Response to Review Comments

We thank the editor and reviewers for their efforts in making constructive remarks and suggestions, which have significantly improved the quality of our manuscript. Below you can find point-by-point replies to the major and minor comments (*font in Italic*) and the corresponding revisions to the manuscript. In the revised manuscript, revisions are highlighted by light-blue color. We hope that all the editor's and reviewers' concerns have been addressed adequately.

Reviewer #2

This manuscript presents an Ocean–Sea Ice–Ice Shelf model for Ross Sea and Amundsen Sea (called RAISE). The ocean and sea ice components are primarily based on the ROMS configurations; a static ice shelf is added to the model, which allows inclusion of ice shelf melting. The manuscript presents most technical details for model implementation and validation with available data for key ocean physical processes relevant to production of Dense Shelf Water and Antarctic Bottom Water.

Most of the implementation seems straightforward and reasonable in most parts. The simulation spans 17 years (2003-2019) with a prior 5-year spin-up. Numerical experiments with an inert tracer (dye) are run, which show the transport and dispersion patterns of Circumpolar Deep Water (CDW) and ice shelf water (ISW). Another experiment is also run by increasing melting rates of ice shelf from Amundsen Sea ice shelves (Melt+). The results, as compared with available in situ observations or remote sensing, seem reasonable in general with one significant exception (explained below). The general functioning of the system (e.g. transport, mixing, ice production/melting) also appears to be consistent with our knowledge. The main finding is that ice melting from Amundsen Sea plays a key role in determining salinity (and hence density) of dense shelf water (DSW) in this region. It could be worth expanding this topic a bit by explaining, for example, does this affect DSW properties in southwestern Ross Sea shelf? Or does it affect Antarctic Bottom Water (ABW) properties or production?

We thank the reviewer for the overall constructive comments on this study, and by addressing these issues, we think the manuscript has been significantly improved and several ambiguities have been resolved, resulting in improved clarity. In the revised manuscript, we added plots showing the impacts of increased meltwater from the Amundsen Sea on the salinity or neutral density over the southwestern Ross Sea shelf (Fig. 10, Fig. 12 and Fig. 14), and please see our detailed response to the reviewer's specific comment relevant to this point below. In the original manuscript, we showed that in the experiment with increased meltwater (Melt+), the AABW thicknesses in the open ocean near the Ross Sea can be reduced by over 100 m (Fig. 14b).

Based on one transect, the model has under-predicted the salinity in the top 150 m by 0.1-0.2 psu (model temperature is slightly, ~1°C, higher than observed) (Figure 6). In contrast, modeled subsurface salinity is higher than observed (by ~0.05 psu). This seems consistent with lower ice concentration than observed (Figure 3). The logic seems to be: Warmer temperature in surface layer leading to less sea ice formation, which in turn leads to insufficient sea brine formation and under-prediction of DSW production. However, this seems contradicting to the sea ice production comparison (Figure 4), which shows model over-predicts the sea ice production. The scatter plot between modeled vs WOD salinity for shelf water also shows a <1 slope, i.e. surface salinity is higher than WOD salinity and vice versa for subsurface water (red dots in Figure 7d)? Perhaps there are spatial mismatch (the transect is on western Ross Sea) or perhaps ice thickness is an issue? In addition, the Melt+ experiment with enhanced ice shelf melting significantly reduces DSW density (salinity), bringing the density much closer to observed. Does this mean now we are getting a smaller slope if we plot a new scatter plot? Regardless, it seems some more clarification is needed to reconcile these.

The reviewer made a valuable point for explaining the overestimate of sea ice production in the model, while sea ice concentration is underestimated likely associated with the overestimate of sea surface temperature. We then examined the modelled ice thickness against observations from a cruise conducted by the PIPERS (Polynyas, Ice Production and Seasonal Evolution in the Ross Sea) project, which lasted from April to June of 2017 (Ackley et al., 2020). The comparison results (Fig. R7) show that our model does overestimate the sea ice thicknesses in the polynyas and over the Ross Sea shelf, which can contribute to the overestimate of sea ice production. We mentioned this in Lines 259–261 of the revised manuscript, and provided Fig. S1 in the Supplementary Information. Note that the in-situ observations of sea ice are partly from AUV observations and partly from visual ice observations. So there could also be inaccuracies in these data.

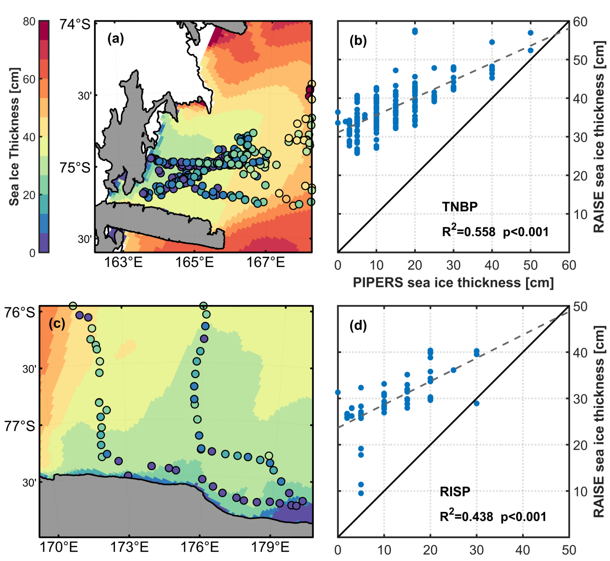

Fig. R7. (a and c) Sea ice thicknesses from the RAISE model (color shading) and from observations (colored dots) during the PIPERS cruise observations in the (a) Terra Nova Bay and (c) Ross Ice Shelf polynya areas. (b and d) The scatter plot of modelled ice thickness versus observed ice thickness in the cruise in the (b) Terra Nova Bay and (c) Ross Ice Shelf polynya areas. The black solid line indicates the 1:1 ratio line, and the grey dashed line indicates the linear regression fit.

Please note that Fig. 7 is plotted for potential temperature and salinity in the bottom 100-m layer (see the figure caption), so all the points are in the bottom layer. It is true that from Fig. 7d, a slope <1 means the model overestimates salinity in the lower-salinity range and underestimates salinity in the higher-salinity range (high salinities exist mostly over the western Ross Sea shelf). This seems contradictory to the fact that our model overestimates sea ice production, salinity and neutral density in the polynya regions located on the western Ross Sea shelf. In fact, such overestimation can also be seen from Fig. 7c in the Terra Nova Bay polynya and the Ross Ice Shelf polynya, and the reviewer is correct that there are spatial variations in the overestimation or underestimation of salinity over the shelf, even just over the western shelf. We added two sentences to discuss this in the revised manuscript (Lines 318–321). As the reviewer suggested, we made the same plot as Fig. 7 for the Melt+ experiment (Fig. R8 shown below), in fact the slope is slightly increased (from 0.693 to 0.726), and such weak increase might be due to the fact that the meltwater from the Amundsen Sea is mainly carried by slope currents and thus causes larger reduction in salinity in the slope regions (i.e. the lower-salinity range in the scatter plot).

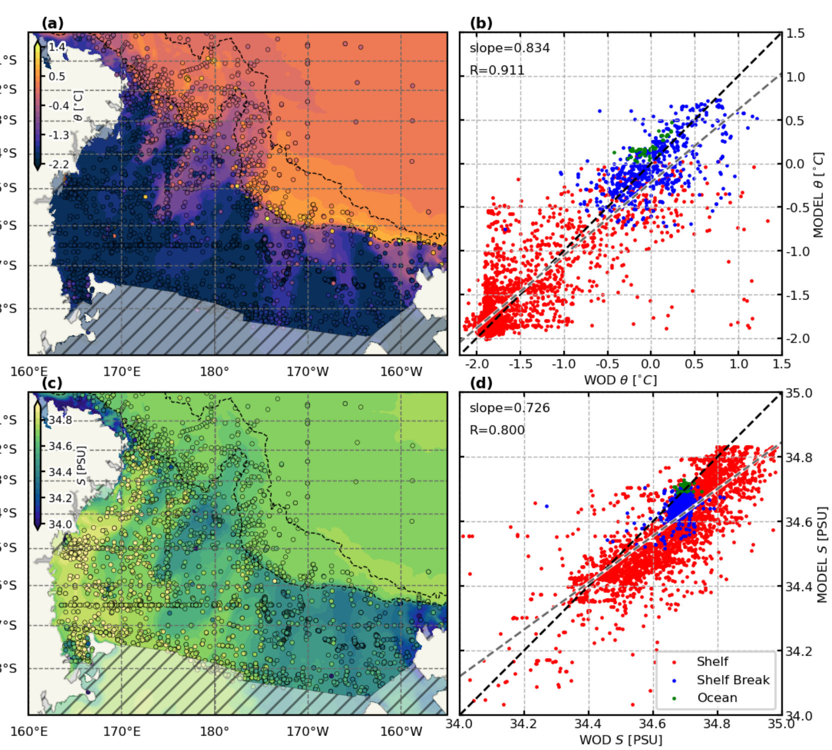

Fig. R8. Same as Fig. 7 in the manuscript but for the Melt+ experiment.

The manuscript can also benefit from some polishing and clarifying a few details. For example, it is unclear, however, how exactly the melting rate under the ice shelf is calculated and applied. It is not clear how Melt+ experiment is being implemented other than that heat/salt transfer coefficients are modulated as done by Nakayama et al. (2020).

The three-equation parameterization scheme for ice shelf simplifies the thermodynamic processes beneath ice shelves by representing the freezing and melting of sea ice. In the heat conservation equation at the ice shelf/ocean interface, based on thermodynamic equilibrium, the latent heat sink (or source) generated by melting (or freezing) balances the heat loss to the ice and the heat supplied by seawater:

$$Q_I^T - Q_W^T = -\rho_I w_B L_f \qquad (1)$$

Here, Q_I^T and Q_W^T represent the heat fluxes at the ice and seawater interfaces (W m^{-2}, positive values indicating upward flux), ρ_I is the ice density (kg m^{-3}), w_B denotes the rate of ice melting (> 0) or freezing (< 0) (m s^{-1}), and L_f is the latent heat of fusion of ice (J kg^{-1}). The heat flux from the water is generally much greater than that through the ice, and thus the model assumes the ice is perfectly insulating (i.e., Q_I^T = 0). Typically, the heat flux from seawater to the ice shelf/ocean interface (Q_W^T) is represented using a bulk turbulent transfer formulation:

$$Q_W^T = -\rho_W C_{pw} \gamma_T (T_B - T_W) \qquad (2)$$

Here, ρ_W is the seawater density (kg m^{-3}), C_{pw} is the specific heat capacity of seawater (J kg^{-1}℃$^{-1}$), T_B is the interface temperature (freezing point), and T_W is the temperature of seawater at a certain distance from the ice shelf/ocean interface. In our model, T_W is defined as the temperature of the uppermost grid cell, a common approach in other studies (Galton-Fenzi et al., 2012; Dansereau et al., 2014). γ_T is the heat transfer coefficient (m s^{-1}), representing the molecular and turbulent mixing coefficient of heat within the ocean boundary layer adjacent to the ice shelf. Some studies assign a constant value for γ_T; however, a more commonly parameterization (Timmermann et al., 2012; Holland et al., 2008) is adopted in our model, where γ_T is parameterized as a function of the friction velocity (Jenkins et al., 2010). In the model, friction velocity is defined similarly to the frictional drag between the ocean and seabed. The second equation is the salt flux conservation equation. At the ice shelf/ocean interface, the freshwater flux generated by melting or freezing ice with salinity S_I balances the salt flux arriving at the interface from seawater (with the salt flux through the ice shelf, Q_I^S, assumed to be zero):

$$-Q_w^S = \rho_I w_B (S_I - S_B) \qquad (3)$$

Here, Q_w^S represents the salt flux at the seawater interface (psu-kg m^{-2} S^{-1}), S_I is the ice salinity, and S_B is the salinity at the interface. Sea ice formed from freezing seawater contains some brine, but observations indicate that the salinity content is very low (0.10 or lower), and thus S_I is set to zero in the model. The salt flux from seawater to the ice shelf/ocean interface, Q_w^S, is typically represented in a turbulent diffusive flux form similar to that of heat:

$$Q_w^S = -\rho_W \gamma_S (S_B - S_W) \qquad (4)$$

Here, γ_S denotes the salt transfer coefficient (m s^{-1}), and S_W represents the salinity at a certain distance from the ice shelf/ocean interface, which in the model is defined as the salinity of the uppermost grid cell. Due to the differing molecular diffusivities of heat and salt, the values of γ_S and γ_T differ. However, γ_S is also parameterized in our model as a function of the friction velocity. The third equation describes the freezing point of seawater as a weakly nonlinear function of salinity and pressure. By linearizing this relationship, an analytical solution for the coupled system of three equations can be obtained:

$$T_B = aS_B + bP_B + c, \qquad (5)$$

Here, we set the salinity coefficient $a = -5.7 \times 10^{-2}$ °C, the pressure coefficient $b = -7.61 \times 10^{-4}$ °C dbar^{-1}, and in the actual solution, the depth of the ice shelf base is used as a substitute for pressure. The coefficient $c = -9.39 \times 10^{-2}$ °C. By simultaneously solving Equations (1) and (2), as well as Equations (3) and (4), we can obtain the following results:

$$\frac{\rho_I w_B L_f}{c_{pw}} = -\rho_W \gamma_T (T_B - T_W) \qquad (6)$$

$$\rho_I w_B S_B = \rho_W \gamma_S (S_B - S_W) \qquad (7)$$

By combining Equations (5), (6), and (7), we can solve for S_B, T_B, and the ice shelf melting rate (w_B). In the revised manuscript, we provided a brief description of the parameterization scheme for simulating ice shelf melting in Section 2.3 as described above (Lines 183–204).

References

Galton-Fenzi, B. K., Hunter, J. R., Coleman, R., Marsland, S. J., and Warner, R. C.: Modeling the basal melting and marine ice accretion of the Amery Ice Shelf, J. Geophys. Res.-Oceans, 117, C9, https://doi.org/10.1029/2012JC008214, 2012.

Dansereau, V., Heimbach, P., and Losch, M.: Simulation of subice shelf melt rates in a general circulation model: Velocity - dependent transfer and the role of friction, J. Geophys. Res.-Oceans, 119, 1765-1790, https://doi.org/10.1002/2013JC008846, 2014.

Timmermann, R., Wang, Q., and Hellmer, H. H.: Ice-shelf basal melting in a global finite-element sea-ice/ice-shelf/ocean model, Annals of Glaciology, 53, 60, 303-314, https://doi.org/10.3189/2012AoG60A156, 2012.

Holland, P. R.: A model of tidally dominated ocean processes near ice shelf grounding lines. J. Geophys. Res.-Oceans, 113, C11, https://doi.org/10.1029/2007JC004576, 2008.

Jenkins, A., Nicholls, K. W., and Corr, H. F.: Observation and parameterization of ablation at the base of Ronne Ice Shelf, Antarctica. J. Phys. Oceanogr., 40, 2298-2312, https://doi.org/10.1175/2010JPO4317.1, 2010.

Figure 10 (TNB mooring) & Figure 12 (Ross Island CTD) suggest modeled salinity was also higher than observed on the western Ross shelf and even areas close to Ross Sea Ice Shelf. Does Melt+ improve salinity simulation in those area too?

In the revised Fig. 10 and Fig. 12, we added the simulated neutral density or salinity from the Melt+ experiment, and the results show that Melt+ also improved the simulations of density or salinity in these areas. Relevant discussions are added in Lines 470–473 of the revised manuscript.

Based on dye experiment, it takes 5 years for those dyes released from Amundsen Sea to reach western Ross Sea, suggesting a delay of 5-10 year in the impacts. This may help to explain why salinity improvements at CA1 and CA2 in Melt+ only takes place after mid-2008? Figure 11 did not show salinity changes before 2008, so it is clear if this is truly the case.

In Fig. 11, we did not show the neutral density (I guess the reviewer mean this variable instead of salinity) changes before 2008, because the mooring data are only available from 2008 to 2016. In fact, if we look carefully, salinity had already been notably decreased in Melt+ in early 2008 (January to March) compared to CTRL, while in mid 2008 it became close to the value in CTRL again. Another issue that we need to clarify is, in Fig. 11 we just showed the distributions of ISW dyes from the Amundsen Sea ice shelves 5 years after their release time (as mentioned in the figure caption), and 5 years is not transport time for the ISW to be carried from the Amundsen Sea to the Ross Sea. In fact, the transport time is shorter, which is approximately 2 years. We calculated the difference in salinity from Melt+ and CTRL at bottom depths of CA1 and CA2 from 2003 (the start time of Melt+) to 2013 (Fig. R9). We can see that from 2003 to 2005, the salinity reduction in Melt+ compared to CTRL is not significant, confirming the fact it takes about 2 years for increased meltwater in the Amundsen Sea to influence salinity in the Ross Sea. We added such information in Lines 345–346 in the revised manuscript.

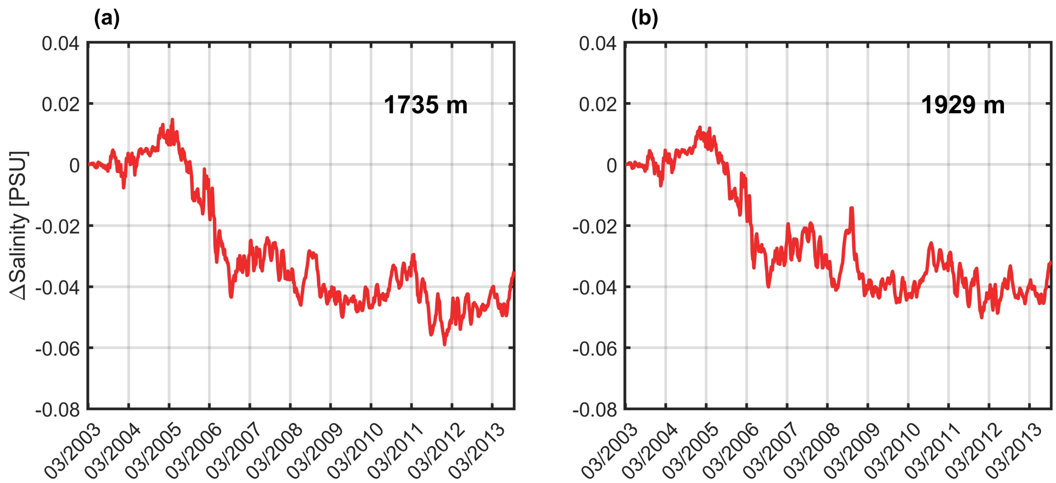

Fig. R9. Time series of differences in salinity from the Melt+ and CTRL simulations (Melt+ minus CTRL) during 2003–2013 at (a) 1735 m at the CA1 location and (b) 1929 m at the CA2 location.

It seems higher model SIP than observed (Figure 4) is contradictory to the lower ice concentration than observed (Figure 3). But this may also mean the model over-estimates the ice thickness. So perhaps some clarifications about how model performs on sea ice thickness will be helpful.

Please see our response to the review's earlier comments about this issue, and we provided ice thickness validation results in the Supplementary Information.

Some editorial comments/suggestions

Page 2, line 31-34, Is this true there were no modeling studies examining this issue?

Up until now we have not noticed studies that evaluated modelled DSW variations in the Southern Ocean on different timescales (interannual and long-term variability as in this study) as well as their driving forcings. But in case that we may have missed any studies, we revised this sentence to "This study represents an attempt to thoroughly evaluate the DSW properties and associated ocean-sea ice-ice shelf coupling processes among modelling studies in the Southern Ocean, using …" (Lines 31–33).

Page 2, line 35, suggest changing "DSW" -> "DSW properties"?

Revised accordingly (Line 35).

Page 2, line 36, "which are not seen in DSW studies before". It is unclear what have not been seen, temporal variations or observations?

The original sentence is "In particular, the modelled temporal variations of DSW properties in polynyas and its key export passages are compared with long-term mooring observations, which are not seen in DSW studies before". We changed it to "In particular, the modelled temporal variations of DSW properties in polynyas and its key export passages are compared with long-term mooring observations, which are rarely seen in studies of the DSW temporal variability before" (Lines 35–37).

Page 3, line 41, "under-estimate" -> "an under-estimate"

Revised accordingly (Line 41).

Page 5, figure 1 lower panel. It is worth noting the model grid is not conforming to orthogonality in those two southern corners. This may be intentional and shall not affect model results since they are land points.

Our intention is that as far as the model can cover the entire Ross Ice Shelf, the smaller model domain (and thus less grid points) the better. This does produce some model grid points at the southwestern and southeastern corners not conforming to orthogonality very well. But as the reviewer mentioned, there are land points and shall not affect the model results.

Page 6, entire paragraph, so some aspects of the interactions between ice shelf melting and DSW may have been examined.

It is true that these studies discussed some aspects of the interactions between ice shelf melting and DSW, which are also mentioned in this paragraph. But as pointed out in this paragraph, the models used in these studies just cover the Ross Sea, and the effects of ice shelf meltwater from the Amundsen Sea are artificially simulated by changing salinity at the open boundaries of the models. Such operations would to some extent help reveal the effects of meltwater on the Ross Sea water properties, but cannot provide an accurate estimate of such effects.

Page 7, line 117, "finite-volume" should be "finite-difference"

Thanks for raising this critical, yet long debated topic. ROMS has been described as a finite-difference model in many literatures over the years, which is correct since the primitive equations are discretized with the finite-difference method. However, a commentary article by Shchepetkin and McWilliams (2009), who are early developers of ROMS, pointed out that ROMS is technically a finite-volume model since the tracer equations are discretized in volume integrated form. Therefore, unlike other finite-difference models, tracer conservation is always guaranteed. The same argument is brought up by Shchepetkin in this post on ROMS forum (https://www.myroms.org/forum/viewtopic.php?t=106), which states that "If one wants to guarantee things like integral conservation of something, and the grid is curvilinear, then "finite volume" is basically the only way to go." He argued that for structured grid models, the key of 'finite volume' is conservation of volume and tracers, and since ROMS's dynamic kernel guarantees conservation, it should be classified as a 'finite volume model'. In this manuscript, we adopted Shchepetkin and McWilliams (2009)'s view, and thus refers ROMS as a finite-volume model. We are aware of the disputes in this community but decide to leave the statement in this manuscript to maintain the integrity of the description.

Reference

Shchepetkin, A., and McWilliams, J.: Correction and commentary for "ocean forecasting in terrain-following coordinate: Formulation and skill assessment of the regional ocean modeling system" by Haidvogel et al., 3595–3634, Journal of Computational Physics, 228, 8985–9000, 2009.

Page, 8, line 133, maybe spell out "EN4" and add either a link or a reference?

The reference for EN4 is provided in the revised version (Line 142). In all references and webpages describing EN4, only the abbreviation is used and we were not able to find a full name for it. So we just used EN4 here.

Page 8, line 135, "Below 1000 m (the isobath at the shelfbreak)". Is 500m a better delineator for shelf-break. 1000 m may be more like upper slope.

The 1000-m isobath is widely used as a delineator for the Antarctic shelf break in previous studies, including studies for the Ross Sea (e.g., Moorman et al. 2020; Morrison et al. 2020; Silvano et al. 2020). Following Goddard et al. (2017), the 1000-m isobath closely tracks the Antarctic Slope Current (ASC) and the Antarctic Slope Front (ASF) (Huneke et al. 2022), making it suitable for being used as a boundary between the continental shelf and the open ocean. In addition, its advantages lie in providing a consistent boundary for circumpolar analysis, accounting for the variable bathymetry along the Antarctic continental margin.

References

Huneke, W. G. C., Morrison, A. K., and Hogg, A. M.: Spatial and Subannual Variability of the Antarctic Slope Current in an Eddying Ocean–Sea Ice Model, Journal of Physical Oceanography, 52, 347-361, https://doi.org/10.1175/JPO-D-21-0143.1, 2022.

Moorman, R., Morrison, A. K., and McC. Hogg, A.: Thermal Responses to Antarctic Ice Shelf Melt in an Eddy-Rich Global Ocean–Sea Ice Model, Journal of Climate, 33, 6599–6620, 10.1175/JCLI-D-19-0846.1, 2020.

Morrison, A., Hogg, A., England, M., and Spence, P.: Warm Circumpolar Deep Water transport toward Antarctica driven by local dense water export in canyons, Science Advances, 6, eaav2516, 10.1126/sciadv.aav2516, 2020.

Silvano, A., Foppert, A., Rintoul, S. R., Holland, P. R., Tamura, T., Kimura, N., Castagno, P., Falco, P., Budillon, G., Haumann, F. A., Naveira Garabato, A. C., and Macdonald, A. M.: Recent recovery of Antarctic Bottom Water formation in the Ross Sea driven by climate anomalies, Nature Geoscience, 13, 780-786, 10.1038/s41561-020-00655-3, 2020.

Page 10, line 153, so does the release only take place once, or is it released continuously? If the latter, for how long?

The dyes are released continuously released during the simulation periods of the experiments. This is clarified in the revised version (Lines 158–159).

Page 10-11, section 2.3, how is ice shelf melting simulated? Since this is one of most critical factors in this study, perhaps spell out some details how this is being implemented?

Please see our response to the Reviewer's comment above.

Page 11, line 179, Melt+, increasing ice melt rate by how much?

The value is actually provided in Section 4.5 (~450 Gt yr^{-1}) when we describe the experiment in more details, so we added "(see the details in Section 4.5)" here (Line 204).

Page 11, line 187, add WOD behind "World Ocean Database"

"WOD" is added (Line 211).

Page 12, line 197. Add period behind 2016.

We apologize for this typo, and a period is added behind 2016.

Page 12, line 205, define SIC here

"SIC" is replaced with sea ice concentration here (Line 229).

Page 13, line 216, remove "sea ice concentration" now

Revised accordingly (Line 241).

Page 13, section 4.1, how about sea ice thickness, any data to compare?

Please see our response above.

Page 14, line 234-240. Not sure about this – so I assume satellite estimates of sea ice production are based on observed (perhaps also estimated) ice thickness and ice concentration? But does this statement suggest that these estimates did not take into accounts the change in ice thickness due to the bottom portion of sea ice?

In fact, satellite estimates of sea ice production are based on estimated ice thickness derived from atmospheric heat fluxes, which are normally from reanalysis products (e.g., Tamura et al., 2008; 2016; Nihashi and Ohshima, 2015; Nihashi et al., 2017). Nihashi et al. (2024) demonstrates notable differences between the satellite estimates of ice thickness using ERA5 and ERA-Interim. Inaccuracy in heat fluxes data can affect both the estimates of sea ice thickness and production rates. In addition, as acknowledged in these studies, oceanic heat fluxes are not included in the estimate of sea ice thickness and ice production, which will affect ice growth at the bottom of sea ice and is important source for the estimation errors.

References

Nihashi, S. & Ohshima, K. I. Circumpolar Mapping of Antarctic Coastal Polynyas and Landfast Sea Ice: Relationship and Variability. Journal of Climate, 28, (2015).

Nihashi, S., Ohshima, K. I. & Tamura, T. Sea-Ice Production in Antarctic Coastal Polynyas Estimated From AMSR2 Data and Its Validation Using AMSR-E and SSM/I-SSMIS Data. IEEE Journal of Selected Topics in Applied Earth Observations and Remote Sensing 10, (2017).

Nihashi, S., Ohshima, K. I. & Tamura, T. Reconstruct the AMSR-E/2 thin ice thickness algorithm to create a long-term time series of sea-ice production in Antarctic coastal polynyas. Polar Science (2024).

Tamura, T., Ohshima, K. I. & Nihashi, S. Mapping of sea ice production for Antarctic coastal polynyas. Geophysical Research Letters 35, 2007GL032903 (2008).

Page 16, line 262, add "underestimates" to read like "overestimates temperature and underestimates salinity in the surface layer…". Also modeled subsurface temperature on top of the bank appears to warmer than observed. Overall, the model predicts a deeper surface mixed layer.

Following the reviewer's suggestion, this sentence has been revised to read as "Compared to observations, the model slightly overestimates temperature and underestimates salinity in the surface layer. In the subsurface layer, the model has lower temperature in the open ocean and higher temperature on the shelf, indicating stronger CDW intrusion in the model relative to the observational data; the subsurface salinity is underestimated in the model." (Lines 299–302).

Page 19, line 293, suggest change "with temperature" to "where water temperature is above 0°C"? And after this, maybe start another sentence, sth like "As seen in Fig. 8a …"?

This sentence is changed to "in the open ocean where water temperature is above 0°C. As seen in Fig. 8a that shows the CDW dye values …" (Line 332).

Page 19, line 298, suggest deleting "the role of"

Revised accordingly, and now the sentence reads as “which could result from a southward flow …” (Line 337).

Page 19, line 301-303, this sentence is confusing. Maybe revise this to sth. like, “Compared to the western portion of the RIS, there is more ISW beneath the eastern portion, indicating stronger influence on the hydrography over the Ross Sea shelf.

We admit that is this sentence is ambiguous, and following the reviewer’s suggestion, we revised it to read as “Compared to the western portion of the RIS, there is more ISW beneath the eastern portion, indicating stronger influence on the hydrography over the Ross Sea shelf” (Lines 340–342).

Page 20, line 312, I assume here dye concentration (and Figure 9) is from bottom layer?

The dye values in Fig. 9 are vertically integrated results. We added this information in Fig. 9 caption.

Page 21, line 321-322, do tides play any role in spreading dyes?

Tidal processes may facilitate the spreading of DSW in the RISP region, particularly at the ice shelf front. As reported by Arzeno et al. (2014), data from two moorings deployed approximately 6 and 16 km south of the ice front east of Ross Island indicate that about half of the variance in the currents there are attributed to tidal influences, predominantly from diurnal components, while the remainder is due to subtidal oscillations with periods of a few days. Concurrently, Jendersie et al. (2018) proposed that the presence of a buoyancy-driven cyclonic circulation (shown in Figure 3b of their work) near the Ross Ice Shelf (RIS) plays a significant role in the southward spreading of DSW into the ice shelf cavity west of 180°. Their sensitivity experiments showed that RIS cavity temperatures and melting rates are very similar between tidal and non-tidal simulations, which indicates that the distributions of DSW should also be similar between these experiments as DSW can contribute to ice shelf melting. Together, these processes highlight the complex dynamics at play in the region. Comprehensive analysis on the role of tides in the DSW spreading beneath the RIS could be undertaken in future work using the RAISE model. As tides may play a role, we revised the sentence to “which can be associated with the southward flow as mentioned above as well as the role of tidal currents (Arzeno et al., 2014)” (Line 361).

Arzeno, I. B., Beardsley, R. C., Limeburner, R., Owens, B., Padman, L., Springer, S. R., Stewart, C. L., and Williams, M. J. M.: Ocean variability contributing to basal melt rate near the ice front of Ross Ice Shelf, Antarctica, Journal of Geophysical Research: Oceans, 119, 4214-4233, https://doi.org/10.1002/2014JC009792, 2014.

Jendersie, S., Williams, M. J. M., Langhorne, P. J., and Robertson, R.: The Density-Driven Winter Intensification of the Ross Sea Circulation, Journal of Geophysical Research: Oceans, 123, 7702–7724, 10.1029/2018JC013965, 2018.

Page 22, line 333, "in both middle and bottom" -> "in both layers"

"in both the middle and bottom layers" is changed to "in both layers" (Line 380).

Page 22, line 335, Section 4.1?

It is actually Section 4.4, and we revised this sentence to "see the discussions in Section 4.4" (Line 382).

Page 22, Figure 10 caption, "… (b) Same as (a) but density"?

Sorry for this typo, and "Same as (a) but for salinity" in the original sentence has been changed to "Same as (a) but for neutral density".

Page 23, line 345, again, this should add "properties" so it reads like "… variations of DSW properties…"

This sentence is revised to "The model also performs well in simulating the temporal variations of DSW properties …" (Line 388).

Page 25, line 384, suggest changing "falls in the range of" to "is in line with" or "is on the upper end of"

"falls in the range of" is replaced with "is in line with" (Line).

Page 25, line 388, "the satellite estimates" - > "all the satellite estimates"

Revised accordingly (Line 435).

Page 29, line 455, again it seems higher model SIP is contradictory to the less ice concentration

Please see our response to the reviewer's comment above.