

Impact of Grid Resolution on the Abyssal Ocean Representation in Numerical Models: A Focus on Vema Channel

The manuscript by Santos et al. investigates the influence of horizontal and vertical grid resolution on the representation of abyssal waters by evaluating three ocean reanalyses (ECCO, SODA, and GLORYS) and one forward ocean model (OFES2) from the Southern Ocean to the Brazilian Basin. The manuscript is well organized and clearly written. I recommend its acceptance for publication after moderate revisions.

Major Comments:

1. The WOA is a robust climatology representing the long-term mean state of the global ocean. However, it is generated using optimal interpolation, meaning that the observational coverage is spatially irregular. Since WOA18 is used as the primary reference for model evaluation, the authors should consider showing the distribution of observations (or data coverage) within the study area, as well as the estimated error field. For example, in Figure 2a, to what extent is the cold tongue surrounding the Antarctic continental shelf supported by observations, and how much is simply an artifact of the interpolation procedure? This information is important for assessing the reliability of the comparisons presented throughout the manuscript.
2. The description of the numerical models should be expanded to include additional information on bathymetry and lateral boundary conditions. Furthermore, I encourage the authors to discuss the fundamental differences between forward ocean models and ocean reanalyses, which would help the readers better understand the expected differences in variability and long-term trends, and, more importantly, the aspects that should be considered when comparing reanalyses with forward ocean models. Finally, some discussion of how differences in model configuration (e.g., atmospheric forcing, vertical and horizontal mixing parametrizations, sea-ice representation, among others) may contribute to the differences found in this study would strengthen the scientific impact of the manuscript.

Minor Comments:

1. Line 30: Should this reference be *Santos et al. (2026)*? In addition, is this really the first published study reporting this result?

2. Lines 53-64: A recent study by Noro et al. (2025), *Variability of the Weddell Sea Deep Waters in the GLORYS12v1 Reanalysis*, should be included in this background discussion.
3. Fig.1: It would be helpful to include latitude labels on the y-axis.
4. Table 2: It would be useful to include the temporal resolution of each model.
5. Lines 188-191: Since the authors acknowledge the limited spatial resolution of WOA18, it is not entirely clear how the comparisons should be interpreted. For example, WOA18 shows temperatures near -0.7°C in the central Weddell Sea, whereas OFES2 simulates colder waters. Given that WOA18 has a horizontal resolution of 0.25° while OFES2 has a resolution of 0.1° , can WOA18 confidently be considered the reference in this region?
6. Figure 2.: I suggest changing the continent color to dark gray, as the Antarctic continent is difficult to distinguish from the light-red color scale. In addition, consider outlining the Argentine and Brazilian basins with rectangles (at least in the WOA18 panels) to facilitate interpretation.
7. Why were HYCOM products not included instead of ECCO or SODA? Recent HYCOM analyses have shown improved performance in representing abyssal circulation and water masses.
8. Fig.5: I suggest changing the color used for either ECCO or OFES, since the two shades of green are difficult to distinguish. Also, am I the only one who cannot identify the GLORYS triangles in the Taylor diagrams?
9. Although FESOM-VH provides the best representation of abyssal water properties and inter-basin connectivity, its correlation coefficient with the moored observations is among the lowest. Do the authors have an explanation for why FESOM-VH reproduces the observed temporal variability so poorly despite its otherwise good performance?
10. Line 499: A correlation coefficient of approximately 0.2 should not be considered statistically meaningful. Therefore, I suggest rephrasing the sentence rather than emphasizing that this was "the highest value." Instead, it would be more appropriate to highlight that none of the four simulations reproduced the temporal variability observed by the moorings with significant skill.
11. Although the study provides a comprehensive analysis of eight simulations, the rationale for selecting each OGCM is not clearly explained, nor is the "motivation" (see line 521) to perform four targeted experiments with FESOM 2. It would be interesting to have this discussed in the manuscript. Moreover, the analyses were well conducted throughout the study area; however, I cannot see a clear focus on the Vema Channel, it appears to be just another analysis site. In practice, the manuscript focuses on the representation of abyssal water masses in numerical

models. Therefore, I suggest removing "*A Focus on Vema Channel*" from the title.

12. There appears to be some inconsistency in the naming of the additional figures, with references alternating between "Appendix" and "Supplementary Material." Please check and standardize the terminology.
13. Although the manuscript focuses on the impact of model resolution, I felt that it lacks a more direct conclusion regarding which reanalysis provides the best representation of abyssal waters and which numerical model performs best overall. A concise synthesis of the relative strengths and weaknesses of each product would help readers better appreciate the main findings and practical implications of the study.