Response to Review Comments

We thank the editor and reviewers for their efforts in making constructive remarks on our revised manuscript. Below you can find point-by-point replies to the review comments. In the revised manuscript, revisions are highlighted by blue color. We hope that all of the Editor's and reviewers' concerns have been addressed adequately.

Reviewer #1

Line 20: Add "atmospheric"

The sentence has been revised as "… a key atmospheric low-pressure system" (Line 20).

Line 22: "a" precursor

The sentence is revised as "DSW, a precursor of the global ocean bottom water mass" (Line 22).

Line 25: Revise the sentence to start with "This is then used to drive a ..."

The sentence is revised as "This is then used to drive a high-resolution coupled ocean-sea ice-ice shelf model covering the Ross Sea and the Amundsen Sea to assess how these ASL-driven future wind changes affect DSW formation in the Ross Sea.". (Lines 25–27)

Line 51: There's also.... Rintoul, S.R., Stewart, A.L., Johnson, G.C. et al. Antarctic Bottom Water in a changing climate. Nat Rev Earth Environ 7, 86–102 (2026). https://doi.org/10.1038/s43017-025-00750-2

This literature is added in the revised manuscript (Line 51).

Line 59: The recent Falco review would be useful here Falco et al 2024. Ocean-atmosphere-ice processes in the Ross Sea: A review. Deep Sea Research Part II: Topical Studies in Oceanography, 218, p.105429.

Falco et al. (2024) has been added to the revised manuscript (Line 77).

Line 60: Add Gunn, K.L., Rintoul, S.R., England, M.H. et al. Recent reduced abyssal overturning and ventilation in the Australian Antarctic Basin. Nat. Clim. Chang. 13, 537–544 (2023). https://doi.org/10.1038/s41558-023-01667-8

Gunn et al. (2023) is added (Line 77).

Line 68: I don't know if there's a reference out but Adele Morison's medal keynote at BACO 2025 examined this and found the Silvano arguments didn't hold up at least in her simulations.

As we were not able to find a published literature questioning the conclusion from Silvano et al. (2020), we decide to keep the original sentence for now.

Line 73: remove "could"

Revised accordingly (Line 58).

Line 74: remove "the" before "DSW"

Revised accordingly (Line 59).

Line 76: Not clear this is true for the Ross polynya... maybe change to a more broad statement about winds draining of polar plateau?

This sentence is changed to "… katabatic winds that are generated by air flows draining of polar plateau" (Line 61).

Line 94: Is "Climatological" needed? It exists for times outside of climatology.

"Climatological" is removed (Line 67).

Lines 117–118: I've come round to thinking that the objective of the study should be signaled in broad terms in the opening paragraph rather than leaving the reader hanging for a page or so. I don't think that precludes restating it here as a summation of the intro.

We agree with the reviewer that the objective of the study appeared too late in the original manuscript. Following the reviewer's suggestion, we have moved this statement to the first page of the revised manuscript (Lines 64–74) and revised the subsequent text accordingly.

Lines 120–121: Can this important sentence be reworked for clarity?

This sentence is revised as "In this work, we used a high-resolution coupled ocean–sea ice–ice shelf model spanning the Ross and Amundsen Seas to examine the effects of future ASL-related wind changes on DSW formation in the Ross Sea." (Lines 123–125)

Line 123: I think there needs to be more clarity around how when the various models are being used.

Sorry that we are a bit confused about "various models" the reviewer mentioned here. Do you mean statistical models? We provided detailed descriptions about the derivation of statistical relations in Section 2.2.1, and avoided to repeat the information in the Introduction section.

Line 128: seeing as the Discussion is minimal and the main delivery is through the subsections in the Results I wonder if they should be listed explicitly here as a guide to what is coming?

Following the reviewer's suggestion, here we added a few sentences describing the major findings from this study, which are "The results suggest that the future deepening and poleward shift of the ASL will enhance sea ice production in the Ross Sea polynyas and reduce freshwater input from basal melting of ice shelves in the Amundsen Sea, thereby increasing DSW formation in the Ross Sea." (Lines 132–135).

If the model has already been presented in previous papers should the Methodology structure be more in the order of work... (i) describe sequence, (ii) ASL projections from CMIP, (iii) ROMS DSW sensitivity studies?

We agree with the reviewer's suggestion that the order of subsections in Section 2 should be changed. The revised order is: 2.1 Numerical model; 2.2 The projection of future ASL changes based on CMIP6 models; 2.3 Sensitivity experiments.

Line 133: How does this differ from say an MITgcm approach - eg Yan et al. (2023). The salinity budget of the Ross Sea continental shelf, Antarctica. Journal of Geophysical Research: Oceans, 128, e2022JC018979. https://doi.org/10.1029/2022JC018979

MITgcm and ROMS are both widely acknowledged GCMs used in the Southern Ocean simulations, and in fact they are very similar in terms of physics kernels (primitive equations, viscosity/diffusivity treatment, flux parameterizations, etc.). MITgcm uses a z-coordinate configuration, which theoretically reduces spurious pressure gradient error that is produced in the ROMS terrain-following coordinate (s-coordinate) near the ice shelf calving front and the shelf break. However, the s-coordinate in ROMS can achieve better vertical resolution on the continental shelf and in ice shelf cavities, which greatly reduces the computational cost. Additionally, MITgcm contains a non-hydrostatic solver that theoretically better resolves the convection process associated with DSW formation, but most ocean-ice coupling applications do not activate this module due to its high computational cost (as in Yan et al., 2023). To our best judgement, the differences between the two models, or more specifically between our and Yan et al. (2023)'s implementations lie in the configurations (model domain, topography, lateral/surface boundary, viscosity, atmosphere-ocean-ice flux parameterization schemes, etc.) and parameter tuning (viscosity/diffusivity coefficients, ice-ocean turbulence, etc.). For example, compared with Yan et al. (2023) (and other regional models of the Ross Sea), our model domain is expanded towards the east to cover the Amundsen Sea; this gives us the advantage to evaluate the variability and impacts of meltwater from the Amundsen Sea ice shelves on the Ross Sea, which is often prescribed in other models. With these treatments, our model shows high skills in simulating the ice production rates, DSW formation rates, DSW temporal and spatial variability as well as ice shelf melting rates, which are summarized in Zhang et al. (2025). These results gave us confidence to conduct studies of DSW variations modulated by processes both in the Ross Sea and Amundsen Sea as shown in this manuscript.

Reference

Zhang, Z., Xie, C., Wang, C., Chen, Y., Hu, H., and Wang, X.: The Ross Sea and Amundsen Sea Ice–Sea Model (RAISE v1.0): A high-resolution ocean–sea ice–ice shelf coupling model for simulating the dense shelf water and Antarctic Bottom Water in the Ross Sea, Antarctica, Geosci. Model Dev., 18, 1375–1393, 2025.

It's not clear to me that ERA5 does well at polynya scale forcing. What are the implications here?

Modelling studies of the Southern Ocean normally use two atmospheric analysis/reanalysis products as forcing fields for their ocean-ice models — products from ECMWF (ERA5) and from the Antarctic Mesoscale Prediction System (AMPS). In our earlier work (Xie et al., 2023), we

compared surface wind, air temperature and pressure from ERA5 and AMPS with measurements at eight automatic weather stations (AWSs) in the Ross Sea and Amundsen Sea regions (including those near the Terra Nova Bay and Ross Ice Shelf polynyas), and found that the ERA5 dataset shows better agreement with observations with respective to temporal variability and root mean square errors of these variables. Correlations between wind speed from ERA5 and the AWSs reach 0.77, while correlations between air temperature reach 0.94. Therefore, we chose ERA5 as the atmospheric forcing product for our coupled ocean–sea ice–ice shelf model. While ERA5 underestimates wind speed compared with the AWSs measurements, the modelled temporal variations of sea ice production and DSW formation in the polynyas are significantly correlated with estimates from satellite and in-situ observations (Zhang et al., 2025), demonstrating that ERA5 works well for simulating the polynyas processes.

References:

Xie, C., Zhang, Z. and Zhou, M: Effects of SAM and ENSO on winter climate over the Ross Sea and the Amundsen Sea. Chin. J. Pol. Res., 35, 167–182, 2023.

Zhang, Z., Xie, C., Wang, C., Chen, Y., Hu, H., and Wang, X.: The Ross Sea and Amundsen Sea Ice–Sea Model (RAISE v1.0): A high-resolution ocean–sea ice–ice shelf coupling model for simulating the dense shelf water and Antarctic Bottom Water in the Ross Sea, Antarctica, Geosci. Model Dev., 18, 1375–1393, 2025.

Line 189: Can a phrase be included to clarify what this is? moorings? hydrography? SST? SIE?

In the original sentence, we have stated it is a comparison between modelled and observed salinity. To be clear about the observational data type, this sentence has been revised as "The simulated interannual variability of DSW salinity near the Ross Ice Shelf polynya is significantly correlated with that from 17-year station-based hydrographic observations" (Lines 199–201).

Line 196: Starting to get a lot of acronyms

We apologize for the usage of several acronyms for the ASL indices, but they are unavoidable as the indices are frequently referred to in the following texts, and we think it is better to use the acronyms to keep the texts concise.

Line 240: As above I wonder if this should come first in terms of methods for this study?

As described in our response to the reviewer's previous comment, we have reorganized Section 2 by moving the subsection on future projections of the ASL to an earlier position in the manuscript.

Lines 248–250: Better suited to a table?

A table summarizing the information of the 10 CMIP6 models selected is added in the revised manuscript (Table 1).

Line 252: Add a reference for this is you want non specialists to understand it.

The following reference is added.

Taylor, K. E: Summarizing multiple aspects of model performance in a single diagram. Journal of Geophysical Research, 106, 7183–7192, 2001.

Line 282: "deeper" in elevation or latitude?

To avoid confusion, this sentence is revised to "… deeper ASL (lower pressure) results in …" (Lines 323–324).

Lines 320–321: Isn't this the entire topic of the study?

As explained in the method section, the ASL-related wind perturbations are applied to three key regions with different dynamical processes influencing the Ross Sea DSW formation, and the 1st region is the Ross Sea continental shelf, which is the focus of Section 3.1.

Line 338: I wonder if these ("Present") should be all caps or italics or something as a reader might confuse it with the word "present".

Following the reviewers' suggestion, we have revised all experiment names to Italics.

Line 400: With the CDW coming into the system and the DSW heading out of the system - and given the importance of the continental shelf troughs - is there some direct interaction between the two water masses?

Note that this section is focused on the changes on the eastern Amundsen Sea shelf, where there is no DSW outflow. Changes in CDW intrusion and associated ice shelf basal melting in this region will affect the DSW formed in the Ross Sea downstream. Therefore, there is no direct interaction between the CDW intrusion and DSW export in this area.

Line 441: This seems more like a conclusion than discussion - as much of the results includes specific points of discussion. I suggest the Discussion and Conclusions be combined and the nice schematic be brought forward.

Following the reviewers' suggestion, we merged the discussion and conclusions into one section (Discussion and Conclusions) in the revised manuscript, and in this case the schematic (Fig.11) does not appear in the end of the manuscript.

Line 505: I think the sea ice production arrows could be shifted west a little to the main areas of polynya activity.

The sea ice production arrows are shifted west to be located in the major ice production areas in the Ross Ice Shelf polynya (Fig. 11).

Line 508: Suggest CDW be in different colour and what about mCDW in the Ross sector?

As explained in the cap 10, we used different colors in this plot to indicate processes in different regions (red for the Ross Sea continental shelf, yellow for the western Amundsen Sea, and blue for the eastern Amundsen Sea shelf and slope). As such, we did not distinguish the color of CDW from the color of other processes in the same region.