already been picked up by several research groups focusing on ice sheet-ocean coupling (R. Gladstone 2020, pers. comm., 29 June), dense water formation (U. Petteri 2020, pers. comm., 7 January) and future predictions of ice shelf melting (J. Moore 2019, pers. comm., 11 June). These groups further calibrate and evaluate the model according to their specific research questions. At this stage, as presented in this manuscript, the model is suited to study individual processes involved in ice shelf-ocean interaction, predominantly tides. We now communicate the distinction between the wider scope future of WAOM and the applications of its first version more clearly.

We sincerely hope that our responses are satisfactory for the reviewers. The diverse, ongoing research with WAOM also highlights the need to publish this tool now.

In the following we have addressed all comments. Reviewer comments are back, our response is blue. Changes to the text are printed in *italic*. Modified or added figures are provided at the end of the document.

For cross referencing we have labelled each comment. R1C2, for example, refers to Reviewer 1, Comment 2. Longer responses can include subsections (e.g. R1C2, Discussion: Biases). A marked-up version of the manuscript is provided along with this document. Our references to figures and lines refer to this manuscript. References from the reviewers refer to the first version of the manuscript.

Changes unrelated to the reviews:

We now have managed to activate parallel I/O for the 2 km version of the model, significantly enhancing the performance of the model. We are also running the model now on the Australian Government National Computing Infrastructure's latest supercomputer gadi.

We have updated Table 1 from:

Model Resolution	10 km	4 km	2 km
Period simulated	1 year	1 year	1 year
CPU hours	280 h	6,840 h	40,030 h
Architecture	Sandy Bridge	Sandy Bridge	Broadwell
Number of CPUs	256	2304	224
Memory	51 GB	2.9 TB	876 GB
Walltime	1 h	3 h	142 h
Storage for 1 3D field	40 MB	250 MB	1 GB

Table 1. Computational requirements at different resolutions. WAOM has been run on the supercomputer Raijin from the National Computing Infrastructure (NCI) in Australia. Sandy Bridge architecture stands for 2x8 core Intel Xeon E5-2670 2.6GHz with 32 GB RAM per node and Broadwell is 2x14 core Intel Xeon E5-2690v4 2.6GHz with 128 GB RAM per node. We needed to ensure a high RAM per CPU for the 2 km application as input-output was handled in serial.

to:

Model Resolution	10 km	4 km	2 km
Period simulated	1 year	1 year	1 year
CPU hours	93 h	1,877 h	16,983 h
Number of CPUs	288	2304	5184
Memory	45 GB	390 GB	1.53 TB
Walltime	0.5 h	1.2 h	4.4 h
Storage for 1 3D field	40 MB	250 MB	1 GB

Table 1. Computational requirements at different resolutions. WAOM has been run on the supercomputer Gadi from the National Computing Infrastructure (NCI) in Australia. The architecture consists of 2x24-core Intel Xeon Platinum 8274 (Cascade Lake) 3.2 GHz CPUs per node with 192 GB RAM per node. Listed times are for time-stepping only, that is without initialization or Input/Output.

And changed the respective paragraph in the model description (lines 126-132):

"Table 1 summarises the computational costs associated with running the model on the Australian National Computing Infrastructure (NCI) supercomputer ~~Raijin~~ Gadi. On the resulting grids with 10 km, 4 km and 2 km resolution the 3-D equations integrate stably with timesteps of, respectively, 900 s, 360 s and 180 s. This leads, for example, to a cost of ~~6,800~~ 1,877 CPU hours for 1 year of simulated period at 4 km resolution. ~~We note that upscaling of the computational architecture for the highest resolution was obscured by the fact that the parallel input-output was not functional. Serial input-output puts the computational burden onto a single CPU, requiring us to choose a suboptimal architecture with few CPUs and large RAM per CPU. This issue should be addressed in future studies.~~ We note that initialization and Input/Output require additional resources."

Respective development advice is also redundant now (lines 640-642):

"

- ~~*Finally, including parallel input-output in WAOM would allow for efficient parallelisation at 2 km resolution. The gain in computational cost might make longer simulation periods feasible or allow for a further increase in horizontal resolution until continental shelf quantities converge.*~~*"*

Ocean Evaluation

Comments from all three reviewers challenge our general evaluation strategy for the ocean conditions. We now include a quantitative comparison of the ocean hydrography against observational estimates, closely following the reviewers suggestion. Related changes to the manuscript are extensive, and best presented together and upfront. In the following, we outline these modifications structured by section. Later, within the individual reviewer comments, we refer back to these changes.

Methods
We have changed the title of the respective section from *Analysis* to *Model Evaluation* and have extended this section by several paragraphs. Upfront we now outline the scope and strategy of the evaluation presented in this study (p. 7,lines 179-186):

"2.6 Model evaluation

In this study we present a tool for the community that can ultimately be used to address many different questions related to ocean-ice shelf interaction. Future studies intending to apply WAOM will need to tune and evaluate the model to their specific needs. In this manuscript we focus on ice shelf basal melting and, hence, have focused our evaluation strategy on this quantity. Also, melt rates contain the integrated history of the upstream ocean and their evaluation implies insights into the hydrography of sub-ice shelf cavities and the adjacent continental shelf. In addition, we directly compare ocean hydrography against observations to provide a first estimate of the biases. This helps to better explain the predicted melt rates and provides a starting point for future studies with different focus."

Next, as a consequence of a detailed description of the ocean evaluation, we have also elaborated on our evaluation strategy for ice shelf melting (lines 187-190):

"We compare annual mean ice shelf mass loss averaged over individual regions and for total Antarctica against satellite observations from Rignot et al. (2013), Depoorter et al. (2013) and Liu et al. (2015). Uncertainties for satellite derived ice shelf-ocean interaction at high resolution are unknown (discussed earlier). In this regard, we showcase models results and compare predictions against theory, regional studies and satellite estimates in the text. [To calculate basal mass loss …]"

For the ocean evaluation, we have removed the paragraph describing SOSE (lines 195-196):

“~~We use SOSE to evaluate the off-shelf ocean. As mentioned earlier, SOSE assimilates many observations from elephant seals, ships and Argo floats in the Southern Ocean, making it very reliable where such observations exist (Mazloff et al., 2010). On the shelf, however, observations are sparse and the ocean dynamics used to integrate SOSE do not include ice shelf interaction. Hence, we expect SOSE to have large biases close to the ice and we only use its solution for the off-shelf ocean to evaluate WAOM.~~”

Instead we now present our choice of observational products and some crucial information about their underlying sampling density. We also use this paragraph to explicitly define the boundaries of the model solution (lines 196-207):

“For the ocean evaluation, we have chosen to use WOA18 climatologies and estimates of on-shelf bottom layer hydrography from Schmidtko et al. (2014). WOA18 is most accurate in summer, when sea ice has its minimum extent and the vast majority of observations are taken. The deep ocean is expected to show little seasonality though. Observations on the shelf are sparse and often concentrated along repeated ship tracks (see Fig. D1). The ocean state on the shelf is critical in determining the circulation and melt rates in the ice shelf cavities. Here, bottom layer hydrography is of particular interest, as it provides information about CDW intrusions and dense water formation. Schmidtko et al. (2014) provides a comprehensive compilation of on-shelf bottom layer hydrography from CTD measurements. The northern extents of the off-shelf ocean should be seen as a sponge layer, likely affected by ECCO2 boundary and initial conditions and not fully spun-up using our procedere. The flux-forced approach at the surface decouples sea ice conditions from the underlying ocean. This is known to create artificial water masses in the uppermost layers of the model. Hence, the top 15 m (equivalent to the uppermost 2 sigma layers in most regions) should be seen as a boundary and are excluded from this analysis.”

Finally, we describe the technical details behind the comparisons regarding the ocean (lines 208-223):

“On the shelf (south of the 1500 m isobath), we compare the summer mean (December, January and February) of the WOA18 climatology from 2005 to 2017 against the summer mean of 2007 as predicted by WAOM. We use Temperature-Salinity (TS) diagrams to assess the water masses and longitudinal transects for the stratification. For the TS-diagrams, both products have been sampled on their original grid. The transects are taken where CTD data underlies the WOA18 product (along ship repeat tracks and on the Amundsen Sea continental shelf, see Figure D1) and WAOM’s estimates have been interpolated to the WOA18 grid (1/4° and up to 102 depth levels) using a nearest neighbour scheme. Further, we compare multidecadal means of bottom layer hydrography from Schmidtko et al. (2014) against the 2007 mean from WAOM. The CTD locations have been interpolated onto the model grid using nearest neighbour interpolation. Then, model data has been interpolated to the depth of the observations using the nearest neighbour scheme. We augment these comparisons by showcasing high resolution transects and regional TS-diagrams that include the cavities from WAOM and compare these results against regional studies in the text.
We define the off-shelf ocean as south of 65°N and north of the 1500 m isobath. Here we compare the summer mean of the 2005-2017 climatology from WOA18 against the 2007 summer prediction of WAOM. We also include the prediction of the 2007 summer mean from

ECCO2, which provides the initial and boundary conditions for WAOM. Differences in bottom layer hydrography between WOA18, ECCO2 and WAOM are assessed using annual means, as we expect little seasonality at such great depths. All observational estimates have been converted to model quantities (potential temperature and practical salinity).”

Results: On-shelf hydrography

We have restructured the evaluation results to better reflect their importance for this study: 1. Ice shelf melting, 2. on-shelf hydrography and 3. off-shelf hydrography. Results related to ocean hydrography are now presented in one section, resulting in the following sections: *3.3. Ice shelf melting* and *3.4 Ocean hydrography*.

For the on-shelf regions we have added 6 figures comparing the model solution against observational hydrography in TS-space, as bottom layer maps and along selected transects (Figs. 8-13). We have added the following paragraphs to describe these figures (lines 410-441):

"3.4 Ocean hydrography

Figure 8 compares the Temperature-Salinity-Depth distribution of WAOM's on-shelf water masses in summer with the summer climatologies of WOA18 (see Methods, Section 2.6). Most of the subsurface waters (Circumpolar Deep Water, CDW, Modified Circumpolar Deep Water, MCDW, and Low Salinity Shelf Water, LSSW) are well represented in the model. However, High Salinity Shelf Water (HSSW), characterised by temperatures close to freezing and salinities higher than 34.5 g/kg, is almost entirely missing. In general, HSSW is the densest water mass on the shelf and mixes with other, lighter waters. As a consequence of its absence, all water masses in WAOM are well restricted by the same isopycnal of 1027.8 kg m-3 (also within the cavities, see Fig. D2). We define the near surface ocean as the 15-100 m depth range (the uppermost 2 model layers are excluded due to limitations in the flux-forcing approach, see Methods). *At these depths, WOA18 is mostly colder than 0 degC. While WAOM predicts similar upper ocean temperatures in some regions, we also identify waters of up to 1 degC at 15 m depth. Finally, WOA18's water masses feature salinities as fresh as 33.5 g/kg (upper ocean and LSSW), but WAOM only reaches 33.75 g/kg. Together with the lack of the densest waters, this hints towards overly mixed conditions in WAOM. Ice Shelf Water (ISW) outside the cavities is only apparent in WAOM.*

Figure 9 compares maps of the annual mean bottom layer hydrography of the on-shelf ocean from WAOM against observational estimates by Schmidtko et al. (2014) (multidecadal mean from CDW measurements; see Methods, Sect. 2.6). Figure 10 presents sector-wise averages of this comparison. WAOM qualitatively captures the distinction between cold and warm regimes, as the bottom waters of the Amundsen-Bellingshausen Seas are distinctly warmer than in the other sectors (Fig. 10a). However, three main modes of biases are also apparent. First, in the Amundsen-Bellingshausen Seas, predicted bottom waters are too fresh and cold. In particular, the deep waters in the Bellingshausen Seas are on average about 0.75 degC colder in the model compared to the observations (Fig. 10a). The spatial characteristics show that the temperature bias in these regions are often small at the shelf break and increase towards the coast (Fig. 9c). This supports the idea that CDW crosses the shelf break in sufficient amounts, but then is getting mixed with the upper ocean too readily before reaching the ice. Second, a warm and fresh bias is apparent in the Ronne Depression and some parts of East Antarctica, related to the previously identified lack of HSSW formation in the model. Third, in the eastern Ross Sea, a warm bias is combined with accurate salinities. The temperature bias is strongest at the shelf break and diminishes *towards the ice, hinting towards intrusions of CDW across the shelf break. Accurate salinities would then be explained by salty CDW offsetting the fresh bias from missing HSSW.*

Figures 11 to 13 compare longitudinal transects of temperature and salinity in summer between WAOM and WOA18 in key regions (see Methods). Mean temperature differences are small (less than 0.4 degC), further supporting that WAOM captures the difference between warm and cold regimes correctly. In all transects, however, WAOM is less stratified than WOA18, as salinity differences oppose the observed salinity trends of the region (salinity controls stratification in the Southern Ocean). In agreement with TS-distribution of the entire shelf (Fig. 8), the deep waters in the troughs of the Ross Sea are too fresh compared to WOA18 estimates."

This direct quantitative comparison is augmented by the original results, showcasing high resolution transects of predicted hydrology in key regions and comparing these against regional studies in the text. From the original transects, we have excluded the uppermost 15 m (due to reasons outlined earlier; Fig 11) and condensed the original description to one paragraph. The text has changed from (lines 342-372):

"~~*Observations on the continental shelf are sparse and cover only short periods of time, which often do not coincide with our simulation. Thus, we do not expect model results to closely match available measurements. Rather, in the following section, we showcase that the model qualitatively captures many of the known, critical features of the onshelf hydrography around Antarctica.*~~

~~*WAOM resolves the important water masses in the Weddell Sea, including Warm Deep Water (WDW) and large amounts of ISW (as shown in Fig. 8; see Nicholls et al., 2009, their Fig. 3). Figure 9a shows a temperature-salinity transect in front of the Filchner Ice Shelf at 35 ◦W. This transect reveals that ISW resides at the bottom of the Filchner trough while warmer waters at mid depth resemble characteristics of Modified Weddell Deep Water or Eastern Shelf Water (also shown in Nicholls et al., 2009, their Fig. 7).*~~

~~*In contrast, deep waters in the Amundsen Sea sector feature some of the highest temperatures of the entire Antarctic continental shelf (see Fig. 8). Figure 9b shows the temperature and salinity distributions along 106 260 ◦W, indicating that these CDW intrusions are overlaid by colder Winter Water and only held stable by a large gradient in salinity (in agreement with, e.g. Jacobs et al., 2011).*~~

~~*Figure 9c shows a temperature-salinity transect on the continental shelf of Prydz Bay along 72 ◦E. Inside the Amery Ice Shelf cavity HSSW and ISW can be seen at the bottom and top of the water column, respectively. Further, we detect Dense Shelf Water with salinities of more than 34.5 psu at depth greater than 500 m (Fig. 8, described by, e.g. Williams et al., 2016). CDW is held back from entering the continental shelf in this region by the Antarctic Slope Front (in agreement with, e.g. Guo et al., 2019, their Fig. 2).*~~

~~*Along the Sabrina and George V coasts, however, some MCDW crosses the continental shelf break, e.g. in front of the Totten Ice Shelf. This is demonstrated by the temperature-salinity distribution along 120 ◦E in Figure 9d. Once on the shelf MCDW 270 competes with the lighter WW which occupies most parts of the shelf ocean close to the coast (in agreement with, e.g. Silvano et al., 2017, their Fig. 2 and 3). AASW with temperatures well above freezing can be seen in all transects (Fig. 9a to 9d). We identify advection of these surface waters into the outer cavities of the Amery ice shelf (see Fig. 9c)*~~

~~*and the Totten ice shelf (see Fig. 9d; in agreement with Silvano et al., 2017, their Fig. 2)*~~*"* to (lines 442-457):

"Figure 14 showcases predicted annual mean temperature salinity transects in key regions on the continental shelf and on the original model grid (2 km resolution). These transects show that WAOM qualitatively captures many of the known regional characteristics of the Antarctic continental hydrology. Examples are given in the following. In the Weddell Sea, ISW resides at the bottom of the Filchner trough, while warmer waters at mid depth resemble characteristics of Modified Weddell Deep Water or Eastern Shelf Water (Fig. 14a; in agreement with, e.g. Nicholls et al., 2009, their Fig. 7). In contrast, deep waters in the Amundsen Sea sector feature some of the highest temperatures of the entire Antarctic continental shelf (Fig. 14b). These CDW intrusions are overlaid by colder Winter Water and only held stable by a large gradient in salinity (in agreement with, e.g. Jacobs et al., 2011). Further, inside the Amery Ice Shelf cavity, we detect dense, cold waters at the bottom of the water column (hinting towards HSSW properties, even though they are not salty enough) and ISW at the top of the water column (Fig. 14c; in agreement with, e.g. Galton-Fenzi et. al. 2012, their Fig. 14). In this region, CDW is held back from entering the continental shelf by a sharp front (the Antarctic Slope Front; exaggerated by the choice of color scale; in agreement with, e.g. Guo et al., 2019, their Fig. 2). Along the Sabrina and George V coasts, some MCDW crosses the continental shelf break, e.g. in front of the Totten Ice Shelf (Fig. 14d). Once on the shelf MCDW competes with the lighter WW which occupies most parts of the shelf ocean close to the coast (in agreement with, e.g. Silvano et al., 2017, their Fig. 2 and 3). Finally, we identify advection of warm surface waters into the outer cavities of the Amery ice shelf (Fig. 14c; in agreement with Galton-Fenzi et. al., 2012, their Fig. 9) and the Totten ice shelf (Fig. 14d; in agreement with Silvano et al., 2017, their Fig. 2).".

Results: Off-shelf hydrography

For the off-shelf hydrography, we now compare model predictions against WOA18 climatologies in TS-space and as bottom layer maps (Figs. 15, 16). We have also included ECCO2 in these comparisons, as it provides the boundary and initial conditions for WAOM and is expected to influence the model solution in this region.

Results involving SOSE have been removed (Fig. 6 and 7; p. 11, lines 290-320):

~~"To assess the broad-scale hydrography simulated in WAOM, we compare results against SOSE (Mazloff et al., 2010), a high quality ocean reanalysis product (See Section 2.6). Figure 6 presents the temperature-salinity distribution for WAOM and SOSE. At depth WAOM agrees well with SOSE in many aspects. In both models we identify the presence of Circumpolar Deep Water (CDW), Modified Circumpolar Deep Water (MCDW), Low-Salinity Shelf Water (LSSW), Antarctic Bottom Water (AABW), Weddell Sea Bottom Water (WSBW), and Ross Sea Bottom Water (RSBW). We note that High Salinity Shelf Water (HSSW) is poorly represented in both models (e.g. see Nicholls et al., 2009, their Fig. 3, for observed HSSW properties in the Weddell Sea), which is likely related to the representation of sea ice and resulting surface fluxes. Below 2000 m depth, WAOM's RSBW and WSBW are up to 0.5 psu saltier than suggested by SOSE. This discrepancy might in part originate from WAOM's boundary conditions, as ECCO2's bottom water features salinities of up to 34.7 psu (slightly~~

~~more than SOSE; not shown). Stronger water mass transformation in WAOM compared to SOSE, however, might also play a role, as WAOM's WSBW is in part saltier than 34.8 psu and this can not be explained with boundary conditions alone. An unambiguous attribution would require further investigations beyond the scope of this study. At shallow depths, however, the models disagree. Antarctic Surface Water (AASW) tends to be several degrees warmer in WAOM compared to SOSE, where the surface is often close to freezing. As the deep and often salty ocean mixes with these surface waters, different tails are shaped in T-S space. While WAOM's surface water is often warmer than the ocean at depth, temperatures and salinities of SOSE's upper ocean mostly resemble freezing conditions. Which of the models is more accurate close to the surface and what is causing the differences is not clear. Figure 8 shows the temperature-salinity distribution for WAOM on the continental shelf and separated by sector. These distributions show that the warm surface waters in WAOM are mostly restricted to the off-shelf ocean and likely driven only by regional phenomena in the Bellingshausen Seas. In contrast to SOSE, WAOM is capable of resolving Ice Shelf Water (ISW). ISW is produced by ice-ocean interaction inside the ice shelf cavities and often forms characteristic linear signatures in T-S space (Gade lines, see Gade, 1979). The z-like signature of ISW in the Ross Sea is likely caused by continued mixing of ISW from one ice shelf inside the cavity of another ice shelf downstream and this further supports the presence of ice shelf teleconnections."~~

And replaced by a more conclusive comparison against observations (lines 458-470):

"*Figure 15 compares the TS distribution of the summer mean climatology from WOA18 against the 2007 summer mean from ECCO2 and WAOM (see Methods, Sect. 2.6). In WAOM, water mass properties at depth mostly resemble the observations, but the warm bias in the upper ocean is even more apparent than on the shelf. Between 15-100 m depth WOA18 is mostly limited to temperatures of less than 0 °C, but WAOM predicts more than 3 °C in some regions. The warm surface bias also affects the properties of adjacent water masses at intermediate depths, effectively warping the overall picture of the TS-distribution away from the freezing point. While ECCO2 also shows shallow waters with temperatures above observed, these are limited to below 2 degC and show little mixing with deeper waters. The densest waters in WAOM show only little isopycnal mixing with colder surface waters (also see Fig. D2). In agreement with the earlier identified lack of HSSW formation, this hints towards bottom waters in WAOM, which are mainly sourced by initial and boundary conditions from ECCO2.*

Figure 16 compares the annual mean bottom layer hydrography from WOA18 against ECCO2 and WAOM. WAOM shows an overall warm bias by about 0.3 °C, which can now clearly be attributed to the initial and boundary conditions from ECCO2. Bottom layer salinities in both models agree well with WOA18. All biases revealed in this section are discussed later in respect of their sources and consequences for ice shelf-ocean interaction (see Sect. Discussion)."

The focus of the ocean evaluation lays now on the on-shelf regions, leading us to remove results regarding off-shelf stratification from the manuscript (previous Fig. 7 and related text shown below; lines 321-338). We note that reviewers have commented on the representation of vertical mixing based on the off-shelf stratification. The same issue is apparent for the on-shelf and is still being discussed (later).

~~*"The stratification of WAOM agrees well with SOSE for the off-shelf ocean and, as expected, diverges towards the ice shelves. Figures 7a to 7d show longitudinal transects of temperature and salinity of both models. In the open ocean away from the continental shelf break, the solutions agree and this supports realistic boundary constraints and mixing processes in WAOM. Towards the shelf break and on the continental shelf WAOM resolves substantially colder and fresher waters compared to SOSE, which we interpret as the result of melt water from the ice shelf cavities. WAOM also often shows stronger vertical mixing close towards the continental shelf, possibly caused by surface forcing, tides or pressure gradient errors. The ocean close to the continental shelf is often well mixed in WAOM, but remains relatively stratified in SOSE (as, e.g., can be seen in Prydz Bay transect, Fig. 7d) and this could have various reasons. First, brine rejection in sea ice polynyas is known to cause deep mixing of the entire water column (e.g. Silvano et al., 2018). While WAOM and SOSE use the same mixed layer parameterisation (KPP), different surface forcing and melt water in WAOM might change the sensitivity to deep convection. Second, SOSE does not include tides. Tidal currents are known to contribute to ocean mixing and tidal strength amplifies towards shallower waters (e.g. Padman et al., 2009). Finally, spurious currents from pressure gradient errors at steep sloping topography in sigma-coordinate ocean models might also contribute to more mixing in WAOM (Mellor et al., 1994, 1998). This argument is supported by the fact that WAOM produces enhanced mixing also in the vicinity of deep ocean ridges, e.g., in the Ross Sea (Fig. 7b)."*~~

Finally, three figures presenting auxiliary information related to the ocean evaluation have been added to the supplemental material (Fig. D1 to D3).

Discussion: Biases

The new findings support that WAOM captures large-scale characteristics of the on-shelf hydrography important for ice-shelf melting. We also have been able to identify three main biases in the ocean: a spuriously warm surface, a lack of HSSW formation and overly mixed conditions on the continental shelf. In the light of ice shelf-ocean interaction, missing HSSW has been rated as most important (in addition to a cold bias in the Amundsen-Bellingshausen Seas, which had already been revealed from the evaluation of ice shelf melting).

In the discussion, we have adapted the paragraph about model biases to communicate these points clearly and hypothesise connections between the biases.

We have changed the original paragraph from (lines 513-554):

~~*"WAOM underestimates melting for some ice shelves and we speculate boundary conditions to be the cause. A cold bias in the Amundsen-Bellingshausen Seas is a common issue in large scale models (e.g. Naughten et al., 2018b) and has been attributed to, either, insufficient transport of deep ocean heat onto the continental shelf (as mentioned earlier), insufficient transport of onshelf heat into the sub-ice shelf cavities or underestimated conversion efficiency of heat into melting inside the cavity (Nakayama et al., 2014; Dinniman et al., 2015). In our simulation, onshelf ocean temperatures in the AmundsenBellingshausen Seas are comparable to observations and where deep warm water intrusions reach the ice*~~

~~*shelf cavities melt rates also agree (e.g. for George V and Abbot). Therefore, we expect insufficient transport of onshelf heat into the cavities to be the cause of underestimated melt rates in the model (e.g. for the ice shelves Pine Island, Getz and Totten, compared to Rignot et al., 2013; Depoorter et al., 2013; Liu et al., 2015). There are a multitude of mechanisms that could prevent onshelf heat from entering the cavity and that could vary between regions. For example, at Pine Island Ice Shelf, Davis et al. (2018) shows that local wind forcing modulates thermocline depth, which in turn controls the access of CDW into the cavity on weekly to monthly timescales. We do not account for the effect of sea ice on surface wind stress in the model and, thus, a bias in thermocline depth might cause low melting in this region. In contrast, for Totten and Moscow University Ice Shelves we attribute underestimated heat flux into the cavity mostly to a bathymetry bias. A regional model by Gwyther et al. (2014) resolves similar continental shelf temperatures, but uses a cavity thickness which is 5 times larger along the centreline compared to Bedmap2, and this model resolves melt rates comparable to satellite estimates."*~~ to:

"WAOM underestimates melting for some warm water ice shelves and produces too little HSSW, both likely related to overly mixed conditions on the continental shelf. A cold bias in the Amundsen-Bellingshausen Seas is a common issue in large-scale models (e.g. Naughten et al., 2018) and has been attributed to, either, insufficient transport of CDW onto the continental shelf (e.g. Thoma et al., 2008; Nakayama et al., 2014; discussed earlier), too rapid erosion of heat on the shelf (e.g. Bett et al. 2020) or underestimated conversion efficiency of heat into melting inside the cavity (e.g. Dinniman et al., 2015). The ocean evaluation indicates that the second cause applies in our case. 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). We note that winds have also been shown to affect shoreward heat transport (Kimura et al., 2017; Greene et al., 2017) and we do not account for the effect of sea ice on wind stress. However, the sensitivity of ice shelf melting to momentum flux modulations have yet to be explored (as done in Jendersie, 2018).

Too much mixing might also be responsible for the reported lack of HSSW formation (Fig. 8). Integrated surface salt input in polynya areas compares well against the original forcing product by Tamura et al. (2011) (not shown), hence our surface salt flux tuning (see Methods) is not the cause for the bias. Instead, waters with salinities higher than 34.5 g/kg are indeed present in the uppermost 15 m, but readily mix within this layer before reaching greater depths (Appendix Figure D2). The reported warm bias at the surface (Fig. 8) could also be linked to reduced HSSW formation. WAOM predicts elevated melt rates right at the ice front in most regions (close to coastal polynyas; Fig. 7) and ISW has been shown to be able to suppress dense water formation (Williams et al., 2016; Silvano et al., 2018). However, we rate this possibility as unlikely, since the warm surface bias is less apparent in winter (not shown), when deep convection events are happening.
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."

Discussion: Future development

The reviewers comments and related investigations allow us to provide more guidance for future development. For this, we emphasize the limited scope of the evaluation of this study and list observational datasets suitable for extended comparisons. We now lead with the following point (lines 577-582):

"

- *Future studies will need to calibrate and evaluate the model according to their research question. Morrison et al. (2020), for example, uses a pan-Antarctic ocean model to study water mass transport across the shelf break and, hence, evaluates the model using hydrographic profiles in the slope region. Suitable observational datasets for studies focused on the Antarctic seas (in addition to the ones applied in this study) include the Marine Mammals Exploring the Oceans Pole to Pole (MEoP) dataset (Roquet et al. 2014), a review of dense shelf water observations around Antarctica (Amblas and Dowdeswell 2018) and a monthly isopycnal/mixed-layer climatology (MIMOC, Schmidtko, Johnson, and Lyman, 2013). Available in-situ observations of ice shelf melting have yet to be compiled (as discussed in the next point)."*

We also have condensed our point regarding a universal evaluation matrix, now focusing exclusively on ice shelf melting at high resolution (rather than ice shelf-ocean interaction; lines 583-596):

"

- ~~*Establishing an evaluation matrix for circum-Antarctic ice shelf-ocean models would open the path for efficient parameter tuning (similar to Nakayama et al., 2017) and allow the community to compare the performance between different models (see Naughten et al., 2018b). Many kinds of observations are useful for this, including ice shelf basal melting from phase-sensitive radar (ApRES), as well as ocean measurements from Conductivity(Salinity)-Temperature-Depth (CTD) sensors, Acoustic Doppler Current Profilers (ADCP) and turbulence measurement packages. These ocean instruments can be mounted on Autonomous Underwater Vehicles (AUVs) with under ice capability, underwater gliders, drifting floats, moorings and Seals. When rating the model performance against such observations, uncertainties of the underlying methods and the spatial and temporal variability of the observed quantities must be carefully considered. ApRES seems particularly suitable for large scale model evaluation as it comprises a robust and cheap method to observe basal melt rates over longer time periods. As more ApRES measurements are becoming available, their compilation could provide the backbone for such an evaluation matrix, similar to tide gauge measurements for tidal accuracy (King and Padman, 2005). Comparison of a wide array of ApRES data is already underway with the NECKLACE programme.*~~

- *Establishing an evaluation matrix for Antarctic ice shelf melting at high resolution would open the path for efficient parameter tuning (similar to Nakayama et al., 2017) and allow the community to compare the performance of different models (see Naughten et al., 2018) and satellite derived estimates. ApRES seems particularly* *suitable for large scale model evaluation (e.g. Gwyther et al., 2020) as it comprises a robust and relatively cheap method to observe basal melt rates over longer time periods. As more ApRES measurements are becoming available, their compilation could provide the backbone for such an evaluation matrix, similar to tide gauge measurements for tidal accuracy (King and Padman, 2005). Comparison of a wide array of ApRES data is already underway with the NECKLACE programme[1].”*

Further, based on the findings of this study, we have added three points to describe future development steps aiming to improve WAOM's accuracy regarding sub-ice shelf cavity conditions (lines 598-622).

“

- *To improve WAOM v1.0 (focused on accurate sub-ice shelf melting) future development should focus on reducing mixing to better represent the stratification on the continental shelf. We have scaled horizontal tracer diffusion linearly with resolution, but have not tuned this parameter against observations. Likewise, stratification is sensitive to the chosen mixing (here LMD, which includes KPP) and advection schemes (here 4th-order Akima for the horizontal and vertical), and the effects of different choices have yet to be tested for WAOM. 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 Sikirić, Janeković, and Kuzmić 2009). However, other pan-Antarctic studies have chosen different routines and algorithms and do not report overly mixed conditions mixing (Naughten et al., 2018). The Haney factor controls the degree of smoothing within any given scheme and, hence, offers a metric to assess the sensitivity without implementation of new procedures.*
- *Second priority should be given to the calibration of the surface heat flux, which is likely to reduce the warm surface bias. The warm bias towards the surface can not be explained by initial and boundary conditions, as ECCO2's upper ocean conditions are more realistic (see Fig. 15). Also, 2007 has not been an anomalously warm year (e.g. measured by sea ice extent; see Parkinson, 2019), rendering interannual variability as an unlikely source. Instead, we suspect the applied surface flux schemes to be responsible. A similar scheme is known to overestimate annual heat flux into the ocean by about 50% (Jendersie et al., 2018). While we aim to account for this by reducing positive heat flux into the ocean by half (see Methods, Sect. 2.4), the approach has not been tested for pan-Antarctic domains.*
- *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.*

”

In the light of the revealed biases, advice for future field campaigns has been removed (lines 598 and 617-621)

“

[1] NECKLACE programme: http://www.soos.aq/news/current-news/330-necklace-workshop-update.

- ~~*Future field campaigns should be guided by model results. To explain why WAOM underestimates the heat flux into the cavities of some of the warm water ice shelves (Pine Island, Getz, combined Brunt and Riiser-Larsen, Shackleton, combined Totten and Moscow University), more ocean measurements, including bathymetry, should be taken near the front of these ice shelves. Also, ApRES measurements are particularly valuable where high resolution satellite estimates have their greatest uncertainties, that is in calving regions and close to grounding lines. Although, crevasses are often present in these regions and can impede the successful interpretation of ApRES results.”*~~

Summary and conclusion

We have adapted the concluding paragraph about model performance and future development, being explicit about the revealed biases (lines 667-695) from:

“~~*Model results compare well against available observations. Continental shelf ocean temperatures and ice shelf melting converge with increasing model resolution, but a further refinement to 1 km grid spacing is likely needed to reach asymptotic behaviour. The accuracy of tidal height signals at the coast is comparable to state-of-the-art barotropic tide models and the off-shelf hydrography agrees well with SOSE, which assimilates most of the available observations in the Southern Ocean. On the continental shelf, where observations are sparse, WAOM resolves realistic hydrography, e.g., featuring bottom layer temperatures of 1 ◦C in the Amundsen-Bellingshausen Seas and WDW in the Weddell Sea. Ice shelf melting and marine ice accretion at high resolution show that WAOM captures the known modes of melting, often in agreement with regional studies. Ice shelf average melt rates agree with satellite observations at many places, but indicate a cold bias for some of the warm water ice shelves in the Amundsen-Bellingshausen Seas as well as the Totten and Moscow University Ice Shelf System. We attribute these discrepancies to insufficient heat flux from the continental shelf into the sub-ice shelf cavities, likely due to regional uncertainties in bathymetry or wind stress.*~~
~~*To further improve WAOM, future studiesshould mostly focus on compiling available observations of ice-ocean interaction aroundAntarctica. Efforts are underway to collect all available ApRES measurements of ice shelf basal melting around Antarctica (the NECKLACE programme)and this could form the base for a consistent evaluation matrix of large-scale ice shelf-ocean models. Such a framework would not just help to tune model parameters in an efficient manner, but also compare the performancebetween different models and , thus, focus community model development. Further, the bathymetry in WAOM should beupdated where regional products are available and future studies should target individual, uncertain aspects in the model, such as how sea ice modulates wind stress and the representation of surface water advection under the ice front.*~~” to:

“WAOM qualitatively captures the broad scale difference between warm and cold regimes and many of the known characteristics of regional ice-ocean interaction. Continental shelf ocean temperatures and ice shelf melting converge with increasing model resolution, but a further refinement to 1 km grid spacing or finer is likely needed to reach asymptotic behaviour. The accuracy of tidal height signals at the coast is comparable to state-of-the-art barotropic tide models. The total ice shelf basal mass loss is close to, but 4% below the

lowest estimate derived from satellite observations. The basal mass balance of individual ice shelves agrees with satellite observations in many places, but indicates a cold bias for some warm water ice shelves in the Amundsen-Bellingshausen Seas as well as the Totten and Moscow-University Ice Shelf System. Ice shelf melting and marine ice accretion at high resolution are often in agreement with regional studies, demonstrating that our model captures the known modes of ice shelf-ocean interaction. The on-shelf hydrography resembles many aspects of WOA18 summer climatologies and decadal mean bottom layer temperatures by Schmidtko et al. (2014), but exhibits a lack of HSSW formation, a warm bias at the surface and excessive mixing. We hypothesize that the cold bias in the Amundsen-Bellingshausen Seas and the lack of HSSW is caused by overly mixed conditions on the continental shelf.
Future studies will need to evaluate and calibrate the model according to their specific research question. To improve the model's accuracy regarding ice shelf melting, the biases revealed here should be addressed first. Any further tuning will first require a compilation of available in-situ observations (from ApRES measurements). Such efforts are underway with the NECKLACE programme."

Introduction

Where we introduce model evaluation, we have adapted the information about ocean observations (lines 62-67) from:

"~~Further, ocean reanalysis products, such as the Southern Ocean State Estimate (SOSE; Mazloff et al., 2010), assimilate most of the available data from elephant seals, ships and Argo Floats, but observations on the Antarctic continental shelf are sparse and the underlying ocean models do not account for ice shelf melting and, hence, the resulting freshwater release.~~" to:

"*Further, compilations of ocean observations, such as the World Ocean Atlas 2018 (WOA18), include most of the available data from ships, Argo Floats, gliders and elephant-seals, but the interpolated temperature and salinity fields are only available as climatologies with up to decadal resolution and observations in sea-ice covered regions are sparse, implying large uncertainties on the Antarctic continental shelf.*"

In the light of the biases, now revealed for the on-shelf ocean, we have relaxed the tone of the paper, emphasizing that WAOM v1.0 should be seen as the first step (lines 68-76) from:

"*~~Here we describe the development and evaluation of a new circum-Antarctic ocean-ice shelf model that aims to overcome some of the shortcomings of previous studies. The Whole Antarctic Ocean Model (WAOM v1.0) includes tides and an eddy resolving horizontal resolution of 2 km and, thus, includes all the model physics of state-of-the-art regional applications. Establishing an evaluation matrix and rigorous model tuning is out of the scope of this study, but we aim to convince the reader that WAOM is capable of simulating an equilibrated and realistic version of present day conditions by comparing model results against a selection of established estimates of Southern Ocean quantities and ice shelf melting for the chosen period of 2007.~~"* to:

“*Here we describe the development and evaluation of a new circum-Antarctic ocean-ice shelf model that aims to overcome some of the shortcomings of previous studies. The Whole Antarctic Ocean Model (WAOM v1.0) includes tides and an eddy-resolving horizontal resolution of 2 km, both known to be critical to resolve accurate ice shelf-ocean interaction. We compare model results against a selection of established estimates of Southern Ocean quantities and ice shelf melting for the chosen period of 2007. This way, we aim to convince the reader that this first version of WAOM is realistic enough to be applied to specific, process oriented studies and to justify further development of our approach.*”

Finally, we have updated the description of the manuscript structure (lines 77-83) from:

~~“The following section (Sect. 2) describes the model and experiments performed in this study. In Section 3, we evaluate tidal accuracy, investigate resolution effects and compare model results against selected off-shelf hydrography from SOSE, as well as estimates of ice shelf-ocean interaction from regional studies and large scale satellite observations. This is followed by a discussion of WAOM’s key strengths and limitations, as well as future development and research questions suitable for exploration with our model (Sect. 4). The last section (Sect. 5) summarises and concludes this study.”~~ to:

“The following section (Sect. 2) describes the model, the experiments performed in this study and our evaluation strategy. In Section 3, we present tidal accuracy, investigate resolution effects and compare model results against estimates of ice shelf-ocean interaction from satellite observations and regional studies, as well as selected hydrography from WAO18 and Schmidtko et al. (2014). This is followed by a discussion of WAOM's key strengths, biases and limitations, as well as future development and research questions suitable for exploration with the model at its current state (Sect. 4). The last section (Sect. 5) summarises and concludes this study.”

Abstract

We also have adapted the abstract to incorporate the new findings and explicitly state the development stage and applicability of WAOM v1.0 (p. 1, lines 6-18):

“~~At the northern boundaries ocean conditions are derived from the ECCO2 reanalysis and tides are incorporated as sea surface height and barotropic currents. The accuracy of tidal height signals close to the coast is comparable to those simulated from widely-used barotropic tide models, while off-shelf hydrography agrees well with the Southern Ocean State Estimate (SOSE) model. On the shelf, most details of ice shelf-ocean interaction are consistent with results from regional modelling and observational studies, although a paucity of observational data (particularly taken during 2007) prohibits a full verification. We conclude that our improved model is well suited to derive a new estimate of present day Antarctic ice shelf melting at high resolution and is able to quantify its sensitivity to tides.”~~

“*Boundary conditions are derived from the ECCO2 ocean state estimate and tides are incorporated as sea surface height and barotropic currents at the open boundary. We evaluate model results using satellite derived estimates of ice shelf melting and established compilations of ocean hydrography. WAOM qualitatively captures the broad scale difference* *between warm and cold regimes and many of the known characteristics of regional ice-ocean interaction. We identify a cold bias for some warm water ice shelves and a lack of HSSW formation. We conclude that further calibration and development of our approach is justified. At its current state, the model is ideal for addressing specific, process-oriented questions, e.g. related to tide-driven ice shelf melting at large scales."*

Response to Review #1

In this manuscript the authors describe a new regional model configuration (WAOM) that encompasses the entire Antarctic continental margins, including the cavities beneath Antarctica's floating glaciers. The scientific focus of the model is on simulating ocean-driven melt of Antarctica's ice shelves. The manuscript describes the set-up and integration of the model at three different resolutions (10km, 4km and 2km grid spacing), the highest of which is ostensibly capable of fully resolving tidal and fine-scale processes that contribute to the circulation and stratification on the continental shelf. The authors evaluate WAOM using an ocean state estimate and independent modeled/measured estimates of Antarctica's ice shelf melt rates, focusing on the highest resolution solution. They concluded that WAOM acceptably reproduces the observed ocean stratification and ice shelf melt rates, and therefore is a suitable tool for addressing scientific questions related to mechanisms of ocean-driven melt. They discuss shortcomings and sources of biases in the model, particularly emphasizing WAOM's lack of an active sea ice component, and discuss future development and scientific goals for the model.

R1C1 My high-level evaluation is that this is a significant model development that is worthy of publication in GMD. As the authors note, this is the first ocean model with (borderline) tidal- and eddy-resolving resolution that includes all of Antarctica's ice shelves, and therefore offers insights into the role of these processes in modulating Antarctic ice loss at the continental scale. The description of WAOM is appropriate for a model definition manuscript, and configuration and analysis scripts are provided (remotely) to allow for complete reproducibility. The manuscript and figures are very clearly composed.

We thank the reviewer for this positive feedback. We would like to highlight that the reviewer rates the development step that WAOM v1.0 represents as significant enough for publication.

R1C2 Below I have provided a series of comments and suggestions on the manuscript for the authors' consideration. My most significant concern pertains to their validation of the ocean state: 1. Offshore, the model is evaluated against the Southern Ocean State Estimate (SOSE), which contains biases of its own, particularly close to the Antarctic margins (see e.g. Dotto et al., 2014, Ocean Sci.). It was unclear to me why the authors chose to SOSE rather than directly against measurements. 2. On the continental shelf the authors perform only a qualitative evaluation of the ocean state, rather than performing direct comparisons. I was surprised by this, because the ocean state on the shelf is critical in determining the circulation and melt rates in the ice shelf cavities. Based on this, I would argue that the hydrography on the continental shelf should be the most closely scrutinized aspect of the model state. Previous studies have compiled measurement from the continental shelf from all around Antarctica in order to compute trends in shelf properties (Schmidtko et al., 2014, Science), characterize different dynamical regimes on the continental shelf (e.g. Amblas and Dowdeswell, 2018, Earth Sci. Rev.) and evaluate models (e.g. Morrison et al., 2020, Sci. Adv.). In my opinion, the manuscript would be strengthened significantly if the authors used one of these datasets to map biases in shelf properties in WAOM.

This comment challenges our general evaluation strategy. We now include a quantitative comparison of the ocean hydrography against observational estimates, closely following the reviewers suggestion (see Sect. Ocean Evaluation). We agree with the reviewer that this has strengthened the manuscript considerably.
Regarding 1.: We had chosen SOSE as it provides estimates for the year 2007, omitting some questions related to interannual variability. However, we acknowledge that SOSE is not the same as observations and in fact is just another model, although it is constrained by data where it is available. Instead, we now use WOA18 climatologies as our main product to evaluate the off-shelf hydrography.
Regarding 2.: We have now added a quantitative comparison of the predicted on-shelf hydrography against observational estimates from WOA18 climatologies and Schmidtko et al. (2014). This first assessment of the on-shelf conditions has helped to explain biases in ice shelf-ocean interaction and provides a sound starting point for future development. We would like to note, however, that WAOM can be used to address many different research questions and future studies intending to use this tool to study processes other than ice shelf melting and beyond hourly to seasonal time scales, will need to tune and evaluate the model to their needs.
Related changes to the manuscript have been outlined earlier (Sect. Ocean Evaluation).

R1C3 L4-5: How significant is lack of interannual variability? Is it possible that some of the biases in the modeled shelf stratification and melt rates occur because the model excludes anomalous years, e.g. years with particularly strong/weak winds or warm/cool atmospheric temperatures?

This is a great question. Also low-frequency intrinsic ocean processes might contribute to interannual variability in the model (Gwyther et al., 2018). Our application of the model using repeated year forcing allows us to study processes with hourly to seasonal time scales. However, we do acknowledge that limitations related to longer scale variability might impact the mean state. With the performed experiments, we can not answer to which degree this is the case. We have added a note to the limitations paragraph, including a statement about the timescales WAOM v1.0 has been designed for (lines 562-566):

“Further, the forcing schemes of this first version of WAOM have been designed to study phenomena with hourly to seasonal timescales (e.g. tides and summer surface water advection). To address scientific questions related to inter-annual change, these schemes will need to be extended first. We note that neglecting larger scale variability from interannual change or intrinsic processes (Gwyther et al., 2018) might impact the mean state of the model.”

R1C4 L61: “beyond the scope of this study” doesn’t really mean anything. If it’s not in the study then it’s beyond the scope by definition. It would be more helpful to state why an evaluation matrix was not pursued. Also is model tuning really “rigorous"? A right answer for the wrong reasons is not necessarily better than a slightly wrong answer based on well-grounded physical parameter choices.

Development of an evaluation matrix was not pursued because we did not have the time resources to do both (compilation of available observations and model development). The difficulty behind evaluation of this particular kind of model has been discussed above (L46-49) and just been picked up here again to define the scope of this study. We agree with the reviewer that "rigorous" is not the right word here. We have removed the whole statement as it seems to cause conflict rather than clarification. We have changed the sentence in question from (lines 71-75):

"~~*Establishing an evaluation matrix and rigorous model tuning is out of the scope of this study, but we aim to convince the reader that WAOM is capable of [...].*~~" to:

"We aim to convince the reader that WAOM is capable of [...] ".

R1C5 L93: Horizontal grid sizes in the form M x N would be more relatable than total number of computational cells.

We agree and have changed the respective numbers from (line 108):

"~~*[...], which results in 52, 130 and 260 million computational cells, respectively*~~." to:

"*[...], which results in 530x630, 1325x1575 and 2650x3150 horizontal cells, respectively.*"

R1C6 L98: Are the authors referring to the scheme of Shchepetkin and McWilliams (2003)?

Yes. We have added the citation at the end of the sentence (line 114):

"*ROMS is designed to minimise this issue by applying the splines density Jacobian method for the calculation of the pressure gradient force (Shchepetkin and McWilliams 2003).*"

R1C7 L100, L304-311: How wide does the "vertical cliff" at the ice shelf face become with this smoothing? Does this bias the model toward more Mode 3 ice shelf melt?

How well WAOM represents processes at the ice shelf front is a great question. In WAOM we have used standard smoothing routines, established in regional studies (Galton-Fenzi et al., 2012; Cougnon et al., 2013; D. E. Gwyther et al., 2014). Assessment of the impact of these routines on the model solution warrants its own studies better done using idealized or regional experiments (similar to Schnaase and Timmermann, 2019). Further, the realism of frontal processes in different coordinate ocean models is ongoing research. New mechanisms for allowing surface waters to access ice shelf cavities continue to be discovered and there is evidence that the unrealistic ice front representation in sigma-coordinates actually compensate for unresolved processes (Malyarenko et al., 2019). WAOM predicts enhanced front melting in many regions, highlighting the importance to answer these questions. The original manuscript already highlights this issue as future work (L 395-397). We now have added a note to the model description (line 119):

"[… we smooth the bathymetry and ice draft iteratively until a maximum slope factor r = (hi − hi+1)/(hi + hi+1) ≤ 0.3 is satisfied … .] While this approach has been developed in regional studies (Galton-Fenzi et al. 2012; Cougnon et al. 2013; D. E. Gwyther et al. 2014), the impact of these manipulations on the ice front representation and related processes (e.g. Mode 3 melting) is unknown."

R1C8 L102: Is the algorithm applied to both the ice shelf draft and the bathymetry, or only to the water column thickness?

We are now more explicit about the smoothing approach. The respective paragraph has changed from (lines 116-119):

"~~*Using the Mellor-Ezer-Oey algorithm (Mellor et al., 1994) we smooth the bathymetry and ice draft iteratively until a maximum slope factor r = (hi − hi+1)/(hi + hi+1) ≤ 0.3 is satisfied (h describes either water column thickness or sea floor depth)*~~" to:

"We apply the Mellor-Ezer-Oey algorithm (Mellor et al., 1994), which is well established for bathymetry smoothing (Sikirić, Janeković, and Kuzmić 2009). We smooth the bathymetry and water column thickness directly until a maximum slope factor r = (hi − hi+1)/(hi + hi+1) ≤ 0.3 is satisfied for both. The ice draft is then redefined as the superposition of bed and water column thickness."

R1C9 L105: Could the authors elaborate on "one of the smallest modifications possible"? I don't understand why there would be a hard limit on the ice shelf thickness - only an increasingly severe time step constraint as the thickness approaches zero.

We agree with the reviewer. 20 m is only a practical limit to stay within a reasonable time step range. We have changed the respective statement (line 124):

"*20 m is considered* ~~*one of the smallest modifications possible*~~ small within reasonable stability constraints *(Schnaase and Timmermann, 2019).*"

R1C10 L115-118: I presume that the authors impose heat and salt fluxes, rather than just buoyancy fluxes.

Yes, we are more precise now (line 134):

"At the surface, we apply daily ~~buoyancy fluxes~~ heat and salt fluxes [...]".

R1C11 L116, L399-400: Imposing surface wind stresses directly with no accounting for sea ice is a significant caveat. For example, recent Arctic-focused work shows that sea ice plays a significant role in modulating the stresses felt at the ocean surface, and the ocean surface currents (see Meneghello et al. 2018, GRL and other subsequent papers from John Marshall's group).

We acknowledge that wind stress modulation by sea ice plays a significant role for the seasonal variations of the larger scale circulation and hydrography in the Arctic (Meneghello, Marshall, Timmermans, et al. 2018; Meneghello, Marshall, Campin, et al. 2018). How well this conclusion can be transferred to the Antarctic, however, is not clear - and thus the significance of this caveat is likewise unclear. The importance of wind variability for Antarctic coastal hydrography and ice shelf melting has been shown in several regions and for various timescales: weekly to monthly variations of thermocline depth in Pine Island Bay (Davis et al. 2018); seasonal variations of shoreward heat transport in the Amundsen Sea (Kimura et al. 2017); interannual variability of Totten ice shelf melting (Greene et al. 2017); seasonal suppression of the thermocline in the Weddell Sea (Hattermann 2018). However, how sensitive these phenomena are to stress modulations by sea ice has yet to be quantified. In fact, the only Antarctic sensitivity study that we are aware of concludes that sea ice wind stress modulations are irrelevant for seasonal variations of the Ross Sea circulation (Jendersie et al. 2018). WAOM would be well suited to extend this sensitivity study to all Antarctic regions. If indeed coastal hydrography in other Antarctic regions is sensitive to sea ice modulation by wind stress, WAOM should be equipped with the parameterisations developed by (Jendersie et al. 2018), or use drift ice observations for surface stress instead. All of these ideas would be worthwhile pursuing in future work.

We have added a note to the model description (lines 150-153):
“[We do not account for the effect of sea ice on wind stress or frazil ice formation (as in, e.g. Galton-Fenzi et al., 2012).] While wind stress modulation by sea ice has been shown to play an important role for the circulation and hydrography in the Arctic (Meneghello, Marshall, Timmermans, et al. 2018; Meneghello, Marshall, Campin, et al. 2018), its importance for Antarctic ocean-ice shelf interaction has yet to be constrained (as discussed in (Jendersie et al. 2018).”

We also have elaborated on our proposal for future work, changing the respective paragraph from (lines 630-636):
~~“*Wind stress has been shown to impact ice shelf melting (Davis et al., 2018; Greene et al., 2017), but how sea ice modulates momentum flux from the atmosphere into the ocean is still an open question (Lüpkes and Birnbaum, 2005; Nøst et al., 2011).*”~~ to:

“*Wind stress has been shown to impact ice shelf melting (Davis et al., 2018; Greene et al., 2017, Kimura et al. 2017, Hattermann 2018), but how sea ice modulates momentum flux from the atmosphere into the ocean is not well constrained (Lüpkes and Birnbaum, 2005; Nøst et al., 2011; see discussion in Jendersie et al. 2018). Jendersie (2018) provides a first parameterisation for windstress modulation by sea ice and performs sensitivity experiments using a ROMS configuration of the Ross Sea. The effects on the seasonal variations of the circulation are negligible. WAOM would be well suited to extend these tests for a pan-Antarctic context.*”

R1C12 L117-118: Please explain why not using a sea ice model is more likely to capture polynyas? This is counter-intuitive.

We now have elaborated on this statement. Following comment R3C20 we have also relaxed our statement from *prescribed surface fluxes accurately capture polynyas* to *prescribed surface fluxes accurately capture polynya position and strength (surface salt flux)*.

The sentence has changed from (lines 135-143):

"~~*Prescribing surface buoyancy fluxes, rather than including a sea ice model, is likely to more accurately capture polynyas that form in the lee of fast ice and icebergs, and are critical to resolve accurate ice shelf melting in cold regimes.*~~" to:

"Accurate coastal polynyas that form in the lee of fast ice and icebergs are critical to resolve accurate ice shelf melting in cold regimes (see Mode 2 melting described in Jacobs et al., 1992). Small scale katabatic winds and grounded icebergs play an important role for these polynyas (Kusahara, Hasumi, and Tamura 2010; Mathiot et al. 2010)*, but both (small-scale winds and ice bergs) are not well represented in current generation sea-ice models. Hence, prescribed surface buoyancy fluxes rather than including a sea ice model, is more likely to capture the position and strength of coastal polynyas."*

R1C13 L119-122: The authors have quite a few rather ad hoc changes to the surface forcing that warrant further explanation. Why do the authors reduce the positive heat flux into the ocean by half? Is this to simulate the sea ice albedo effect? I understand the physical motivation for changing the brine rejection and restoring surface temperatures from below freezing, but it seems inconsistent to do so when all other fluxes are fixed. How do the authors gauge that one month is a "long" time scale for surface relaxation?

Decoupling sea ice-ocean fluxes from the ocean state is known to create artificial water masses. The applied tuning aims to compensate for these effects, but compromises consistency in the conservation of heat and salt. This is an inevitable downside of our approach. The tuning has been developed in regional studies and not yet calibrated for the pan-Antarctic domain. We believe that 0.5 is a good starting point for the heat flux tuning parameter, as a similar approach is known to overestimate heat flux into the ocean by up to 51% (Jendersie et al. 2018). The overestimation of summer heat flux into the ocean arises from the blended products (NCAR/NCEP or ERA-Interim) that are used where no sea ice is present. These products do not account for latent heat of sea ice melting.

In the methods, we now communicate the limitations of the flux-forced approach more clearly and provide more background for the tuning. We have added the following note (lines 143-147):

"~~We tune the surface forcing by reducing positive heat flux into the ocean to half its original value, omit brine injection when the ocean is warmer than the freezing point and relax surface temperatures towards freezing when they are being forced below freezing.~~"

"Decoupling sea ice-ocean fluxes from the ocean state is known to create artificial water masses. A similar approach, for example, is known to overestimate heat flux into the ocean by up to 51% (Jendersie et al. 2018). To reduce such effects, we tune the surface forcing by reducing positive heat flux into the ocean to half its original value, omit brine injection when the ocean is warmer than the freezing point and relax surface temperatures towards freezing when they are being forced below freezing."

We now communicate explicitly that few resources have been dedicated for model calibration (changes repeated from R1C3, Discussion, Future development; lines 514-516):

"A similar scheme is known to overestimate annual heat flux into the ocean by up to 51% (Jendersie et al. 2018). While we aim to account for this by reducing positive heat flux into the ocean by half (see Methods, Sect. 2.4), the approach has not been tested for pan-Antarctic domains."

The surface salt flux relaxation with timescales of one month is "long" in comparison to the daily atmosphere-sea ice-ocean flux forcing. The reviewer is right that we can not extend this statement easily to heat flux tuning. We have changed the statement to be purely factual (line 148):

"*Further, to avoid model drift, the surface ocean is relaxed~~ over long timescales~~ to the solution from SOSE [...].*"

R1C14 L125-126: I am skeptical about the claim that the model state at the boundary is primarily dictated by the interior. Surely this is not a desired outcome, as remotely-formed water masses (especially CDW) need to be supplied by the open boundary conditions.

We have introduced stronger nudging on inflow than outflow to reduce boundary effects. The reviewer is right, we do not actually know the consequences of this tuning for the model state at the boundary.

We change the statement to be purely factual (lines 155-156):
"~~The model solution, however, mostly dictates the conditions at the boundary, as ~~we nudge inflow and outflow with timescales of 1 day and 1 year, respectively."

<u>R1C15</u> Fig. 2: It looks like the melt rate drops instantaneously upon re-initialization with a 4km grid spacing, and again with a 2km grid spacing. Is there some geometrical impact of the grid refinement that causes this, or is the adjustment time scale just shorter than the monthly frequency of the model output that was used to create the plot?

It is a geometrical impact. Coarser resolution runs have greater ice shelf area than higher resolution versions (e.g. 4 km has 11% less area than 10 km). Most of the additional ice appears in frontal regions where melt rates are elevated.

We have added a note about this (lines 176-178):
"The instantaneous drops in melting upon re-initialization is caused by geometrical effects of the grid refinement. The ice shelf area reduces with increasing resolution (e.g. 11% between 10 km and 4 km), predominantly in ice shelf frontal regions, where melt rates are elevated."

R1C16 (a) Eq. (1): Shouldn't the denominator be 1/N. (b) Also, if Z_jˆm and Z_jˆo are complex variables (Z is the complex amplitude), then shouldn't the complex magnitude be taken before squaring (or equivalently multiplication by complex conjugate).

Regarding (a): In Eq. (1), the factor 2 in the denominator comes from the definition of the deviation variance for each harmonic X by Padman and Fricker (2005):

"At each location (xi, yi) where we have tidal harmonics from in situ data, we define the deviation variance for harmonic X, with observed (modeled) amplitude and phase ai,Xobs (ai,Xmod) and pi,Xobs (pi,Xmod) as:

$$d_{i,X}^2 = \frac{1}{2}\left(a_{i,X}^{obs}\cos\left(p_{i,X}^{obs}\right) - a_{i,X}^{mod}\cos\left(p_{i,X}^{mod}\right)\right)^2 + \left(a_{i,X}^{obs}\sin\left(p_{i,X}^{obs}\right) - a_{i,X}^{mod}\sin\left(p_{i,X}^{mod}\right)\right)^2.$$

The ensemble-averaged rms deviation for a set of N tidal stations is given by

$$E_X = \left(\frac{1}{N}\sum_i d_{i,X}^2\right)^{1/2},$$ "

In Eq. (1) shown in our manuscript, we have taken the factor 0.5 out of the sum (here the deviation variance is denoted as σ):

$$\sigma_x = \sqrt{\frac{1}{2N}\sum_{j=1}^{N}\left[Z_j^m - Z_j^o\right]^2},$$

The variance definition of a complex number involves the factor 2. This arises from the idea that a complex number can be described by two real and uncorrelated vectors (e.g. well explained in the prepress from Robert G. Gallager[2]).

Regarding (b): The reviewer is right. We have changed the formulas accordingly ("| |" instead of "[ ]"):

$$\sigma_x = \sqrt{\frac{1}{2N}\sum_{j=1}^{N}\left|Z_j^m - Z_j^o\right|^2},$$

and

$$\sigma_{comb} = \sqrt{\frac{1}{2N}\sum_{k=1}^{4}\sum_{j=1}^{N}\left|Z_{k,j}^m - Z_{k,j}^o\right|^2},$$

R1C17 Eq. (2): Shouldn't the denominator be 1/(4N)? Also, shouldn't the Z_jˆm and Z_jˆo be indexed by k as well, to distinguish tidal components.

[2] https://www.rle.mit.edu/rgallager/documents/CircSymGauss.pdf

We do not take the mean of the deviation of all constituents, but the sum. Hence, factor 2 in the denominator comes from the discussion above and no further factor is required. We have corrected the indexing for Equation 2 to include k:

$$\sigma_{comb} = \sqrt{\frac{1}{2N}\sum_{k=1}^{4}\sum_{j=1}^{N}\left|Z_{k,j}^{m} - Z_{k,j}^{o}\right|^{2}} \,,$$

<u>R1C18</u> L175-176: Is a 2005 tide model still considered state-of the art?

We are not aware of significant improvements (or a study that shows these). However, we do acknowledge that referring to a 2005 study with *state-of-the-art* causes confusion. We have removed *state-of-the-art* from the statement. We have also updated our tide assessment study resulting in a slightly better performance.

Overall, the performance statement changes from (lines 245-246):
~~*"The model has a combined RMS error of 27 cm, 17 % higher compared to an RMS error of 23 cm for state-of-the-art 2D Antarctic tide models assessed in King and Padman (2005)."*~~
to:

"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)."

We also have added a description of the update in the methodology (lines 236-237):
"[... fails to converge.] We also disregard 3 stations, which are noted as partially grounded and show non-sinusoidal and complex behaviour (70 Amery IS, 43 Rutford ISTR, 106 Evans ISTR).".

R1C19 Table 2: I think I understand how the stated quantities (e.g. RMSD phase in deg) are related to equations (1) and (2), but the naming convention and formulation of equations (1) and (2) make this much less clear than it could be.

The reviewer makes a fair point here. We have not presented the equations for the second and the third row of Table 2 (RMSD amplitude and RMSD phase). It is not custom to present this much detail (see e.g. Padman and Fricker, 2005; King and Padman, 2005) and we do not discuss these quantities in the text. Discussion of the RMSD of the complex amplitude is sufficient. We clarified the Table by removing the two rows in question and printing the mathematical symbols for the RMSD complex amplitude and combined complex amplitude (as in the equations above).

Table 2 changed from:

	M2	S2	O1	K1
Number of ATG stations	101	94	87	79
RMSD amp in m	0.23	0.18	0.07	0.09
RMSD phase in deg	27.14	22.65	8.92	8.30
RMSD complex amp in m	0.20	0.15	0.06	0.07
Combined complex RMSD in m		0.27		

to:

	M2	S2	O1	K1
Number of ATG stations	98	91	87	79
σ_x [m]	0.14	0.11	0.06	0.08
σ_{comb} [m]		0.20		

.

R1C20 L190-191: Should we expect the model state to asymptote under grid refinement? After all, various model parameters (e.g. viscosities/diffusivities) implicitly change with the grid, as does the bathymetry.

The reviewer's concerns are justified. We do not perform a CFD-like resolution study, as we change several aspects of the model at the same time (sub-gridscale turbulence description, eddies, bathymetry, ice draft, tidal effects, etc). We use the term "model" in a very wide sense, including all these aspects and our experience with them. As stated in the text, convergence of this wide-sense model shows "that we start resolving what is most critical to our problem". We would hope that as we resolve these critical aspects better (and tune the other parameters according to our experience), the solution converges further. We acknowledge that this discussion is somewhat philosophical and we have changed the text to reflect this from (lines 260-268):

"~~Grid convergence confirms that we start resolving the processes most critical to our problem. The model solution, however, has not yet reached asymptotic behaviour, motivating further refinement.~~" to:

"We note that several aspects related to model resolution have been changed simultaneously (bathymetry, ice draft topography, horizontal viscosity, horizontal diffusion, the model's ability to resolve physical processes such as internal tides and eddies). Thus, we use the term "model" in its widest possible sense here, referring to all these aspects together. From 10 km to 1 km, we expect the model solution to be less dependent on resolution, as we start resolving the processes most critical to our problem. Demonstrating convergence of WAOM as a whole is an important first step, proving consistency between our understanding and the models behaviour. Attribution of change to the individual resolution dependent aspects is also important, but out of the scope of this study, as it would require several additional series of experiments (discussed later)."

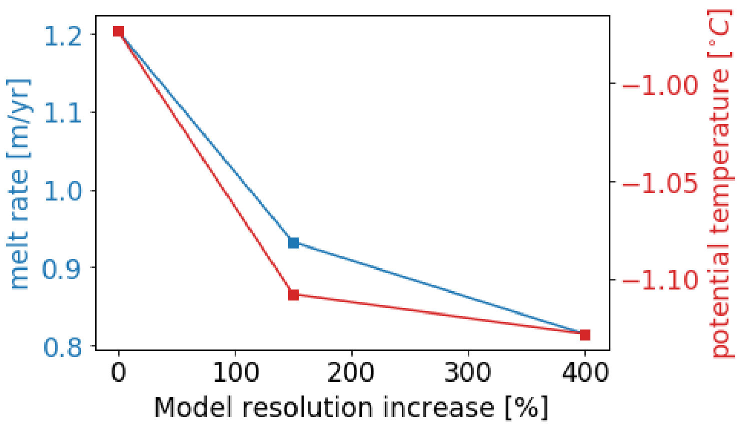
We also describe future experiments in this regards in more detail (lines 648-653):

~~"Repeating the resolution experiment introduced in this chapter, but, successively deactivating tides and keeping the bathymetry resolution constant, would unravel the impact of grid spacing on shoreward heat flux from tides, eddies and bathymetry.~~"

"Extending the resolution study introduced here would help to attribute the convergence behavior to individual aspects of the model. Future experiments should be designed to isolate effects due to changes in bathymetry, ice draft, tides and sub-grid scale turbulence parameterisation. This way, changes in shore-ward heat flux with increasing model resolution could be more clearly related to better representation of troughs, eddies and internal tides."

R1C21 Fig. 4: Is 10km->4km grid spacing a 250% increase in resolution? If the resolution is defined as the number of grid points per km, then the resolutions are 0.1, 0.25 and 0.5 kmˆ-1. So 10km->4km->2km grid spacings correspond to resolutions of 0.1->0.25->0.5 kmˆ-1, so the resolutions have increased by 0%, 150% and 400% relative to the 10km grid. Or the resolutions have been multiplied by 100%, 250% and 500% relative to the 10km grid. Please pick a consistent convention!

We corrected this mistake and changed the values for the increase in resolution to 150% and 400% in the text and Figure 4:



This mistake does not affect our discussion.

R1C22 L215-222: I was initially confused by the authors' explanation of the RSBW and WSBW salinities, which the attribute to the (ECCO2-sourced) open boundary conditions. I would have hoped that water masses formed within the domain would not depend on the boundary conditions. That said, the model boundary cuts right through the Weddell Gyre, so perhaps it is reasonable to have inflow of some bottom waters. Perhaps the authors could clarify this in the text?

The reviewer makes a good point here. RSBW and WSBW are not entirely formed within the domain. They are the combination of dense shelf water (expected to form within the domain) mixing with other deep water masses like CDW (inherited from the boundary conditions) as they are exported down the continental slope. The model has been designed to resolve accurate ocean conditions on the continental shelf. The off-shelf ocean should be seen as a sponge layer, likely affected by boundary and initial conditions. We have now included a comparison of bottom layer hydrography between WOA18, ECCO2 and WAOM (see Fig. 13, also see Sect. Ocean Evaluation). This comparison shows indeed that most of WAOM's deep ocean biases originate from ECCO2 boundary and initial conditions. We now define the boundaries of the model solution upfront (repeated from Sect. Ocean Evaluation, Methods; lines 203-204):

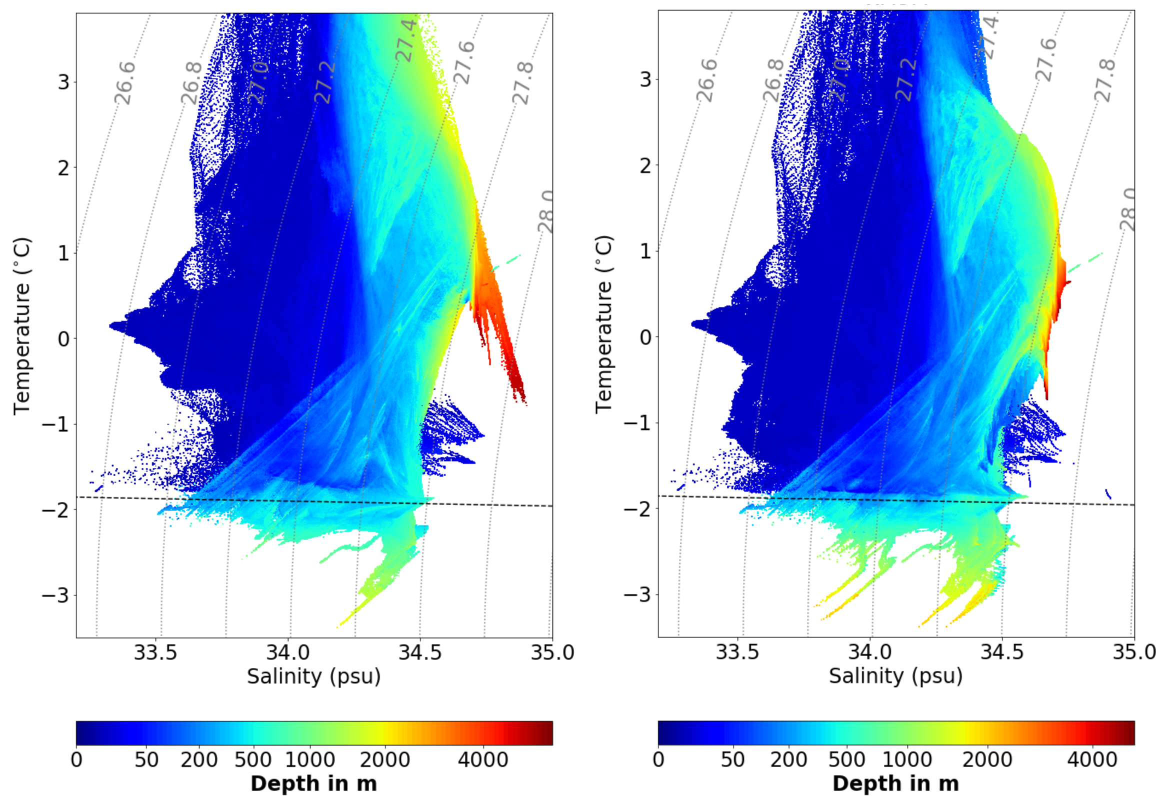
"*The northern extents of the off-shelf ocean should be seen as a sponge layer, likely affected by ECCO2 boundary and initial conditions and not fully spun-up using our procedere.*"

And state the source of the bottom layer bias in the results (repeated from Sect. Ocean Evaluation, Results: Off-shelf hydrography; lines 468-469):

"*WAOM shows an overall warm bias by about 0.3 °C, which can clearly be attributed to the initial and boundary conditions from ECCO2. Bottom layer salinities in both models agree well with WOA18.*"

R1C23 I am very confused by the waters with salinities reaching 34.8 in Fig. 6 (only in old manuscript). There is no other water anywhere in the model domain with such high salinities, and these high salinities are only found at great depth, far from the surface. So, what is the source of the very salty bottom waters?

We have made a mistake in the plotting routine, mixing up latitude and longitude. Instead of showing pan-Antarctic water masses south of 65 degS latitude, we originally plotted all water in the domain (incl. its boundaries) from 65-180 deg W. We have corrected the figure (left old; right new):



In the corrected figure salinities in the deep do not exceed 34.7. However, a source for these very salty bottom waters is still not apparent. The plot shows surface waters with comparable densities at about -1 degC, but these are mixed with lighter waters close to the surface, rather than descending to greater depths. A possible explanation is that the salty bottom waters are sourced by initial and/or boundary conditions from ECCO2 (also suggested by the reviewer under R1C22). WAOM does not generate sufficient HSSW to fuel these bottom waters. The lack of HSSW formation has been identified and discussed earlier (Sect. Ocean Evaluation). We now also have included a comment about the source of the salty deep waters (repeated from Sect. Ocean Evaluation, Results: Off-shelf hydrography, lines 564-566):

"The densest waters in WAOM show only little isopycnal mixing with colder surface waters (also see Fig. D2). In agreement with the earlier identified lack of HSSW formation, this hints towards bottom waters in WAOM, which are mainly sourced by initial and boundary conditions from ECCO2."

The corrected figure is shown as supplemental material now (Fig. D2; as a consequence of the changes under Sect. Ocean Evaluation). We note that other TS-diagrams are not affected by this mistake.

R1C24 Fig. 6 (only in old manuscript): I suggest adding more lines/arrows to indicate water mass locations more precisely. The CDW label looks to be much too fresh (e.g. compare with Fig. 7; removed in new manuscript), and AABW is labeled at a lower density than CDW!

We have placed the labels more carefully in the new reference figures (Fig. 8, 15 and D2). As the salinity and temperature ranges for the individual water masses are not precisely constrained, we decided against lines and arrows to reflect this.

R1C25 Also, I presume the portions of T/S space labeled "Weddell Sea" and "Ross Sea" are actually the Filchner-Ronne Ice Shelf cavity and the Ross Ice Shelf cavity, respectively.

The reviewer is correct. Water masses in the sub-ice shelf cavities are now only presented in the appendix (Fig. D2; as a consequence of the new evaluation strategy). We now have labeled the source region of ISW in this figure precisely. The new presentation of on-shelf water masses displayed in the manuscript (Fig. 8b) does not include the cavities and attribution of remaining ISW does not add value to the discussion.

R1C26 L235: Are the transects all annually-averaged? Are there significant deviations in the agreement between SOSE and WAOM in different seasons?

Yes, these are annual averages. We are now more explicit about this in the text (e.g. see Sect. Ocean Evaluation, Results) and the captions of all relevant figures (e.g. Fig. 8 and 9). The focus of the ocean evaluation lays now on the shelf, leading us to remove off-shelf stratification from the manuscript (including previous Fig. 7; see discussion under Sect. Ocean Evaluation, Results: Off-shelf hydrography).

Seasonality of model prediction is a great question. However, we only aim to provide a first bias estimate of the ocean conditions for WAOM. Future studies aiming to investigate seasonal processes will need to tune and evaluate the model to their needs. We communicate this more clearly now and provide suitable datasets for future evaluations (see first point under Sect. Ocean Evaluation, Discussion: Future development).

R1C27 Fig. 7 (removed in new manuscript): Why does the grid spacing in WAOM appear to be so coarse? The model grid spacing is 2km but the data points in this figure appear to be spaced 50-100km apart.

Fig. 7 has been removed from the manuscript (see discussion under R1C26 and Sect. Ocean Evaluation, Results). WAOM's computational grid is not aligned with geographical coordinates, hence the data has to be interpolated to longitude transects. We had chosen to use a target resolution identical to SOSE (⅙ deg) which is sufficient for the comparison and computational efficient. High resolution transects are showcased for the on-shelf (Fig. 14).

R1C28 Fig. 7 (now removed from the manuscript): Given that the authors have just compared a few transects here, why not align the transects with WOCE transects? Then they could include a third column of panels showing the WOCE measurements for additional reference.

The focus of the ocean evaluation lays now on the on-shelf, leading us to remove off-shelf stratification from the manuscript (including previous Fig. 7; see discussion under Sect. Ocean Evaluation, Results). Transects on the shelf have been aligned with ship repeat tracks (see Sect. Ocean Evaluation, Methods; Fig. D1) as the reviewer suggests.

R1C29 Also, often these transects are visually very similar, especially with these colormaps. Difference plots would be more revealing with regard to the model biases.

We now compare WOA18 and WAOM along transects using difference plots (Figs. 11-13).

R1C30 L421: Has the model equilibrated in the higher-resolution cases? I don't see how the authors can judge this from just a one-year time series in the 2km simulation. I think it would be fairer to say that the model has equilibrated at 10km grid spacing, and has been continued from that equilibrated state at higher resolution.

We agree with the reviewer and have relaxed the respective statement from (lines 664-666):

"~~We have simulated present day conditions by spinning up the model to a quasi equilibrium with repeated 2007-forcing.~~" to:

"We have simulated present-day conditions by spinning up the model with repeated 2007-forcing. The model has equilibrated at 10 km grid spacing, and has been continued from that equilibrated state at higher resolutions (up to 2 km)."

R1C31 L433: It's really the "surface" stress that is uncertain: the wind stress is relatively well constrained by reanalyses, whereas the ice-ocean stress is much less well constrained.

We agree with the reviewer. The respective statement, however, has been removed as a consequence of new findings related to the on-shelf evaluation (see Sect. Ocean Evaluation, Summary and Conclusion).

R1C32 Table C2: Please specify which relaxation parameters pertain to the open boundaries vs the ocean surface.

We now have specified the boundary for each relaxation time scale in Table C2:

Table C2. Some key model parameters.

Parameter	**value (10/4/2 km resolution)**
Vertical resolution (# layers)	31
Vertical coordinate transformation equation #	2
Vertical coordinate transformation stretching function #	4
Surface stretching parameter	7
Bottom stretching parameter	8
Critical depth (m)	250
Baroclinic timestep (s)	900/360/180
Barotropic timestep (s)	25/10/5
Horizontal diffusivity ($m^2 s^{-1}$)	50/20/10
Horizontal viscosity ($m^2 s^{-1}$)	500/200/100
Relaxation time scale for tracers at the surface (days)	365
Relaxation time scale for ocean elevation at the surface (days)	3
Relaxation time scale for barotropic momentum at the open boundary (days)	3
Relaxation time scale for baroclinic momentum at the open boundary (days)	3
Open boundary outflow/inflow nudging factor	365

Response to Review #2

R2C0 This is an interesting manuscript that describes some of the more technical aspects of a new circum-Antarctic configuration of ROMS. The novel features are that it includes sub-ice-shelf cavities and tidal forcing, and it is run at high resolution. The subject matter certainly fits the remit of GMD and I think that it could make a valuable contribution to the growing literature on modelling the ocean circulation beneath the Antarctic ice shelves. However, I have a number of concerns with the way the model has been set up and validated, and I think the authors should clarify the reasons for the approach they have taken and provide a more critical evaluation of their results.

We thank the reviewer for this positive feedback. We would like to highlight that they too rate the development step that WAOM v1.0 represents as worthy of publication in GMD. We have elaborated on the design choices where questions have been raised and substantially strengthened the evaluation by comparing model results directly against ocean observations (see Sect. Ocean Evaluation). We are explicit about the revealed biases and communicate the development state of the model more clearly (see Sect. Ocean Evaluation, Discussion).

R2C1 Questions: 1) While terrain-following coordinates have a number of advantages, their performance over steep topography can be an issue. The authors apparently deal with this critical point in lines 95-106, but nowhere do they state the extent to which the topography of the ice front and continental slope have been modified by smoothing, or the magnitude of any residual pressure-gradient errors that they would expect after the smoothing process. This is an important point, because in Figure 7 (now removed) there are some very obvious artefacts in the model results that appear to be associated with topography. Could similar artefacts be affecting the properties and circulation at the continental shelf edge and at the ice fronts? If so, they could have a major impact on the results that have been presented.

This is a fair, but far reaching question. As the reviewer already points out, one of the challenges of using sigma coordinate is to strike a delicate balance between realistic topography (ice and bed) and little spurious mixing due to Pressure Gradient Force Errors (PGFE, well discussed in Naughten et al., 2018). It is customary to present the smoothness of the final topography using the slope parameter (r factor). Quantification of remaining artificial currents is not straight forward (Mellor, Ezer, and Oey 1994) and has not been done before for ice shelf-ocean models, where the concurrent change of bedrock and ice draft poses additional complexity. The effect of ice draft smoothing on ice shelf frontal processes is an active research question (Malyarenko et al. 2019; Wåhlin et al. 2020), best addressed in idealized or regional models first. We agree with the reviewer, that the off-shelf artefacts (previous Fig. 7) are likely related to PGFE. However, judged by the high resolution transects shown in Figure 14, we rate the level of localized artificial mixing on the continental shelf as acceptable. However, the entire on-shelf ocean seems overly mixed (see Fig. 11-13), possibly related to PGFE.
We are now more explicit about our smoothing procedure (see R1C8) and discuss the possibility of PGFE as a source for overly mixed conditions on the shelf (see Sect. Ocean Evaluation, Discussion: Future development).

R2C2 2) The surface forcing is unconventional, in that heat and freshwater fluxes are applied rather than derived from atmospheric variables driving a physical parameterisation of the atmosphere-ice-ocean exchange. This has the advantage of removing the need for a sea ice model that can introduce biases, but has the potential to introduce its own biases. Presumably that is the motivation for the rather ad hoc adjustments made to the fluxes, described in lines 119-124? Those adjustments really should be more carefully described and motivated. Furthermore the use of wind stress without a dynamic sea ice model is questionable. Wouldn't it be more consistent to derive ocean surface stress from ice motion observations? While wind stress may be a reasonable proxy for surface stress where the ice is in free drift, free drift is a poor assumption in many of the regions of interest in this study, particularly the Weddell Sea and almost all near-coastal regions.

Yes, the modifications aim to compensate for the absent coupling between sea ice-ocean fluxes and the ocean state ocean. This has already been discussed under R1C13. We have described the motivation and approaches in more detail now (changes to the text also presented under R1C13).
The issue of missing wind stress modulation by sea ice has already been discussed under R1C11 (including changes to elaborate on this issue in the manuscript). Deriving surface stress from ice motion is an interesting proposition and we have added it as suggestion to future work (lines 636-637; in addition to changes outlined under R1C11):

“If sea ice wind stress modulation is indeed important, ice motion observations could be included for assimilation or calibration.”

R2C3 3) It is not obvious why the model has been forced with one year of data repeated, especially when that one year is characterised by “a paucity of observational data” (lines 10-11). Why not choose a year with more data? On lines 130-132 the authors state that the strategy of using one year allows them to integrate the model to quasi equilibrium, but does it really reach that state in only 8 years? I think the authors should show some further diagnostics, such as domain-averaged temperature, salinity, KE, etc, to justify that statement.

At the time of development, 2007 has been one of the few years for which all input data has been readily available (2005-2010; SOSE, ECCO2, surface fluxes from sea ice observations, Era-Interim). 2007 has been selected from these years, because surface heat, salt and momentum fluxes did not deviate qualitatively from the 1992-2011 mean.
We acknowledge that our point about the paucity of observations is poorly formulated. It is not particularly 2007, but rather observations for any individual year. As a consequence of our new ocean evaluation, this statement has already been removed from the manuscript (see Sect. Ocean Evaluation, Abstract).
The statement about the model equilibrium has been bold. We agree that it is unlikely that the entire domain (incl. the deep ocean) equilibrates in just 8 years. We now emphasise that the off-shelf part of the domain should be seen as a “sponge layer” rather than part of the model's solution (see Sect. Ocean Evaluation, Methods). Hence, we only focus on the equilibration of the on-shelf ocean. For this, mean ice shelf melting is a very powerful quantity, as the model has been designed to derive accurate sub-ice shelf melt rates and they include the integrated information of the shelf ocean upstream. We have clarified the importance of ice shelf melting for our evaluation (Sect. Ocean Evaluation, Methods).
Further, as a consequence to R1C30, we have already relaxed our statement about the quasi equilibrium to the 10 km version only. After these changes, we believe it is no longer necessary to present the temporal evolution of additional quantities.

R2C4 4) It is also not clear to me why the authors have used one model (ECCO) to supply initial and open boundary conditions, then compared the results to a second model (SOSE). Comparing with observations (however limited) would give some kind of indication as to how realistic the results are, while comparing to the same model (ECCO) would inform us about how the differing model architecture and surface boundary forcing impacted the model evolution. Without knowing how ECCO and SOSE differ, it is not clear what conclusions can be drawn from the comparisons that are made between WAOM and SOSE. The problem is highlighted by Figure 6 (only in old manuscript), where WAOM and SOSE look completely different, particularly at depth. Is that because ECCO and SOSE are very different, or are the differences produced by the surface forcing? If it is the latter, how can the deep ocean have been so extensively modified in only a few years? It tends to suggest that the model is much too prone to deep convection, which is quite a common problem in Southern Ocean models. But does that happen everywhere, or is that all a product of those model artefacts that appear over steep topography (see 1 above)?

This has been a very valuable comment. We have changed our strategy for the ocean evaluation (see Sect. Ocean Evaluation). We now compare model results against WOA18 climatologies and have included ECCO2 in these comparisons (Fig. 12, 13). We have removed comparisons to SOSE (incl. Fig. 6). We did indeed find that the off-shelf ocean is impacted to large degrees by the boundary and initial conditions, explaining most of the biases for the off-shelf (see Sect. Ocean Evaluation, Results: Off-shelf hydrography).

R2C5 5) Figure 6 (only in old manuscript) raises some other serious questions about the results. The ISW and RSBW/WSBW mixing lines appear to point to a water mass that apparently doesn't exist (HSSW). That suggests that they were formed from HSSW that was prescribed in the initial conditions, but has since been used up and not renewed. If that is the case, it points towards a model that is not in a quasi-equilibrium state (question 3 above), but is still in the process of evolving from initial conditions to some other state. Similarly the water masses at 1000 m depth are much too warm, suggesting that the original CDW prescribed in the initial conditions has also vanished and has been replaced by something quite different. That suggests some issue with the surface fluxes (question 2 above). It also suggests (again) that a longer integration would see the model continuing to evolve to a different state. If deep waters as warm as 3-4 deg C found their way onto the continental shelf, ice shelf melt rates would be much higher.

We have made a mistake in plotting of Figure 6 (discussed under R1C23). The problem of mixing towards non-existent HSSW remains. We now have identified a lack of HSSW in the model and communicate this clearly (see Sect. Ocean Evaluation, Discussion: Biases). We also communicate missing isopycnal mixing of deep waters with the surface (see changes under R1C23). Bottom waters are fueled by boundary and initial conditions to large degrees. How far the off-shelf ocean is from a quasi equilibrium, however, is not relevant for this study. The off-shelf ocean should be seen as a “sponge layer” (now communicated explicitly under Sect. Ocean Evaluation, Methods).
The corrected TS-diagram (now included as Fig. D2) now shows cooler temperatures at 1000 m depth, more in line with CDW. Still, we have now identified a warm bias at the surface, impacting deeper water masses (mostly in the off-shelf regions, see Sect. Ocean Evaluation, Results: Off-shelf hydrography). This has been communicated clearly including a recommendation to tune surface fluxes in future studies (see Sect. Ocean Evaluation, Discussion: Future development).

R2C6 6) But perhaps the error is just in the plotting of Figure 6 (only in old manuscript). The deep ocean stratification suggested by the trajectory of points from RSBW/WSBW to the (poorly placed) AABW label appears to be much too strong. I find it hard to believe that the whole domain can have been so extensively altered in 8 years of integration (assuming the initial conditions taken from ECCO looked something like the SOSE results). Has some error been made in converting temperature to potential temperature, or density to potential density?

We have made a plotting mistake (related to the region plotted; discussed under R1C23). The new comparison (Fig. 12) looks much closer to the observations, rendering the comment about deep ocean stratification redundant. All labels have been placed more carefully (including the AABW one). All TS-diagrams have been plotted using the displayed quantities.

R2C7 7) (a) The problems with the water mass structure apparent in Figure 6 (only in old manuscript) are also seen in Figures 7 & 8 (only in old manuscript). On line 234 the authors state that “The stratification of WAOM agrees well with SOSE for the off-shelf ocean”, but I would disagree with that. In most transects the main pycnocline is not well represented, either in strength or depth, and the mid-depth salinity maximum appears to be absent (worryingly consistent with Figure 6). While this is far from being the only model to get the stratification wrong (it is notoriously hard to get right), I think the results presented here warrant more than the glib statement that they agree well with SOSE. (b) In Figure 8 (removed from new manuscript) the absence of a source water mass for all the colder forms of ISW is again apparent, while ISW seems much too prevalent in the Amundsen and Bellingshausen seas, where it is hardly ever observed. Cooling waters too efficiently in the cavities hints at a problem with the balance of heat fluxes into the boundary layer (KPP) and at the ice-ocean interface. But again, some of these issues might arise because of the plotting, which makes the stratification look very odd. (c) The densest waters are shaded blue (Figure 8), implying they are at the surface. How is that possible?

Regarding (a), we now have focused our evaluation on the on-shelf regions (incl. their stratification) and results regarding the off-shelf stratification are no longer central to the manuscript (see discussion under Sect. Ocean Evaluation, Methods). We still respond to the reviewer’s concern here. We fully agree about their rating of the realism of WOAM’s off-shelf stratification. WAOM generates too much mixing (vertical for the off-shelf; vertical or horizontal for the onshelf). We do not know the source of the spurious vertical mixing, but suspect PGFE or the mixing scheme to be responsible (now discussed as future development, Sect. Ocean Evaluation, Discussion: Future development). Therefore, we agree with the reviewer that the original statement was too glib and it would be more correct to say that WAOM’s stratification in the off-shelf regions shows overly mixed conditions and the magnitude of the bias is comparable to other forward models. We now show and discuss spurious mixing for the on-shelf regions (Fig. 11-13; Sect. Ocean Evaluation, Results, Discussion), including suggestions for future development (see Sect. Ocean Evaluation, Discussion).

Regarding (b), the lack of HSSW production and artificially dense surface waters have been discussed earlier (see Sect. Ocean Evaluation, Discussion). In summary, we now have identified a lack of HSSW formation as a main bias and communicate explicitly that the uppermost 15 m of the model are not part of the model solution (also Sect. Ocean Evaluation, Methods). In addition to these points the reviewer is concerned about the realism of ISW. We agree with the reviewer that missing HSSW is apparent, as the Gade lines do not extend up to the surface freezing point (as, e.g. in Naughten et al. 2018), but rather are constrained to individual isopycnals at the salty end (e.g. 1027.8 kg m-3, in the densest case). We would like to note that this is not necessarily pointing towards non-equilibrated conditions on the shelf (i.e. remnants of HSSW from initial conditions), as the displayed behaviour can also be explained with a combination of isopycnal mixing and mixing along Gade lines. In this scenario, the second component for “Gade line-mixing” (the first is glacial melt water) has already been supercooled due to isopycnal mixing with melt water from upstream regions. We now have added a comment to the evidence of missing HSSW presented in Sect. Ocean Evaluation, Results (additions in bold, line 416-417):

"*In general, HSSW comprises the densest water mass on the shelf and mixes with other, lighter waters. As a consequence of its absence, all water masses in WAOM are well restricted by the same isopycnal of 1027.8 kg m-3* ***(also within the cavities, see Fig. D2).****"*

The lack of HSSW and the possibility of spurious vertical mixing are now discussed under future work (see Sect. Ocean Evaluation, Discussion: Future development).

ISW in the Amundsen-Bellingshausen Seas are only apparent inside the cavities (e.g. compare previous Fig. 8 (old manuscript) with Fig. 8 (new manuscript), where no ISW fresher than 34.0 g/kg is apparent). However, we are not aware of observations of ISW in this region, even inside the cavities. Potential artificial ISW in the AB-Seas is likely linked to the cold bias. This bias has already been identified using ice shelf melting and on-shelf hydrography (Ocean Evaluation, Results: On-shelf hydrography), and is now discussed in more detail (see Ocean Evaluation, Discussion: Biases). Figure 8 has been removed from the manuscript as a consequence of the new strategy for the Ocean Evaluation (see Ocean Evaluation, Methods). Therefore presentation of potential artificial ISW does not add much information to the discussion and we have not modified the manuscript in this respect.

Regarding (c), the fluxed forced approach is known to generate artificial water masses in the uppermost layers of the model. We have now excluded the top 15 m from our analysis and explicitly state that these regions do not belong to the model's solution (see Ocean Evaluation, Methods). Nevertheless, we mention this feature as part of the discussion around the lack of HSSW (repeated from Ocean Evaluation, Discussion: Biases; lines 532-533):

"[...]. Instead, waters with salinities higher than 34.5 g/kg are indeed present in the uppermost 15 m, but readily mix within this layer before reaching greater depths (Appendix Figure D2)."

R2C8 8) (a) Throughout the paper there is an implication that higher resolution is intrinsically better, but improvement in the model results with increasing resolution is never actually demonstrated. The implication of the discussion around Figure 4 (lines 184-191) is that at higher resolution still, shelf water temperatures and melt rates would drop further. However, at 2 deg resolution the mean melt rate has already dropped below the observed value and the 4 deg resolution simulation is arguably the best according to that single metric. (b) On a related note, I don't understand what the authors mean by "convergence" in that discussion. This term normally refers to the ability of a numerical code to reproduce an analytical solution as the grid size tends to zero. What solution should the model "converge" to in this case? The authors describe some features, cooling of the Bellingshausen Sea (line 196), for example, that appear to be worse in the higher resolution runs.

Regarding (a), the reviewer makes a fair point here. Yes, we implicitly convey that higher resolution is more accurate, as the model relies less on uncertain parameterisations. We are convinced that this is true for well calibrated ice shelf-ocean models and in the range of 10 km to 1 km. For WAOM v1.0 only few resources have been invested into calibration. Indeed, the 4 km solution of mean ice shelf melting is closest to observations and it could be argued that, if no further calibration is done, this solution should be used for scientific questions in this regard. However, depending on the research question, resolving relevant processes can be more important than model agreement with observations (as also stated by Reviewer 1 under R1C4). It is established in the field that eddies and troughs are important processes for ice shelf melting and need a kilometer scale resolution in models like ROMS (Dinniman et al. 2016). Ultimately, we should direct future efforts to get an eddying model (at least 2km) with realistic tides. WAOM is a major step towards this goal. We now have added a discussion around this point (lines 500-506):

"[These findings stress the importance of resolving tides at 4 km horizontal resolution or finer in large-scale models.] Studies aiming to use WAOM for future predictions should consider the option of applying it at 10 km or 4 km horizontal resolution for computational efficiency. Such studies will need to evaluate the model (at different resolutions) depending on their research question. Judging on the single scale metric of mean ice shelf melting, the 4 km solution of WAOM is closest to the observations (Fig. 4). For process oriented studies, however, we recommend using the 2 km version, as resolving eddies at a kilometer scale resolution is critical for accurate ice shelf-ocean interaction in some regions (Stewart and Thompson 2015). Ultimately, we should direct future efforts towards an accurate eddying model with tides."

Regarding (b), we use *convergence* for lack of a better term. "Model" includes our understanding of the processes and tuning (discussed under R1C20). If this "wide sense" model would diverge with increasing resolution, we would have a serious problem with our understanding of the models behaviour and the importance of the processes included. All we want to show is that this is not the case. We acknowledge that *convergence* often implies convergence towards a known solution in the field of model evaluation and that the term might be misleading. We now have defined what we mean by *convergence* (repeated from R1C20; lines 263-267):

"From 10 km to 1 km, we expect the model solution to be less dependent on resolution, as we start resolving the processes most critical to our problem. Demonstrating convergence of WAOM as a whole [...]"

and clarified that we only expect the model to converge towards **a** solution, not necessarily reality (at the end of the same paragraph, lines 268-269):

"*We note that we do not necessarily expect the model to converge towards the observations, without further calibration."*

We would like to highlight that most studies do not include any kind of grid resolution analysis.

R2C9 9) Again in Figure 10 (now Fig. 6), it is not entirely obvious what has been gained by the addition of tides and the use of high resolution. Results are different from previous studies, but not obviously any better. The spatial distribution of re-freezing is rather poorly captured (Figure 11; now Fig. 14): almost nothing on Amery and Larsen C ice shelves, and very little in the central Ronne Ice Shelf. Many of the early (admittedly) regional models did much better, despite being run at much lower resolution. That again points to the fact that increasing resolution does not necessarily improve results. I agree that a higher resolution model has the potential to improve the representation of reality, because it can resolve more processes, but no model can resolve every process, and the key to getting things right at any resolution is knowledge of how best to parameterise whatever remains in the sub-grid-scale. Arguably the authors have done a better job at that with the 4 km version of WAOM than with the 2 km.

It is well known that eddies, troughs and tides play an important role for ice shelf melting. Our model, for the first time, allows us to explore the impact of these processes on, and their interaction with, ice shelf melting in a circum-Antarctic sense. This is the key strength of our approach over previous large scale models. Beyond that we also represent processes (tides, eddies permitted by the higher resolution) which were completely excluded from many previous regional studies. These studies might have captured ice shelf melting at high resolution more accurately. However, that does not exclude the possibility that regional models might have been getting the right answer for the wrong reasons - as they missed important processes.

R2C10 In summary, while this circum-Antarctic model has the potential to be a useful addition to the growing collection of such tools, I feel the authors should do more to critically evaluate their results and explain the impact of including extra processes and using finer resolution. The main issues at present that make the model results questionable for the applications that the authors have in mind are the problems with water column structure (that may be related to sigma-coordinate problems over steep topography) and the curious water mass properties (Figure 6; only in old manuscript). If the latter are not due to misplotting, then it suggests some serious issues with the model (potentially associated with the surface flux forcing and/or the sigma coordinates). Those really need to be sorted out before the model can be considered fit for purpose.

There has been a plotting mistake in Figure 6 and the corrected figure looks much closer to observations (see R1C23). We substantially strengthened the model evaluation by providing a first bias estimate of the on-shelf ocean (see Sect. Ocean Evaluation). We have elaborated on the purpose of the resolution study (see R1C20, R2C8). We rate attribution of change to individual processes as out of the scope of this study, but propose experiments in more detail to address these questions (see R1C20). WAOM v1.0 indeed has biases (e.g. related to ocean stratification). We communicate these clearly (see Sect. Ocean Evaluation, Discussion: Biases) and propose future development to address them (see Sect. Ocean Evaluation, Discussion: Future Development). WAOM v1.0 is already being used for ice sheet coupling (pers. com. Rupert Gladstone), quantifying the impact of future climate scenarios (pers. com. John Moore) and studying dense water production (pers. Com. Petteri Uotila). Further calibration of WAOM is ongoing within these studies.

Response to Review #3

R3C0 In this paper, the authors report on the development of a new, high-resolution model of the Southern Ocean including its ice shelf cavities. The inclusion of tides represents a major feature of this model and a significant progress in scientific model development. The paper discusses model design and the evaluation of results.

The paper is well written and presents a lot of useful information. Figures are clear and well crafted. I recommend to accept the paper pending revisions guided by the following specific comments. Note that numbers 12 and 19 are a bit more substantial.

We would like to thank the reviewer for this positive feedback. They too rate the development step that WAOM v1.0 represents as worthy for publication.

R3C1. Throughout the text, I felt the urge to add a significant amount of hyphens in composite terms like “eddy-scale circulation”, “large-scale models”, “present-day conditions”, “eddyresolving horizontal resolution”, “nearest-neighbour method“, „Spin-up procedure“, „depth-averaged temperature“ etc. I trust this will be handled by the copy editor at one of the final stages of publication, but I also encourage the authors to revisit these composits.

We have corrected the listed composites.

R3C2. l. 23: To the list of papers trying to predict future changes, you may want to add Timmermann and Hellmer (2013).
We have added the reference.

R3C3. l. 27: It may be worth mentioning that several coupled ice sheet--ocean models exist already: Timmermann and Goeller (2017) with global ocean (but regional ice sheet) is one example, your co-author KA Naughten runs another.

We agree and have added (line 33):

“[... and coupled ice sheet-ocean models for climate predictions will ultimately need Antarctic-wide domains (Asay-Davis et al., 2017).] The first realistic, coupled models are now becoming available (Ralph Timmermann and Goeller 2017; Naughten et al. 2021).”

R3C4. l. 29: "augmented" (like an add-on) does not quite match the fact that at least some of these models were designed with ice shelves included right from the start. Two of the early, pioneering models of this kind were Beckmann et al. (1999) and Timmermann et al. (2002).

We thank the reviewer for this information. We have adapted the phasing from (lines 35-39): ~~*“Many ocean models with pan-Antarctic coverage have now been augmented by an ice shelf component (e.g. Hellmer, 2004; Timmermann et al., 2012; Kusahara and Hasumi, 2013; Dinniman et al., 2015; Schodlok et al., 2016; Mathiot et al., 2017; Naughten et al., 2018b; for review see Dinniman et al., 2016; Asay-Davis et al., 2017)”*~~ to:

“Many ocean models with pan-Antarctic coverage have either been designed with cavities from the beginning (Beckmann, Hellmer, and Timmermann 1999; R. Timmermann, Beckmann, and Hellmer 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., 2018). Reviews about ocean-ice shelf modelling are presented by Dinniman (2016) and Asay-Davis (2017).”

R3C5. l. 47: instead of “usually”, I would find “often” or maybe even only “sometimes” more appropriate.

We agree that “often” is more appropriate and have changed the term.

R3C6. l. 60: The statement that the model “includes all the model physics of state-of-the-art regional applications.” seems a bit daring to me, given that sea ice (which is commonly regarded as part of the ocean) is only roughly approximated in this model.

We fully agree with the reviewer and have changed the wording from (lines 69-73):

~~*“The Whole Antarctic Ocean Model (WAOM v1.0) includes tides and an eddy-resolving horizontal resolution of 2 km and, thus, includes all the model physics of state-of-the-art regional applications.*~~*”* to:

“The Whole Antarctic Ocean Model (WAOM v1.0) includes tides and an eddy-resolving horizontal resolution of 2 km, both known to be critical to resolve accurate ice shelf-ocean interaction.” (already shown under Sect. Ocean Evaluation, Introduction).

R3C7. l. 119: “[polynyas] are critical to resolve accurate ice shelf melting in cold regimes”: That's what people say. In fact, it is quite en vogue to stress the importance of coastal or flaw polynyas. And it is not totally wrong at all. If you do the budgets though (let's say: for the continental shelf of the southern Weddell Sea), it turns out that the leads in the (vast) pack add up more salt flux through sea ice formation than the (comparatively tiny) coastal polynyas. What does make coastal or flaw polynyas important indeed is the fact that they are persistent and stationary. See, e.g., Haid and Timmermann (JGR 2013). So that statement is not totally wrong, but maybe a tad on the simplifying side.

We thank the reviewer for this clarification. We agree that the statement is to simple and have changed added the following note (lines 138-141):

“*While flaw leads in the vast pack ice are likely to add more salt into the ocean in total (shown for the Weddell See region***,** see Haid and Timmermann, 2013*), coastal polynyas play a more critical role for regional ice shelf interaction due to their stationary character.***”**

R3C8. l. 149: “while Bedmap2 ice thickness data is mostly based on laser altimetry data from 1994 to 1995” Are you sure this is true? Bedmap2 is much younger than this. Please double-check.

We have double checked this. The original statement is true.

Fretwell et al. (2013) states: *“A single gridded dataset of ice thickness derived from satellite altimetry (Griggs and Bamber, 2011) provided full coverage and uniform consistency of all the significant floating ice shelves around Antarctica. This was adopted as the primary ice-thickness data source for these regions.”*

Griggs and Bamber (2011) states: *“We present a satellite retrieval of the ice thickness for all Antarctic ice shelves using satellite radar altimeter data from the geodetic phases of the*

European Remote-sensing Satellite (ERS-1) during 1994–95 supplemented by ICESat data for regions south of the ERS-1 latitudinal limit."

R3C9. l. 166: I believe it should be Z = H (cosG+i sinG) (with brackets)

We have corrected this mistake.

R3C10. l. 183 etc: (a) In the "Resolution effects" section, I think it would be very useful to not only discuss resolution-caused changes, but also whether these bring modelled hydrography closer to or further away from observations. Maybe this is easier if this section is moved to the end of the chapter? No preference, just an idea. (b) Judging from Fig. 4, I am not convinved that I find the statement "the model solution [....] converges with increasing resolution" fully justified. We are indeed far away still from an asymptotic behaviour. Which is probably true for the vast majority of models in use today, so I am not criticizing the model here.

Regarding (a): Evaluation and tuning depends on the question you want to answer. Here we focus on developing a tool for process oriented studies. Such studies should aim to get the right answers for the right reasons, i.e. use an eddying model. Future studies aiming to use WAOM as a tool for future prediction, however, should consider the option of using it at lower resolution. Such studies will need to evaluate the model (at different resolutions) depending on their research question. There are many different measures to assess the model. We have focused on melt rates, and indeed, for the single measure total mass loss it looks best for 4 km.

We now have communicated the scope of this study more clearly:

(repeated from Sect. Ocean Evaluation, Introduction; lines 74-76): "*This way, we aim to convince the reader that this first version of WAOM is realistic enough to be applied to specific, process oriented studies and to justify further development of our approach.*"

We have also added a discussion around the use and evaluation of WAOM at lower resolutions (repeated from R2C8; lines 495-506):

"[These findings stress the importance of resolving tides at 4 km horizontal resolution or finer in large-scale models.] Studies aiming to use WAOM for future predictions should consider the option of applying it at 10 km or 4 km horizontal resolution. Such studies will need to evaluate the model (at different resolutions) depending on their research question. Judging on the single scale metric of mean ice shelf melting, the 4 km solution of WAOM is closest to the observations (Fig. 4). For process oriented studies, however, we recommend using the 2 km version, as resolving eddies at a kilometer scale resolution is critical for accurate ice shelf-ocean interaction in some regions (Stewart and Thompson 2015). Ultimately, we should direct future efforts towards an accurate eddying model with tides."

We acknowledge that the term "convergence" is somewhat misleading (see discussion under R2C8). We now have defined what we mean by "convergence" (repeated from R1C20; lines 263-265):

"From 10 km to 1 km, we expect the model solution to be less dependent on resolution, as we start resolving the processes most critical to our problem. Demonstrating convergence of WAOM as a whole is [...]"

R3C11. l. 207: You may want to finish the sentence with "and the representation of narrow troughs at the continental shelf break (Nakayama et al., 2014)"

We gratefully apply this recommendation (lines 283-286):

"As mentioned earlier, this phenomenon is often associated with shoreward heat transport by eddies that need a grid spacing on the order of 1 km to be resolved by ocean models (Dinniman et al., 2016; Mack et al., 2019) and the representation of narrow troughs at the continental shelf break (Nakayama et al., 2014)."

R3C12. l. 220-222: This passage is not fully convincing. Stronger water mass transformation would (in my view) go via more or saltier HSSW - which (according to their statement a few lines above, and consistent with Fig. 6; removed from new manuscript) is not what the authors find. How does the model form WSBW with S>34.8 if no HSSW with at least the same salinity exists? There has to be a source somewhere, and I do not agree that finding this source can be beyond the scope of this study.

There has been a plotting mistake in Figure 6 (see R1C23). Dense waters shown by the corrected plot can be well explained by boundary and initial conditions (already discussed under R1C23, incl. changes to the text).

R3C13. l. 225-227, particularly with regard to "Which of the models is more accurate close to the surface and what is causing the differences is not clear.": A purely observation-based data product (like the World Ocean Atlas) might help.

We are now comparing model predictions against WOA18 climatologies (see Sect. Ocean Evaluation, Results: On-shelf hydrography, Results: Off-shelf hydrography). We have identified a warm bias at the surface and discussed this bias in the light of ice shelf basal melting (taken from Sect. Ocean Evaluation, Discussion: Biases; lines 533-536):

"The reported warm bias at the surface (Fig. 8) could also be linked to reduced HSSW formation. WAOM predicts elevated melt rates right at the ice front in most regions (close to coastal polynyas; Fig. 7) and ISW has been shown to be able to suppress dense water formation (Williams et al. 2016; Silvano et al. 2018)."

and proposed steps for future development (taken from Sect. Ocean Evaluation, Discussion: Future development; lines 610-616):

"Second priority should be given to the calibration of the surface heat flux, which is likely to reduce the warm surface bias. The warm bias towards the surface can not be explained by initial and boundary conditions, as ECCO2's upper ocean conditions are more realistic (see Fig. 15). Also, 2007 has not been an anomalously warm year (e.g. measured by sea ice extent; see Parkinson, 2019), rendering interannual variability as an unlikely source. Instead,

we suspect the applied surface flux schemes to be responsible. A similar scheme is known to overestimate annual heat flux into the ocean by about 50% (Jendersie et al. 2018). While we aim to account for this by reducing positive heat flux into the ocean by half (see Methods, Sect. 2.4), the approach has not been tested for pan-Antarctic domains."

R3C14. l. 232/233: "The z-like signature of ISW in the Ross Sea is likely caused by continued mixing of ISW from one ice shelf inside the cavity of another ice shelf downstream and this further supports the presence of ice shelf teleconnections." I think the statement here could/should be more precise. The idea is that these patterns are signatures of meltwater originating from ice shelves upstream from Ross Ice Shelf, right? So, this would be meltwater from the Amundsen / Bellingshausen Seas? I am not sure whether this explains the structure in the Ross ISW, but the idea of a teleconnection between this and those is supported by the findings of Nakayama et al. (2020).

This part of our results is a very small detail and we agree that the evidence for this hypothesis is very thin. We have removed this hypothesis from the manuscript (no longer present in the new ocean evaluation presented under Sect. Ocean Evaluation, Results: On-shelf hydrography, Results: Off-shelf hydrography).

R3C15. l. 239-248: To me it seems as if this whole paragraph calls for a model-to-data comparison instead of (or in addition to) model-to-model.

We now have compared the model to observations, instead of SOSE. We have identified overly mixed conditions in WAOM (see Sect. Ocean Evaluation, Results: On-shelf hydrography), discussed the consequences of this bias for predicted ice shelf melt rates and provided suggestions for future development (see Sect. Ocean Evaluation, Discussion: Biases, Discussion: Future development).

R3C16. Figure 9 (now Fig. 14): Having the ice shelf on the left of the plot in the section AND in the map would be nice.

We agree and have adapted the insets accordingly (see new Fig. 14).

R3C17. Figure 9 (now Fig. 14) again: Is the very sharp front in the (c) panels a simulation result or are we too close to the open boundary / sponge layer here?

This is a model result. The choice of colors scale (only up to -1.5) exaggerates the front, but is necessary to display the stratification inside the cavity. We have added a note to the text (taken from Sect. Ocean Evaluation, Results: On-shelf hydrography; additions in bold; lines 451-453):

"*In this region, CDW is held back from entering the continental shelf by a sharp front (the Antarctic Slope Front; **exaggerated by the choice of color scale**; in agreement with, e.g. Guo et al., 2019, their Fig. 2)."*

R3C18. l.283: I think it should be "in agreement or close to FOR others"

We have corrected this mistake.

R3C19. l.310: The finding that strong ice-shelf basal melting near the ice front in this model is a widespread feature needs some discussion in context with numerics / sigma coordinates. Is there any risk that the particularities of terrain-following coordinates create a certain tendency / bias here? If mixing is not carefully controlled and ideally rotated to density surfaces (instead following lateral coordinate lines), a spurious exchange between the openocean surface and the ocean in touch with ice the shelf base near the ice front may be something to keep an eye on, I think.

We understand the reviewers' concern. A smooth and sloping representation of the ice front in sigma coordinates favours elevated melt rates (also see discussion under R2C1). More ice shelf area is exposed to warm surface waters (geometrical consequence) and baroclinic transport is eased (dynamic consequence, see Wåhlin et al., 2020). However, the realism of frontal processes in models with different coordinates is an active research question. Malyarenko et al.(2019), for example, suggest that a smooth ice front representation in sigma coordinates actually compensates for unresolved processes that enhance surface water intrusion (the right outcome for the wrong reason). WAOM's results stress the timeliness of research in this area.

We already explicitly note this point under future studies (lines 628-630):

"Studying individual aspects of the model will help gain trust in quantitative results. Schnaase and Timmermann (2019), for example, show that artificially deepening the water column thickness near grounding zones (necessary for numerical stability), does not affect ice shelf average melt rates, and Malyarenko et al. (2019) suggest that the unrealistic ice front representation in sigma-coordinates, could actually account for unresolved small scale processes."

And now have added a discussion around this point (in addition to the biases presented under Sect. Ocean Evaluation, Discussion: Biases; lines 539-549):

"We note that elevated frontal melting in WAOM is likely favoured by its representation of the ice front. A sloping and smooth representation of the vertical cliff face exposes more ice shelf area to warm surface waters (a geometrical consequence) and eases baroclinic transport (Wåhlin et al. 2020). Ice shelf frontal processes and their representation in models, however, are not well explored. There is evidence, for example, that a smooth representation of the ice front in sigma coordinates actually compensates for an unresolved wedge mechanism that favours intrusions of water surface waters under the ice (Malyarenko et al. 2019). The results presented in this study stress the importance of further research in this area."

R3C20. l. 313, "accurate polynyas": Whether these are better in terms of giving the correct buoyancy flux than a prognostic sea-ice model may still be debatable. So, compared to resolution and tides, this may be a weaker point in the list of strengths of this model. Personally, I would concentrate on the "real strengths", with tides probably being the leader here, followed by resolution, and tune down the enthusiasm about the model's approach to sea ice processes. This may be a matter of scientific taste though and I do not insist that the authors follow my suggestion.

We agree with the reviewer about the list of strengths and their order. Accurate amount and position of the surface fluxes resulting from polynya activity does not necessarily translate into accurate polynya-driven effects on ice shelf-ocean interaction. The lack of HSSW in our model is evidence for this point. We now have removed accurate polynya activity from the strengths of the model and have put tides upfront (line 473):

"*Compared to other models, WAOM includes tides and an eddy resolving resolution~~, tides and accurate polynyas~~, a first for a circumAntarctic ice-ocean simulation."*

We already have specified which part of the polynyas are accurate with our approach (repeated from R1C12; lines 142-143):

"Hence, prescribed surface buoyancy fluxes rather than including a sea ice model, is more likely to capture the position and strength of coastal polynyas."

R3C21. l 330: It is SUCH a pity that results from coarser resolution or deactivated tides are not shown!

We do show the impact of resolution as difference plots in Fig. 5. Tidal modulations are the main results of a separate paper (Richter et al. in review) and can not be included here. We acknowledge that referring to these results without showing them is unsatisfactory for the reader. We have now relaxed the related statement (continental shelf cooling due to better resolved tidal effects) to a hypothesis and base this hypothesis purley on results shown in this study (resolution effects) and results from previous studies.

For this we have removed some results (lines 270-281):

~~"~~*When increasing the grid resolution from 10 km to 4 km, the shelf ocean cools at many places~~, most likely due to better resolved tidal processes.~~ We find that resolution-induced changes in depth averaged temperature are governed by changes in the bottom sigma layer (not shown). Figure 5 shows how bottom sigma layer temperatures change with increasing resolution. The ocean cools at many places when refining the horizontal grid spacing from 10 km to 4 km (Fig. 5a). Differences exceed 1 ◦C in the eastern Bellingshausen Sea and in the eastern Ross Sea, and are on the order of 0.25 ◦C in the Amundsen Sea and around the East Antarctic coastline. ~~We attribute most of these changes to better resolved tidal processes, based on additional sensitivity experiments that remove the tides (not shown). For example, activating tides in the model at 4 km resolution also leads to warm water intrusions that extend under the north-western part of the Ronne Ice Shelf and ocean temperature changes 200 that resemble a dipole pattern in the eastern Ross Sea. Also, in both cases, effects are well constrained by the continental shelf break, where tides start to weaken with increasing water column thickness. Finally, the overall reduction in continental shelf temperature has a similar magnitude in both experiments."~~*

And rewritten the discussion around this (lines 489-494):

"*The overall picture, however, is dominated by different processes. ~~Compared to our most complex simulation (2 km resolution and with tides), coarsening the horizontal resolution or deactivating tides (not shown) leads to a warmer continental shelf with similar regional changes.~~ Increasing the resolution leads to an overall cooling of the continental shelf. Similar studies without tides only report a warming with increasing resolution* (Nakayama et al. 2014; Dinniman et al. 2016)*, hinting towards better resolved tidal processes to be the cause."*

R3C22. l. 334, "spuriously low conversion rate of heat into ice shelf melting": This point I don't see, because even if you have a spuriously low ocean-to-ice heat flux, the transport of warm water onto the continental shelf is still the same, isn't it?

This is a misunderstanding. The reviewer is referring to heat transport across the shelf break, while we mean across the cavity entrance. Enhanced melt rates would cool the continental shelf ocean (given that heat flux across the surface and shelf break are constant). We have rewritten this discussion for clarification (lines 496-499).

"~~It appears that a poor representation of tides causes overestimated heat transport onto the shelf or underestimated heat loss out of the shelf ocean. The latter could be caused by decreased heat loss to the surface or a spuriously low conversion rate of heat into ice shelf melting. These findings stress the importance of resolving tides at 4 km horizontal resolution or finer in large-scale models.~~"

"A cooling continental shelf could either be realized by decreased heat flux onto the shelf, increased heat flux to the atmosphere/sea-ice or increased heat flux into the ice. (Stewart, Klocker, and Menemenlis 2018) find that tide driven heat flux across the shelf break is mostly balanced by mean flow and, in our simulation, melt rates also decrease with increasing resolution (Fig. 4) and changes in temperature are strongest outside the cavities (Fig. 5). Hence, we hypothesise that increased vertical mixing due to better resolved tidal processes are responsible for the reported continental shelf cooling with increasing resolution."

And leave the confirmation of this hypothesis to future studies (lines 654-656):

"To confirm our hypothesis that tidal mixing governs the reported cooling of the continental shelf ocean with increasing horizontal resolution, future studies should perform additional experiments without tides and apply heat flux analysis across the shelf break, surface and cavity entrance."

R3C23. l. 355-357: I will shamelessly advertise RTopo-2 here. That said, it is highly unlikely that everything in RTopo-2 is perfect.

We thank the reviewer for the advice. We do not have a preference. Both are state-of-the-art products. Now there is also a BedMachine (Morlighem et al. 2020). In the text, the discussion has moved away from biases in bathymetry (see Sect. Ocean Evaluation, Discussion: Biases).

R3C24. l. 359: In the list of studies on interaction between sea ice and ice shelves, you may REALLY want to add Timmermann and Hellmer (2013).

We gratefully follow this advice (lines 556-557; additions in bold):
"[...] having motivated many previous studies to include sea ice models (e.g. Hellmer, 2004; Timmermann et al., 2012; ***Timmermann and Hellmer, 2013****; Naughten et al., 2018)."*

R3C25. l. 360-362, "This study, however, prioritises accurate polynyas by prescribing surface fluxes from sea ice observations. While this is likely to result in more accurate melt rates at the base of the ice shelves" : I am not sure I agree with this. Having the polynyas at the right places is a good step, but the fluxes computed from there are probably much less well constrained.

We fully agree (see discussions under R1C12, R3C20). We now also have changed this part of the discussion to reflect this (lines 557-560) from:

~~*"This study, however, prioritises accurate polynyas by prescribing surface fluxes from sea ice observations. While this is likely to result in more accurate melt rates at the base of the ice shelves, "*~~ to:

"This study, however, follows an approach that prescribes surface fluxes from sea ice observations to accurately capture the position and strength of coastal polynyas. While this is a major component towards accurate ice shelf melt rates, [...]"

R3C26. l. 367/368, "This design, however, has been chosen to simplify future efforts that aim to couple WAOM with models of Antarctic ice sheet flow": This has just been said (two sentences back).

We agree and have removed the repetition from the beginning of the paragraph (line 567):

"The many wasted land cells in WAOM's domain could also be considered a limitation~~, but the model design simplifies future coupling with models of ice sheet flow~~."

R3C27. l. 406: "harness": Sure? Maybe "harvest"?

We believe the use of "harness" is more appropriate here.

R3C28. l. 420: Limitations of using just one particular year over and over again as atmospheric forcing need to be discussed. Think of periodic modes of variability and how each of these modes is randomly sampled in one particular phase and then repeated over and over again.

The consequences of missing interannual variability in the model have already been discussed under R1C3 (incl. changes to the text). This discussion includes atmospheric forcing.

Figures

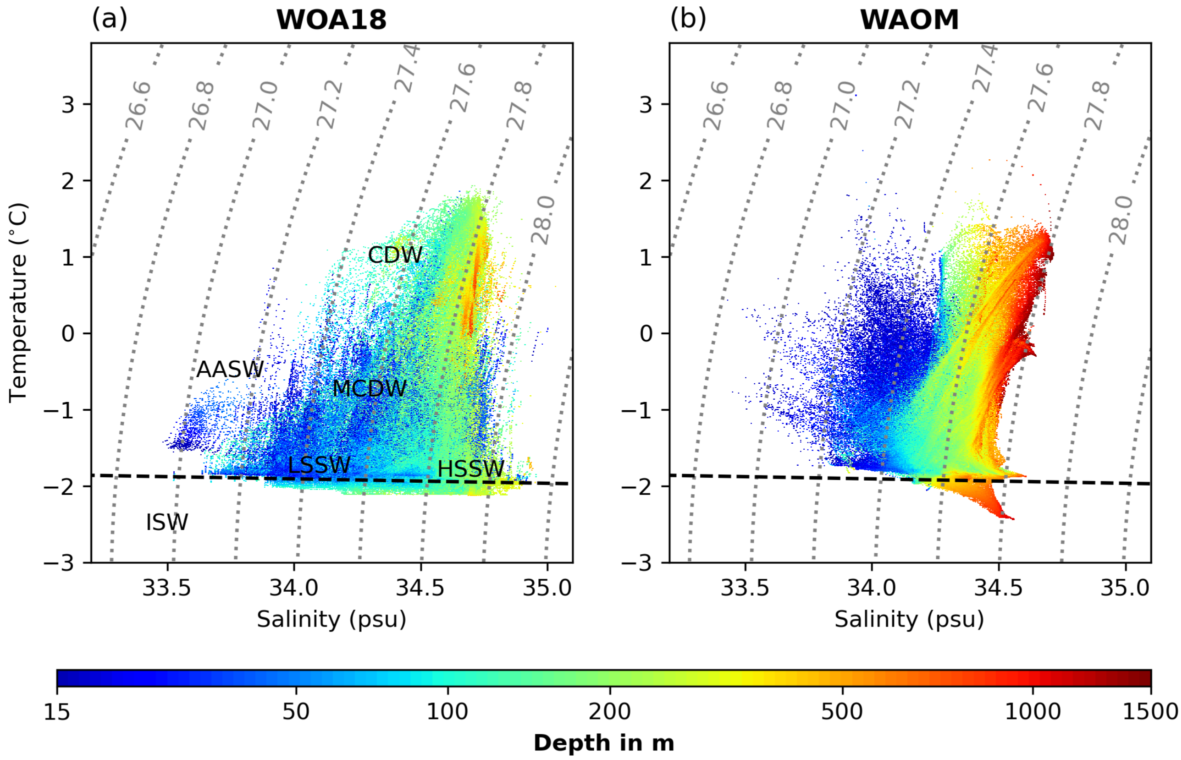


Figure 8: On-shelf summer water masses from (a) WAO18, (b) WAOM. Shown are the Potential Temperature-Salinity-Depth distributions of the continental shelf ocean (south of 1500 m isobath and excluding sub-ice shelf cavities) averaged over December, January and February. WOA18 is the seasonal climatology from 2005 to 2017, while WAOM is 2007 only. The uppermost 15 m are excluded for reasons given in the text (see Methods Section 2.6). Each product has been analysed on their original grid. For the analysis, each grid cell has been sorted into 1000x1000 temperature and salinity bins and the depth shown for each bin is the volume-weighted average of all the grid cells in this bin. The dashed black lines show the freezing point at the surface and the dotted grey lines are potential density anomaly contours (in km m −3 −1000; referenced to the surface). Labels show different water masses referred to in the text: CDW indicates Circumpolar Deep Water, MCDW indicates Modified Circumpolar Deep Water, LSSW indicates low-salinity shelf water, HSSW indicates high-salinity shelf water, AASW indicates AntarcticSurface Water, and ISW indicates Ice-Shelf Water. WAOM presents a lack of HSSW and bias towards warm waters at shallow and intermediate depths.

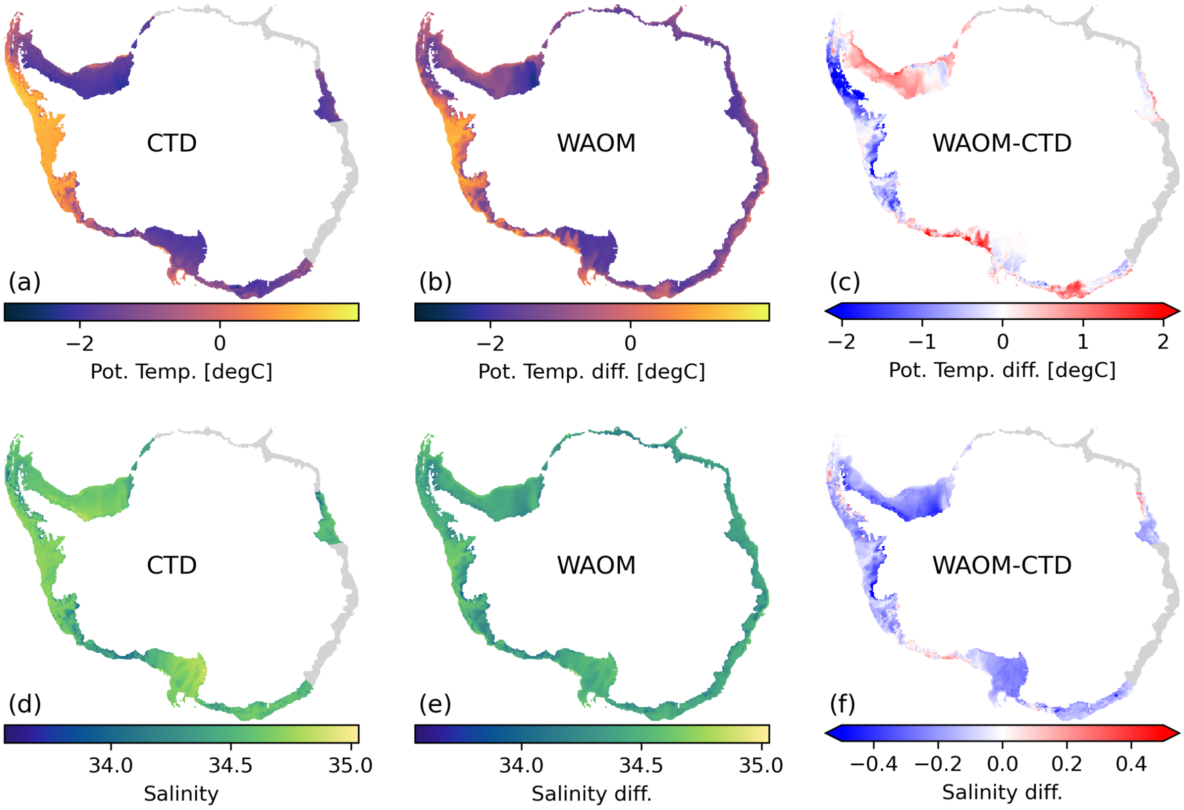


Figure 9: Spatial distribution of on-shelf bottom layer hydrography compared against observations. (a) and (d) are multi decadal mean of Potential Temperature and Practical Salinity from Schmidtko et al. (2014). (b) and (e) are 2007 mean of the same quantities as predicted by WAOM and (c) and (f) are the differences between the model and the observations (WAOM - CTD). For the analysis bottom layer CTD measurements[3] have been converted to model quantities (Conservative Temperature to Potential Temperature; Absolute Salinity to Practical Salinity) and interpolated onto the model grid using the nearest neighbour scheme. Model data has been interpolated to the same depth as the observations using the nearest neighbour scheme. Regions with sparse observations have been excluded from the analysis (Western East Antarctica and Sabrina Coast; see Fig. 1 for Sector boundaries)

[3] https://www.geomar.de/fileadmin/personal/fb1/po/sschmidtko/Antarctic_shelf_data.txt

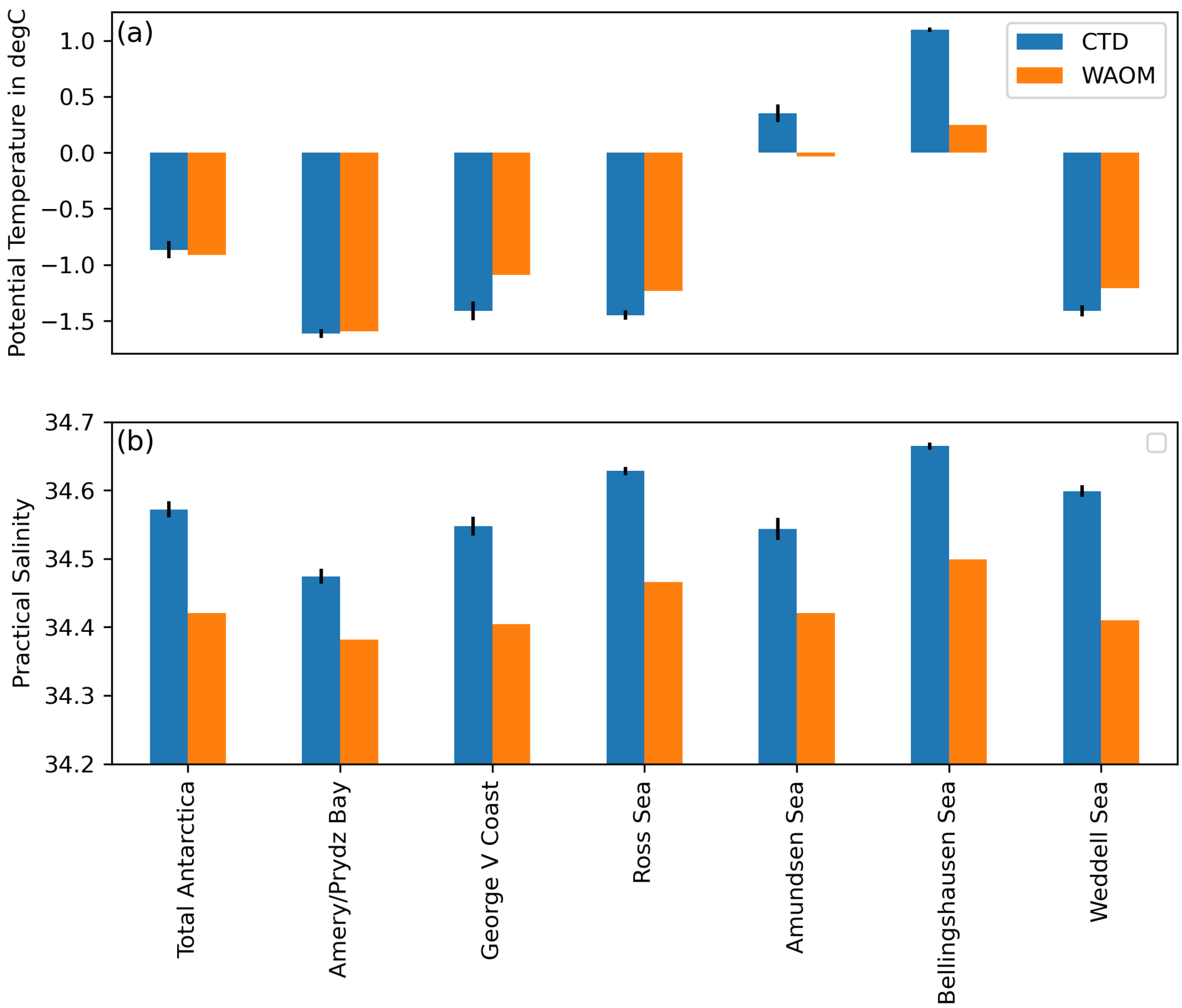


Figure 10: Sector-wise mean of on-shelf bottom layer hydrography from WAOM and observations. (a) Potential temperature and (b) Practical Salinity. As Figure 9, but area averaged over individual Antarctic sectors. CTD data also shows the sector mean of the standard deviations provided by Schmidtko et al., (2014). Regions with sparse observations have been excluded from the analysis (Western East Antarctica and Sabrina Coast; see Fig. 1 for Sector boundaries)

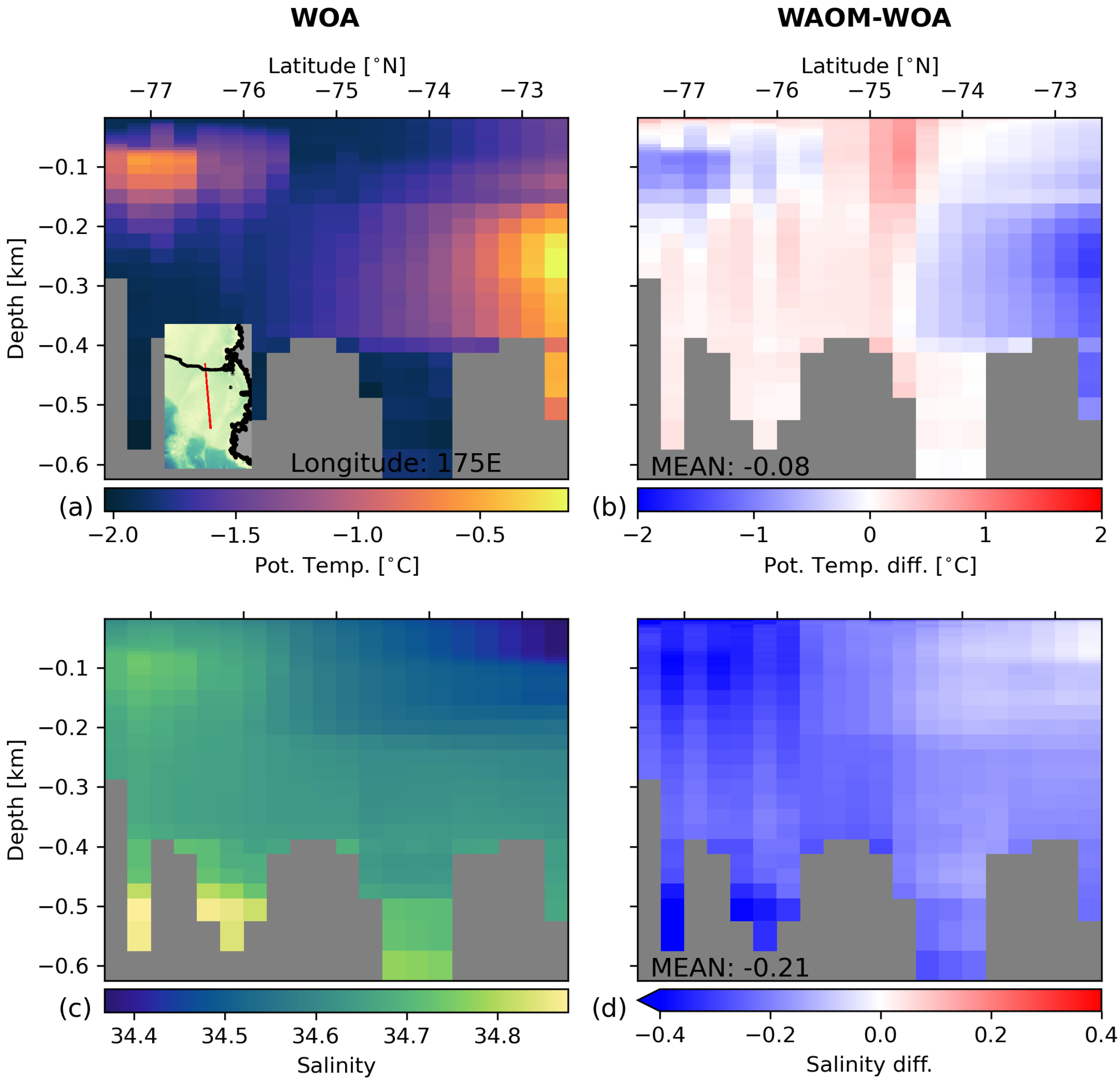


Figure 11: Temperature and Salinity transect on the Ross Sea continental shelf (175E) compared against observations. (a) and (c) are WOA18 2005-2017 summer mean temperature and salinity and (b) and (d) are the perspective differences to WAOM's 2007 summer mean (WAOM - WOA18). Prior to the comparison, WAOM's data has been interpolated to the WOA18 grid using nearest neighbours.

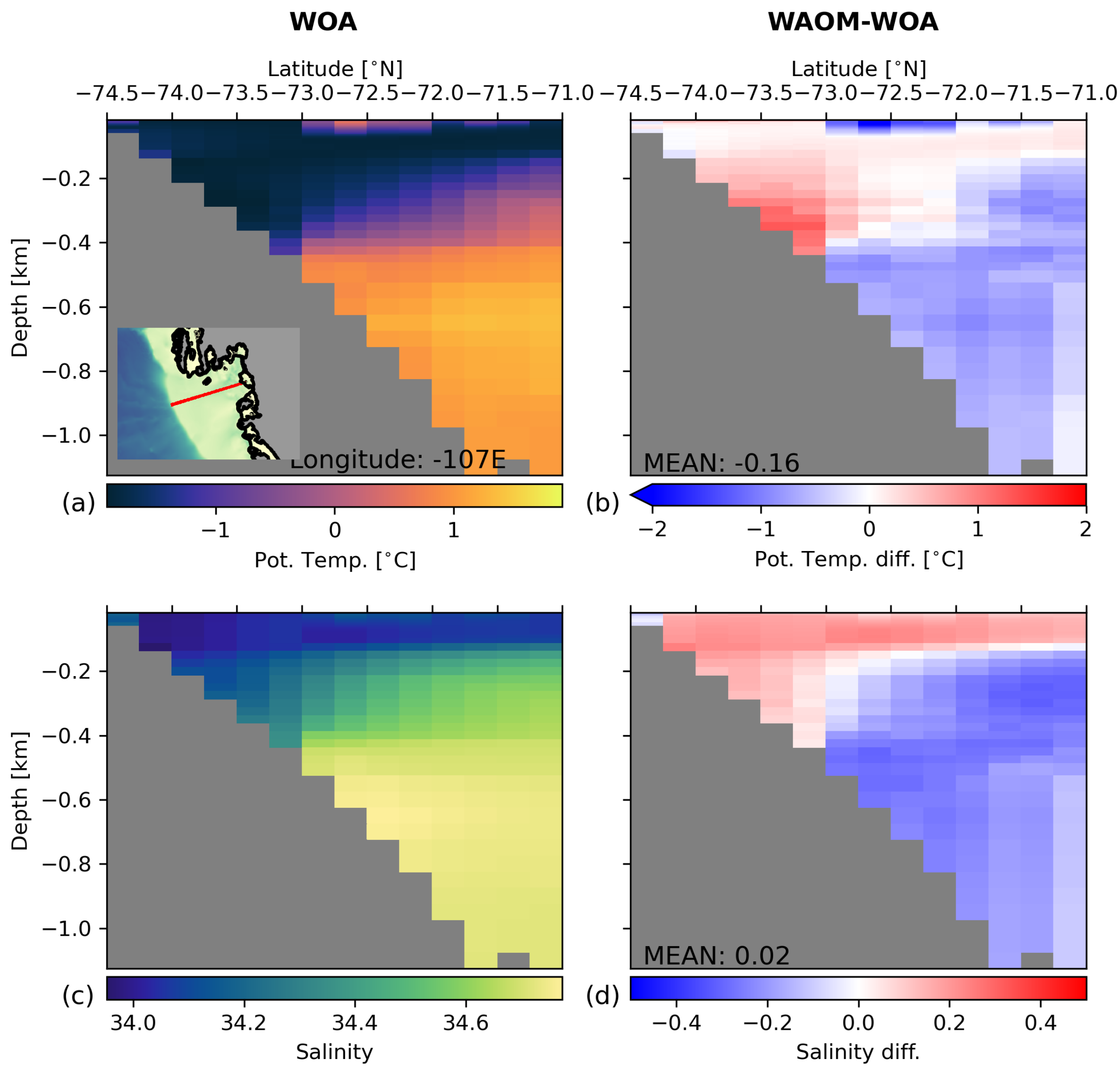


Figure 12: As Fig. 11, but for a transect across the Amundsen Seas along 107degW.

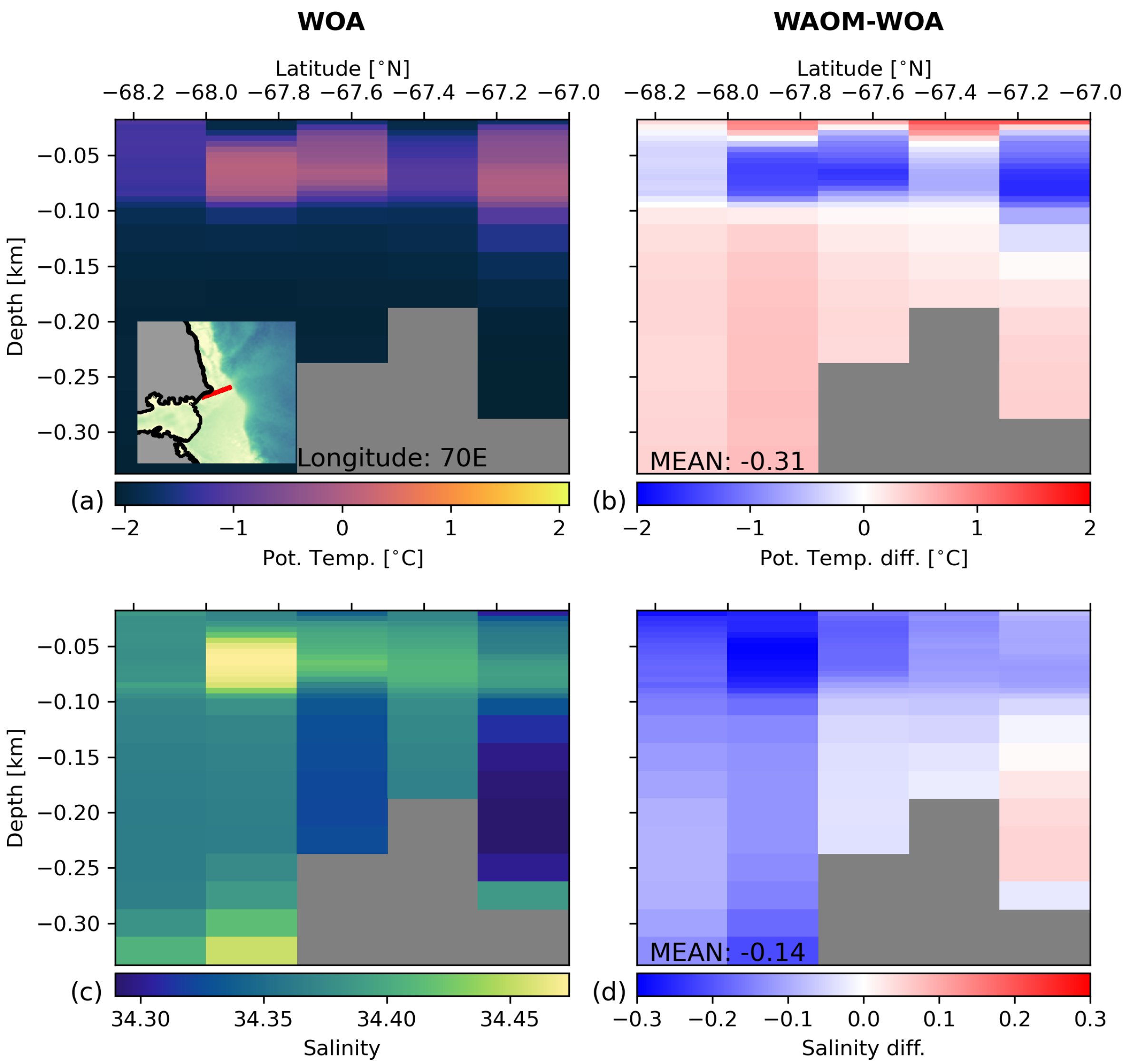


Figure 13: As Fig. 11, but for a transect in Prydz Bay (Davis Sea continental shelf) along 70degE.

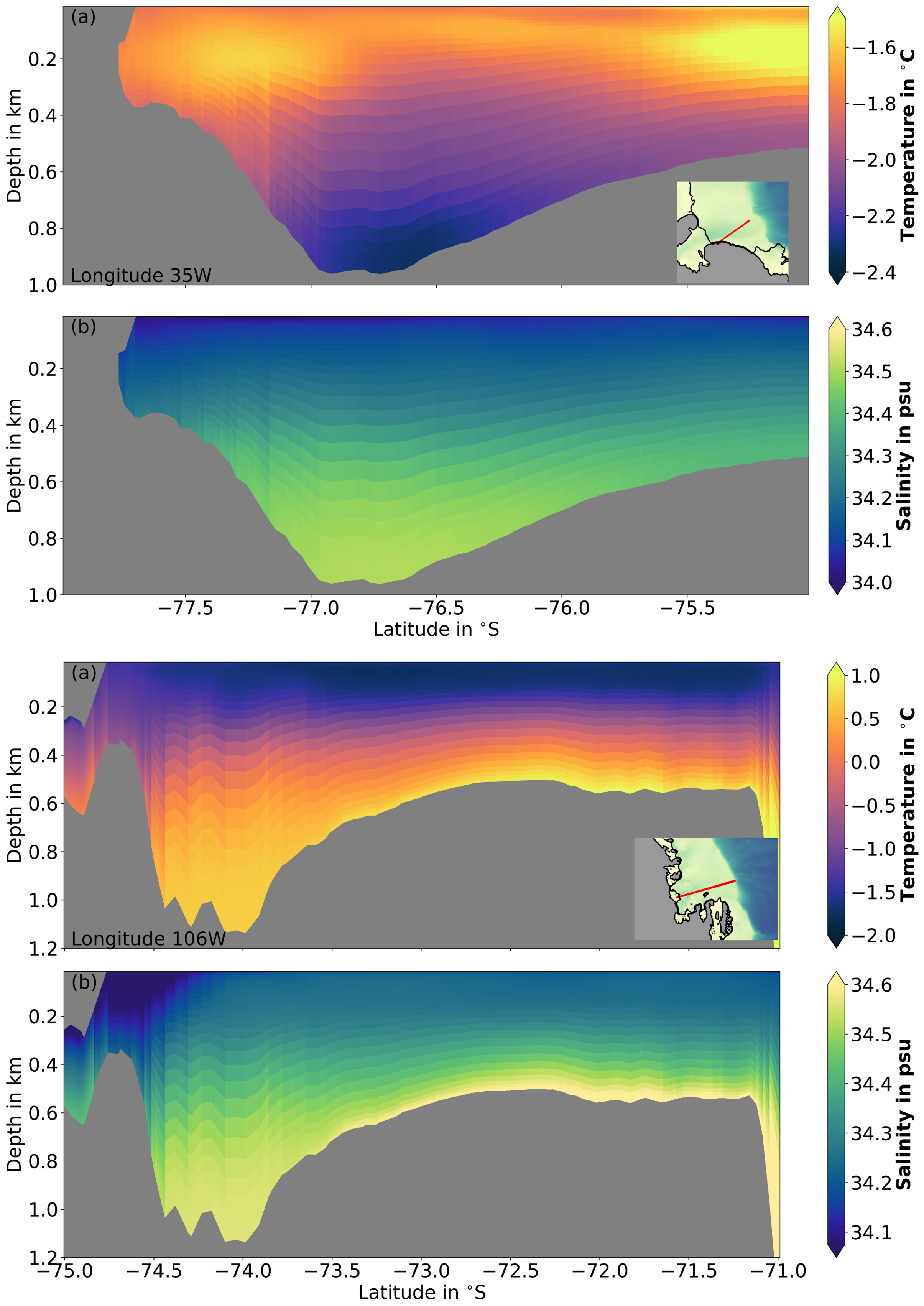


Figure 14: Temperature-Salinity transect on (a) the Weddell Sea continental shelf at 35 °W, (b) the Amundsen Sea at 106 °W, (c) the Prydz Bay at 72 °E and (d) the Sabrina Coast at 120 °E. Insets show the transect locations.

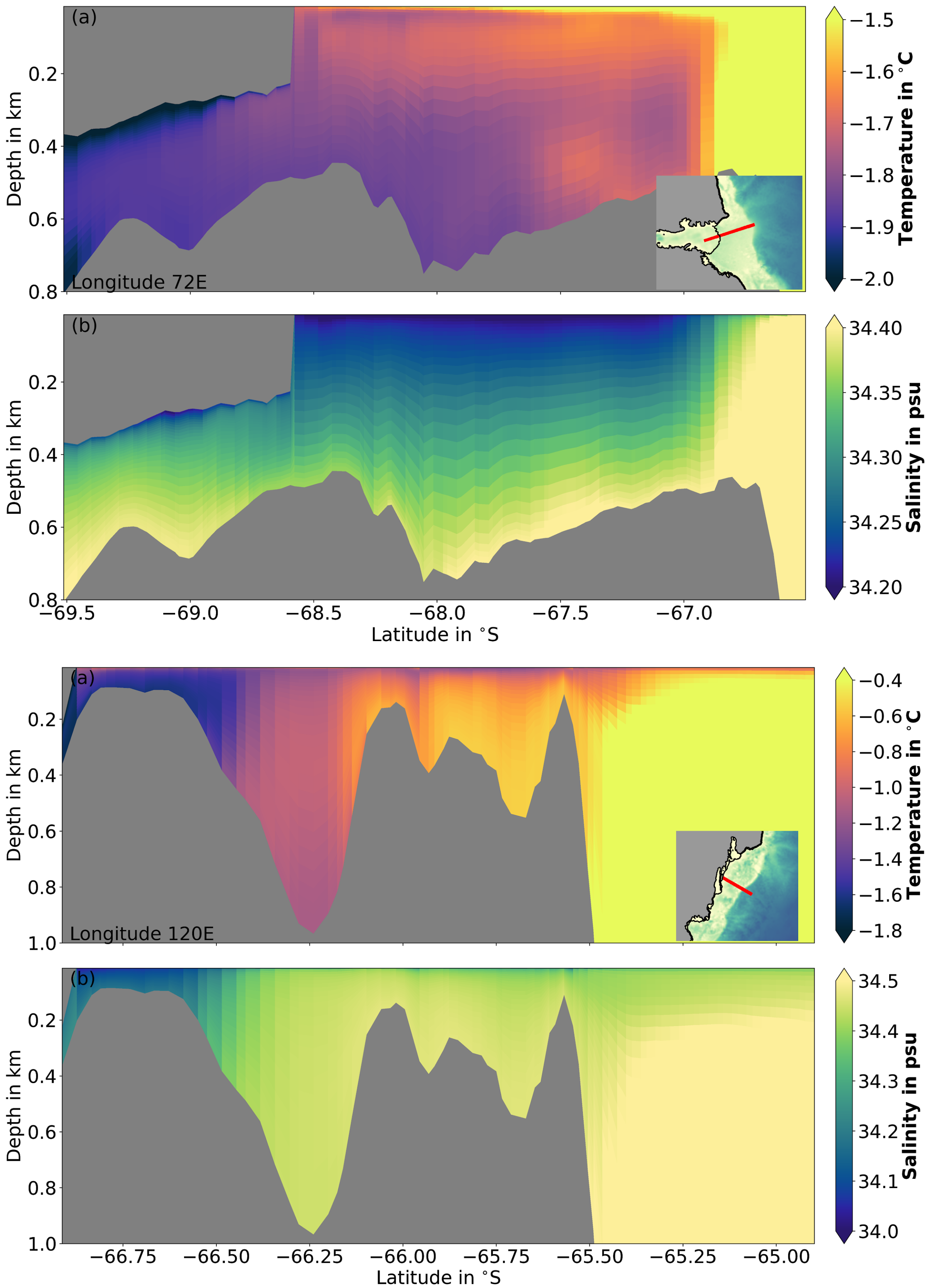


Figure 14: Temperature-Salinity transect on (a) the Weddell Sea continental shelf at 35 °W, (b) the Amundsen Sea at 106 °W, (c) the Prydz Bay at 72 °E and (d) the Sabrina Coast at 120 °E. Insets show the transect locations. (cont.)

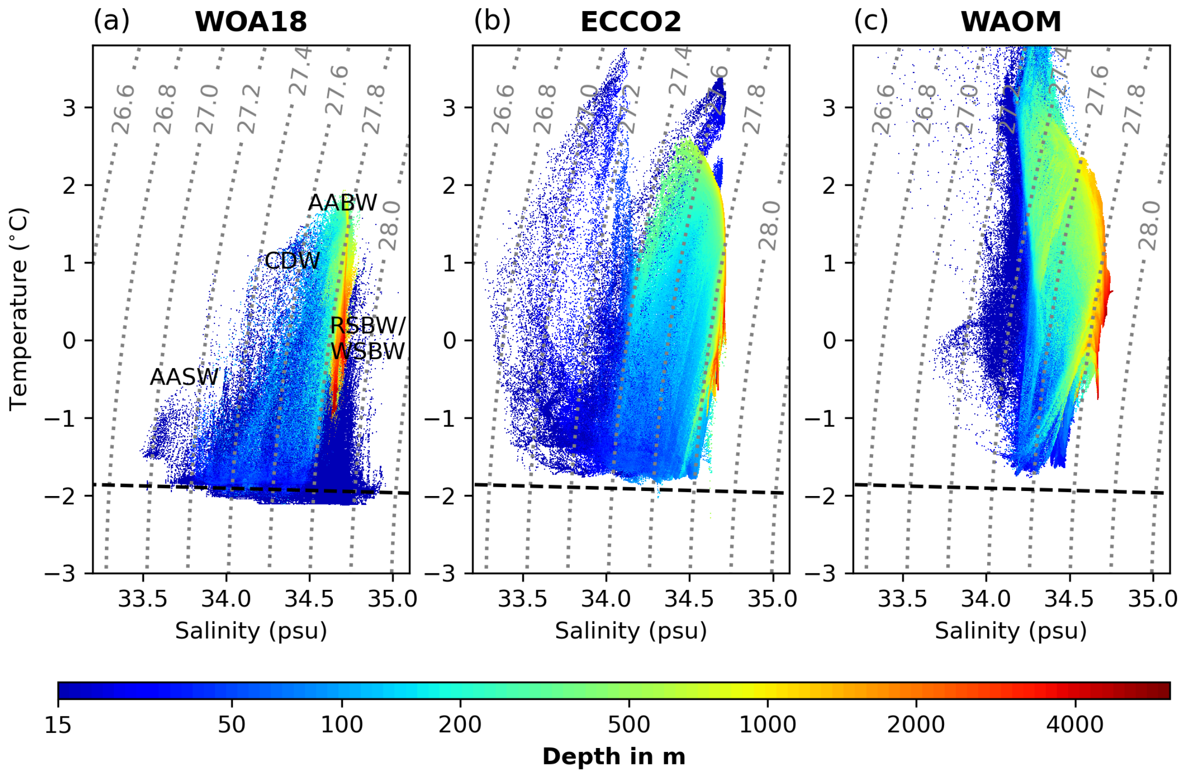


Figure 15: Off-shelf summer water masses from (a) WAO18, (b) ECCO2 and (c) WAOM. Sown are the Potential Temperature-Salinity-Depth distributions north of 1500 m isobath and south of 65°S averaged over December, January and February. WOA18 is the seasonal climatology from 2005 to 2017, while ECCO2 and WAOM is 2007 only. The uppermost 15 m are excluded for reasons given in the text. Each product has been analysed on their original grid. For the analysis, each grid cell has been sorted into 1000x1000 temperature and salinity bins and the depth shown for each bin is the volume-weighted average of all the grid cells in this bin. The dashed black lines show the freezing point at the surface and the dotted grey lines are potential density anomaly contours (in km m −3 −1000; referenced to the surface). Labels show different water masses referred to in the text: AABW indicates Antarctic Bottom Water, WSBW/RSBW indicates Weddell/Ross Sea Bottom Water, CDW indicates Circumpolar Deep Water and AASW indicates Antarctic Surface Water. WAOM has a fresh and warm bias, which originates from the surface and can not be explained by boundary or initial conditions (ECCO2).

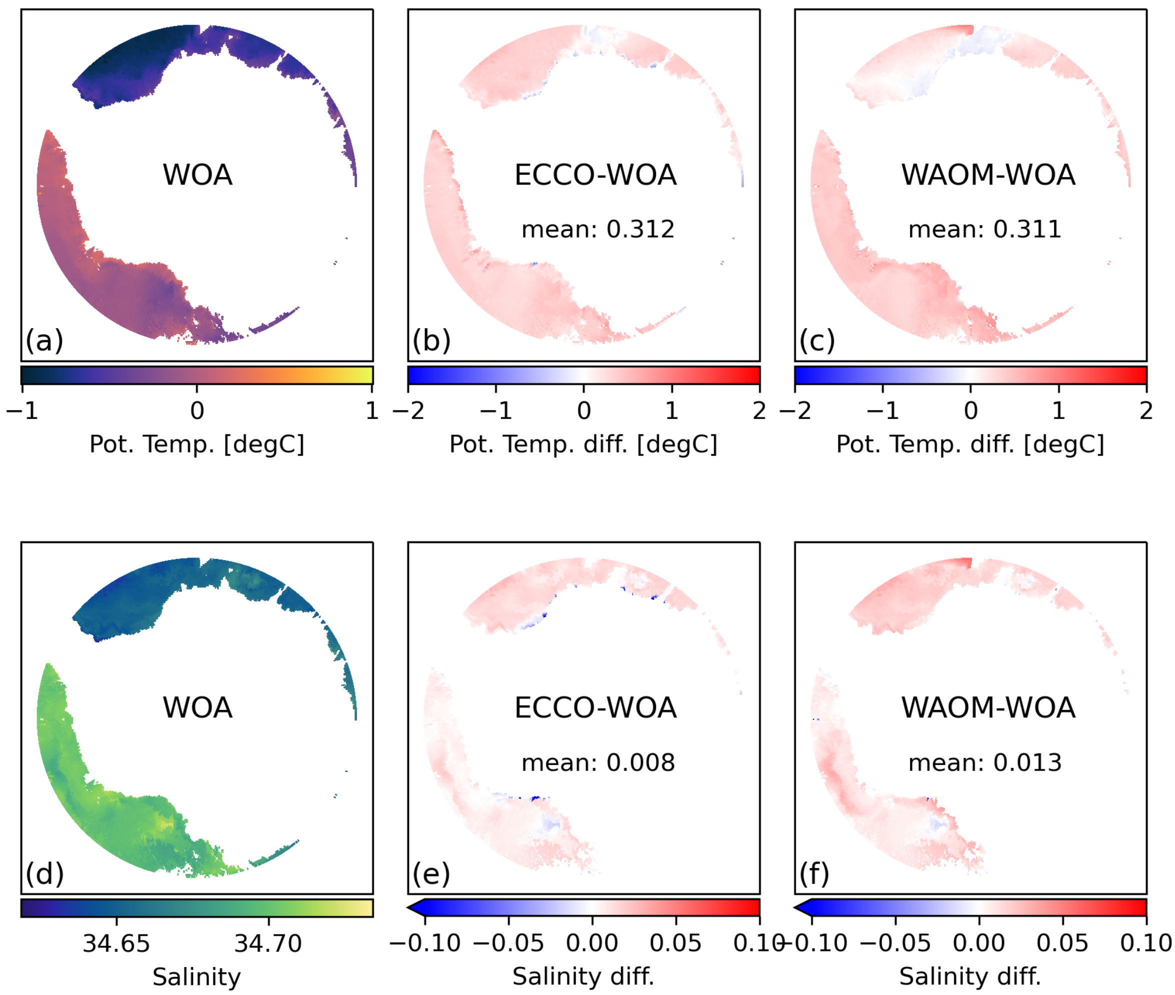
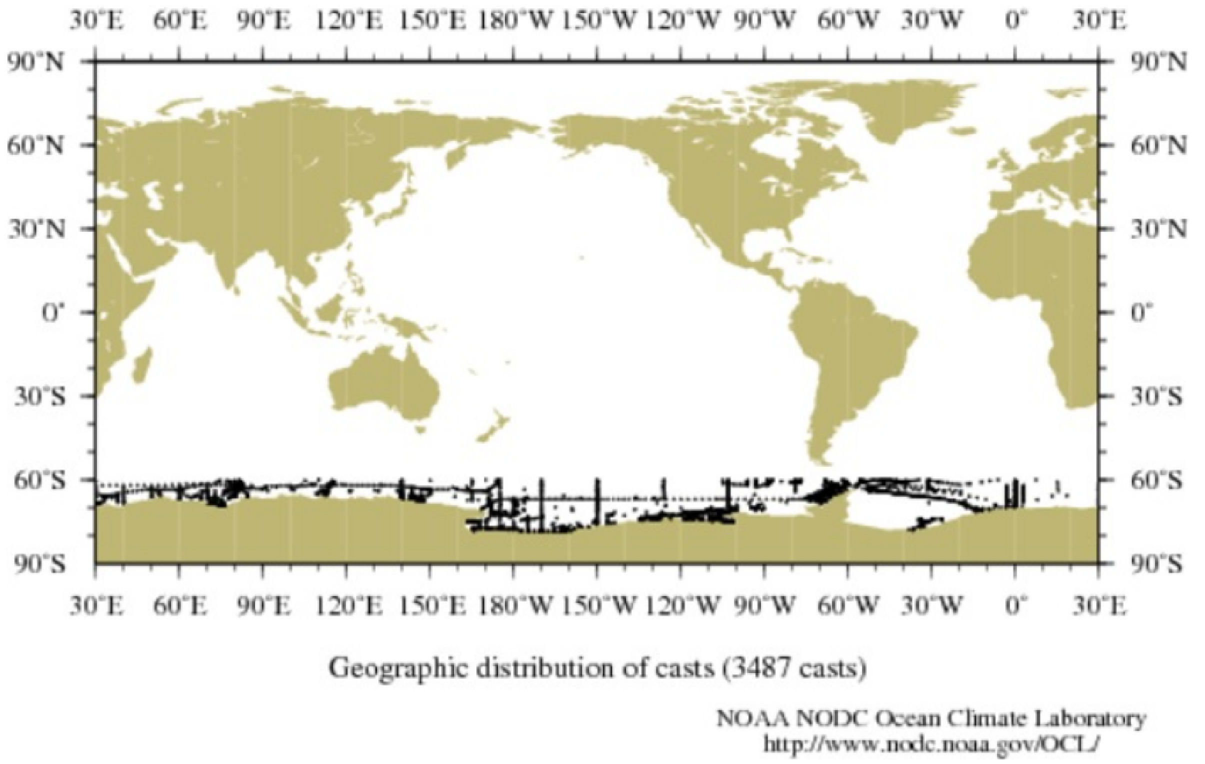


Figure 16: Mean bottom water hydrography compared against observations. (a) WOA18 2005 to 2017 climatology mean bottom layer Potential Temperature, (b) difference to ECCO2 2007 mean (ECCO2-WOA18) and (c) difference to WAOM 2007 mean. (d) to (f) are the same for salinity. WAOM and ECCO data has been interpolated to the WOA18 bottom layer using linear interpolation in the vertical and nearest neighbours in the horizontal. Only data for depths below 3000 m and south of 65°S are shown. WAOM has a salty and warm bias, which can mostly be explained by initial and boundary conditions (ECCO2).

Appendix

DATA DISTRIBUTION PLOT:



COPY OF YOUR SEARCH CRITERIA:

DEEPEST MEASUREMENTS:>	**400**
OBSERVATION DATES:	**Year** from 2005 to 2017
GEOGRAPHIC COORDINATES:	**Longitude** from -180.0000 to 180.0000; **Latitude** from -60.0000 to -80.0000
DATASET:	CTD,UOR
MEASURED VARIABLES (extract):	Temperature, Salinity

Figure D1.: Sampling distribution underlying WOA18 data. Only CTD casks that reached a depth below 400 m and measured both, Temperature and Salinity, are shown. The distribution clearly shows a high sampling density along summer ship tracks (e.g. along longitudes: 170W, 150W, 102W, 40E, 60E, 70E and 175E) and on the Amundsen Sea continental shelf. The figure has been produced using the World Ocean Database Search Query web application: https://www.nodc.noaa.gov/OC5/SELECT/dbsearch/dbsearch.html.

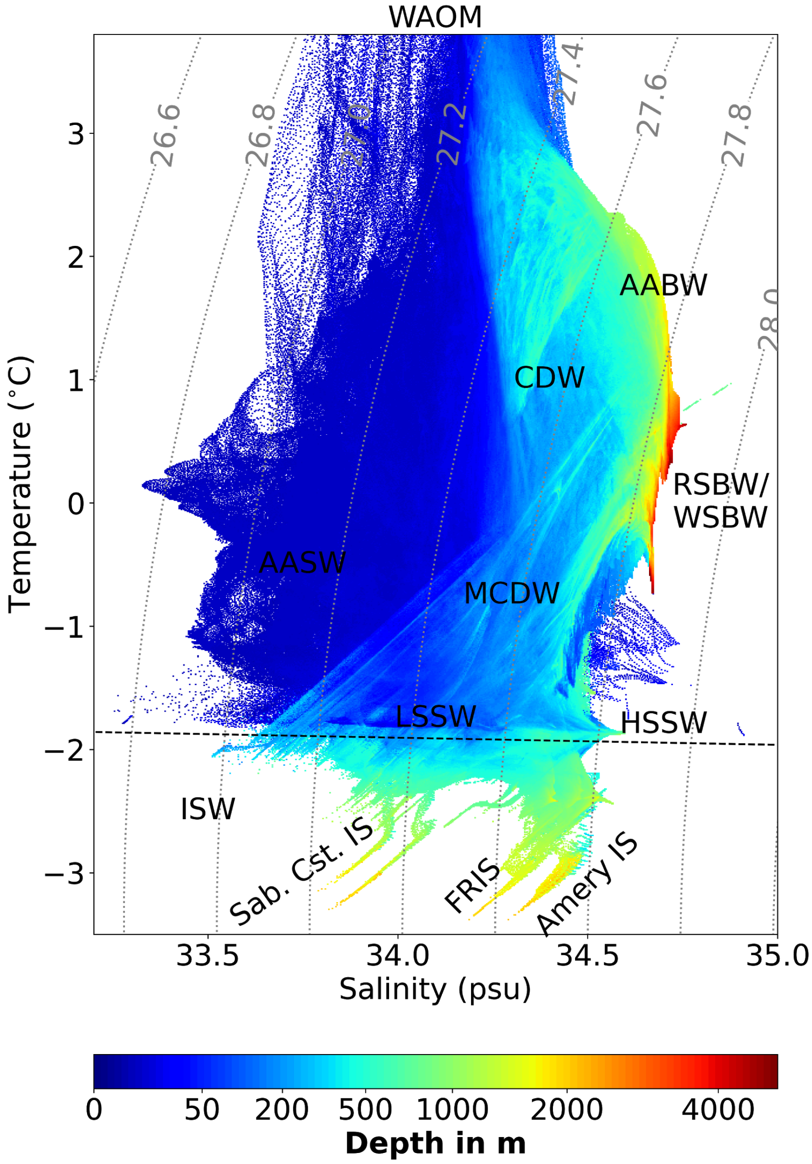


Figure D2: WAOM's water masses. Shown are the 2007 mean Potential Temperature-Salinity-Depth distributions south of 65°S (off-shelf, on-shelf and sub-ice shelf cavities). For the analysis, each grid cell has been sorted into 1000x1000 temperature and salinity bins and the depth shown for each bin is the volume-weighted average of all the grid cells in this bin. The dashed black lines show the freezing point at the surface and the dotted grey lines are potential density anomaly contours (in km m −3 −1000; referenced to the surface). Horizontal labels show different water masses: CDW indicates Circumpolar Deep Water, MCDW indicates Modified Circumpolar Deep Water, LSSW indicates low-salinity shelf water, HSSW indicates high-salinity shelf water, AASW indicates AntarcticSurface Water, ISW indicates Ice-Shelf Water, AABW indicates Antarctic Bottom Water, and WSBW/RSBW indicates Weddell/Ross Sea Bottom Water. Rotated labels show source region. Abbreviations are Ice Shelf (IS), Filchner-Ronne (FR) and Sabrina Coast (Sab. Cst.).

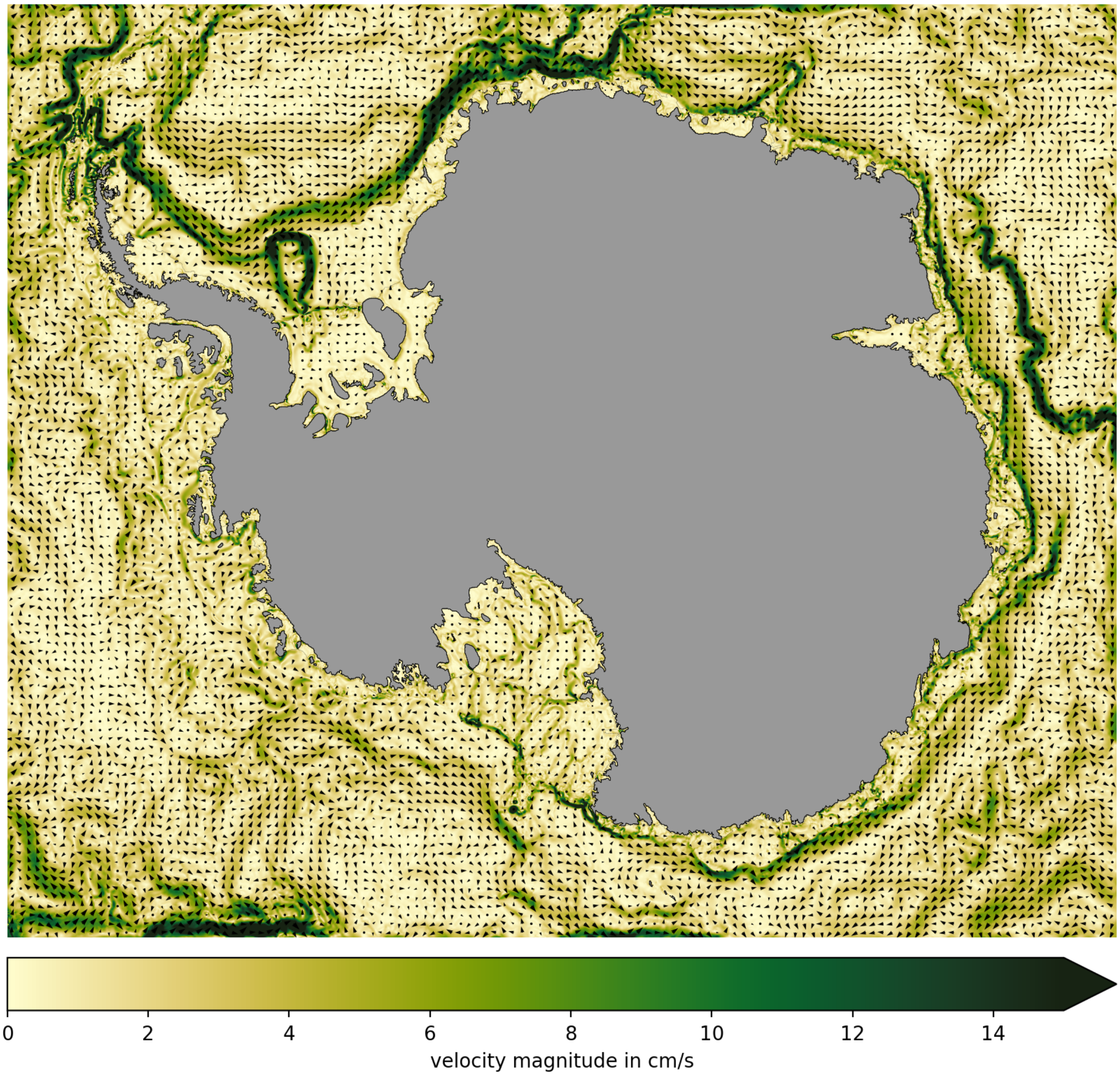


Figure D3: Mean barotropic currents in WAOM. Colors denote 2007 mean barotropic current velocity magnitude and arrows indicate direction. WAOM reproduces known features, such as the southern limb of the ACC around the Kerguelen Plateau, the southern limbs of the Ross and Weddell Sea Gyres, the slope current (e.g. around East Antarctica) and coastal currents (apparent in, e.g. Prydz Bay and in front of the Totten Ice Shelf). However, some boundary effects are apparent in the Eastern Ross Sea.

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