Reply to Referee #2

We are grateful to Referee #2 for the insightful suggestions on model validation, the robustness of the methods, and the Discussion section. We have followed each of the comments, presented in black font below, to carefully revise our manuscript. Our replies to the comments are given in blue, and the line numbers indicated are those of the revised manuscript with track changes enabled.

The manuscript describes the tidal energetics from a simulation of the Ross Sea. A study of this type is very much needed and there are several interesting conclusions that can help understand the role of tides in this area. The methods look robust in general, but there is no verification of the main results with observations. It was good to see the authors acknowledge the limitations of the simulations at the end of the manuscript. However, several further limitations need to be discussed.

We sincerely thank the referee for these constructive and insightful comments. We agree that, although the simulated tides have already been compared with moored observations, further validation of the mean state would strengthen the manuscript. Accordingly, the revised manuscript now includes comparisons between the simulated mean state and observations. We also appreciate the suggestion to expand the Discussion. In response, we have revised the Discussion to address the points highlighted by the referee, including the potential limitations associated with stratification sensitivity, the resolution dependence of the tidal component, and the assumptions underlying the modal decomposition. We hope the revised manuscript adequately addresses the referee's comments, and we would welcome any further specific suggestions.

Major comments:

1. The authors do not use any observations to check the results of the tidally-rectified flow. They do compare the model tides with moored observations in the methods, but they could have compared averages of the flows with the moored observations as well. There are also satellite altimetry products (for example Auger et al. (2022)) that could be compared to the output. Validating the results with observations would give the

reader more confidence in the results.

We thank the referee for this insightful suggestion. We fully agree that validating the simulated mean state against observations would substantially strengthen the robustness of our results. Accordingly, in the revised manuscript we have included comparisons of the simulated mean flow and cross-slope temperature section with observational data (Fig. 4 and Fig. S17, also attached below).
Regarding satellite altimetry, we considered the product of Auger et al. (2022), but it provides only anomalies relative to an unavailable time-mean background state (see Fig. R1 attached). Furthermore, since our model is the same as that used in Liu et al. (2025), and they have already presented a validation of the modelled sea surface height against satellite data (see Fig. R2 attached), we chose not to duplicate that comparison here. For a broader evaluation, including sea ice and water masses, we refer readers to the supplementary material of Yan et al. (2023). Collectively, we believe these newly added observational constraints can enhance confidence in our model simulation.

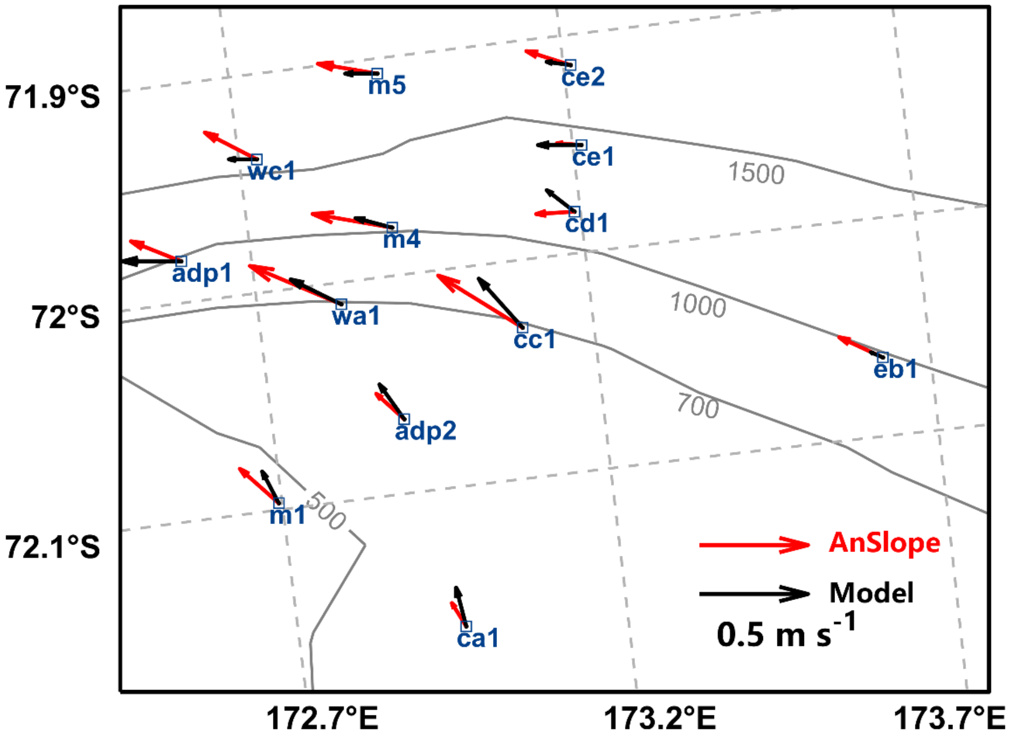

Figure 4. Comparison of the observed (AnSlope project) and simulated depth-averaged mean currents on the slope. Simulated and observed data are shown in black and red, respectively. Bathymetric contours (grey lines) are drawn for the 500, 700, 1000, and 1500 m depths. Mooring locations are indicated by open blue squares.

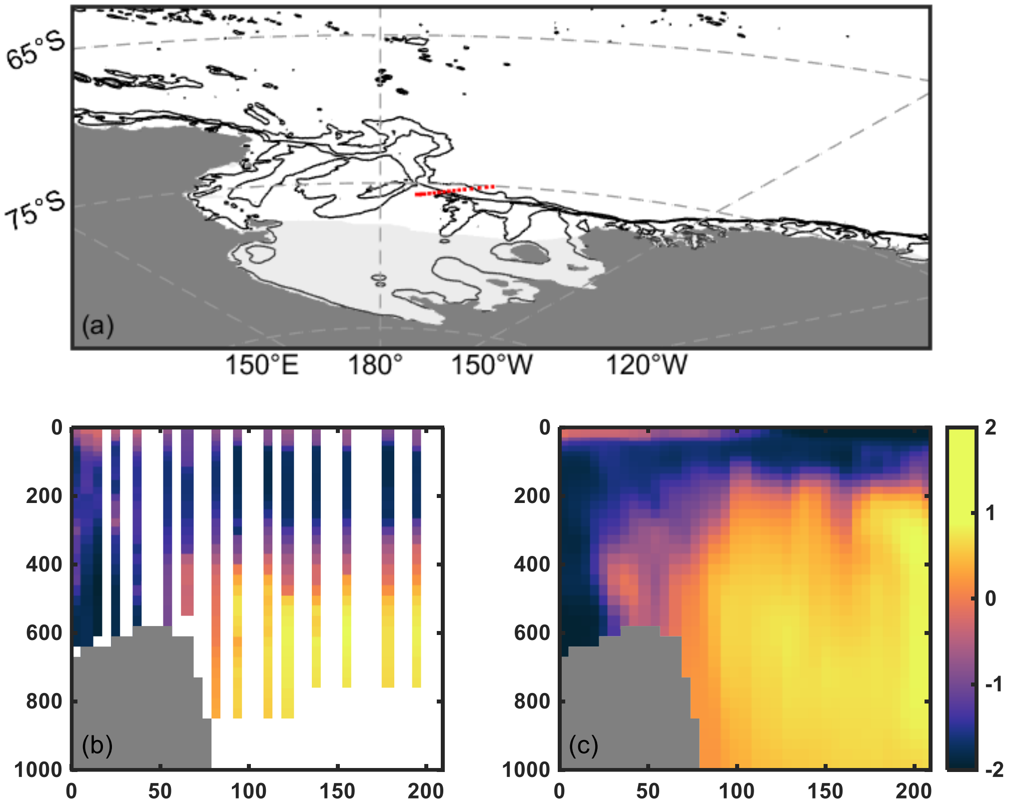

Figure S17. (a) XBT transect across the Ross Sea continental slope in the PRISM project (red dots). (b) Observed and (c) simulated temperature sections along the transect.

An estimation is computed for each gridpoint every day, allowing a daily, 25 km resolution dataset in the Southern Ocean south of 50°S. Finally, once the product is corrected, we remove the temporal mean of the SLA for a better concordance and comparison with previously published products. Our SLA product represents therefore anomalies from the 2013-2019 mean sea level.

Figure R1. Screenshot showing that Auger et al. (2022) provides only anomalies relative to their own time-mean state.

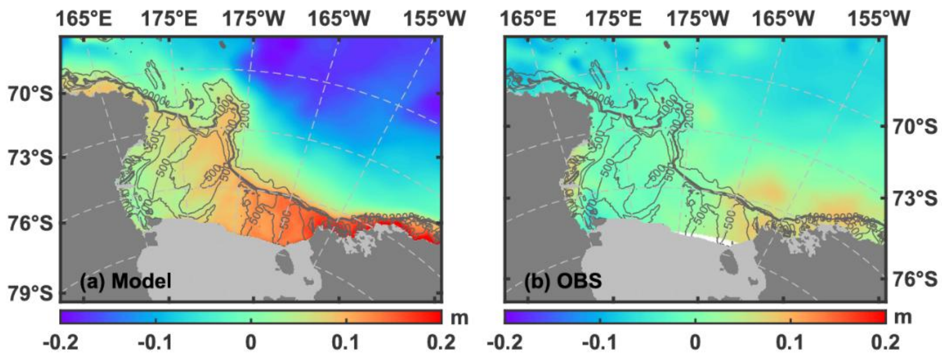

Figure 2. (a) Sea surface height from the model. The gray lines denote the isobaths (at 500, 700, 1,000, and 2,000 m). (b) As in (a), but from the CNES-CLS22.

Figure R2. Figure 2 from Liu et al. (2025), comparing modelled and satellite-observed (CNES-CLS22) mean sea surface height.

Reference:

Yan, L., Wang, Z., Liu, C., Wu, Y., Qin, Q., Sun, C., Qian, J., Zhang, L.: The salinity budget of the ross Sea Continental Shelf, Antarctica. J. Geophys. Res.-Oceans, 128, e2022JC018979, https://doi.org/10.1029/2022jc018979, 2023.

Liu,Y., Liu,C., Wang,Z., Yan,L., Haigh, M., Han,X., Liu K., Xia Y., Zeng J., Li X., Li X.: Spatial characteristics and dynamic mechanisms of the Antarctic Slope Current in the Ross Sea. J. Geophys. Res.-Oceans, 130, e2025JC023219, https://doi.org/10.1029/2025JC0232192025, 2025.

2. Sensitivity of the results to stratification is not mentioned but should be included. The baroclinic structure of the tides can change substantially with changes in stratification (see for example Song and Chen (2020), Varlamov et al. (2015)). This can be added to the discussion of the vertical resolution.

We thank the referee for pointing out this important issue. We fully agree that stratification plays a critical role in modulating the baroclinic structure and energy conversion of internal tides, and that this aspect deserves explicit discussion in the context of vertical resolution. In the revised manuscript, we have expanded the discussion at Lines 575–581 to address the limitations of vertical resolution in resolving bottom-trapped tides and higher modes, as well as the modulating effects of stratification on energy conversion, with reference to the relevant literature. We believe this addition effectively addresses the referee's concern and strengthens the discussion of vertical resolution in our study.

3. The sensitivity of the heat flux to the resolution is mentioned with respect to underestimating the contribution from the mesoscale component. How the tidal component would change with increasing resolution is not discussed but needs consideration.

We thank the referee for raising this important point. We agree that the resolution dependence of the tidal component of heat flux deserves consideration alongside the mesoscale component. In the revised manuscript, we have added a discussion on this issue at lines 570-574 and 581-583. We propose that barotropic tidal heat flux is largely insensitive to horizontal resolution, but internal tide generation and its contribution to heat flux may be altered by finer grids through better resolved small-scale topography.

Finer grids may also better capture nonlinear interactions between internal tides and mesoscale eddies, which can modulate the spatial redistribution of tidal energy and indirectly affect heat flux patterns. While Si et al. (2023) examined the resolution dependence of tidal heat advection, their analysis focused primarily on the fresh-shelf regime, leaving the dense-shelf response largely unexplored. We therefore discuss this as an open question and a direction for future work.

Reference:

Si, Y., Stewart, A. L., and Eisenman, I.: Heat transport across the Antarctic Slope Front controlled by cross-slope salinity gradients. Sci. Adv., 9, eadd7049, https://doi.org/10.1126/sciadv.add7049, 2023.

4. When decomposing the tidal modes into surface and bottom-trapped modes, Tanaka et al. (2023) assumes the sea surface height changes of the surface mode are an order of magnitude greater than those of the bottom-trapped mode. Comparing the examples from that study with these, it’s not clear that this is the case on the Ross Sea shelf, particularly in the troughs where the bottom-trapped mode coincides with the stratification due to dense water. How would the interpretation change if the two were coupled?

We appreciate the referee 's concern about Tanaka's (2023) modal decomposition on the Ross Sea shelf. To address this, we have calculated the baroclinic pseudo sea level (BPSL) from the density field following McWilliams et al. (2024, their Eq. 17). This quantity should include the combined effect of the bottom-trapped mode and stratification on sea surface height. Our calculation shows that BPSL variations are less than 2 cm throughout the Ross Sea, even in the troughs where dense-water stratification is strongest (Fig. S20a, also attached below). This amplitude is more than an order of magnitude smaller than the total SSH change (Fig. S20b, also attached below), confirming that the surface mode dominates the SSH variability and that Tanaka's (2023) decomposition should be valid for this region. We have discussed this in Lines 395-400 in the revised manuscript.

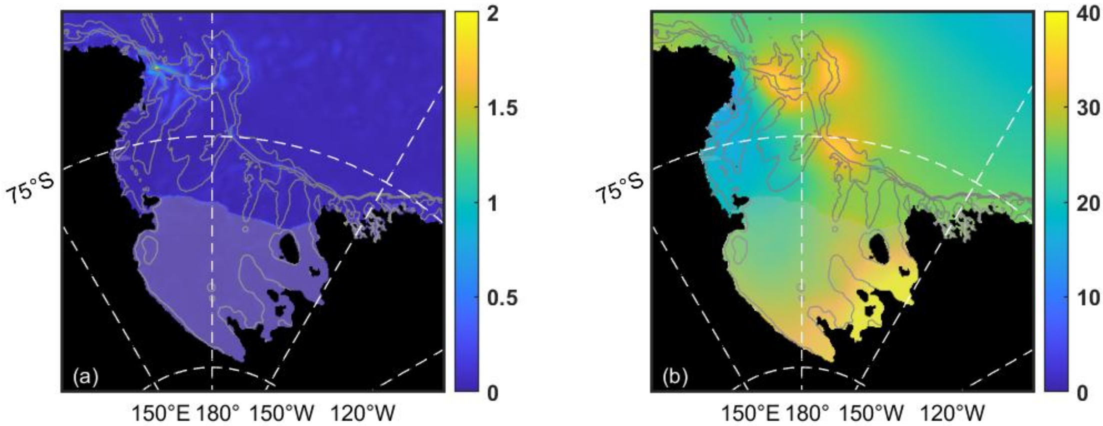

Figure S20. Spatial distribution of (a) the time-mean magnitude of baroclinic pseudo sea level (cm), and (b) the time-mean magnitude of total tidal SSH (cm). Bathymetry contours are spaced at 500, 1000, and 2000 m.

Reference:

McWilliams, J. C., Molemaker, M. J., and Damien, P.: Baroclinic sea level. J. Adv. Model. Earth Syst., 16, e2023MS003977, https://doi.org/10.1029/2023MS003977, 2024.

Minor comments:

Line 58: “Previous studies” would be more appropriate than “Precious works”.

Sorry for the typo. The wording has been revised to “previous works” (Line 58).