

Response to Reviewer #2

Please see below for a point by point response to your comments. Comments are in blue, our responses are in black.

Sincerely,
Dylan Schlichting

RC: *This manuscript is clear, well written, and easy to follow. The figures are of high quality, and the abstract provides an accurate summary of the work. The paper presents a thorough assessment of the strengths and weaknesses of MPAS-O in representing submesoscale dynamics in the northern Gulf of Mexico compared with dedicated regional ROMS simulations.*

AR: Thank you very much. We have done our best to address your comments and questions, which have improved the manuscript. Note that we have made substantial changes to §1 and §3 in response to comments by the other reviewer.

RC: *My main concern is that the discussion of the causes of the MPAS-O biases, particularly the lack of submesoscale eddies over the shelf, remains somewhat limited. For example, I was surprised to see that MPAS-O uses a first-order Forward Euler scheme for tracer time stepping. Such a low-order scheme is highly diffusive and may substantially damp tracer gradients and associated submesoscale eddy activity. It would therefore be valuable to assess whether numerical diffusion contributes to the weaker shelf variability. One possible approach would be to compare the effective horizontal resolution of MPAS-O and ROMS using kinetic energy spectra at selected locations, following the methodology of Soufflet et al. (2016).*

AR: Thanks for the comment. We agree that we are not able to fully explain the weaker submesoscale eddy activity in MPAS-O; as noted in §4, we cannot attribute this discrepancy to a single, clearly identified cause. However, we have expanded our discussion in the revised manuscript as noted in response to other comments.

Regarding tracer time stepping, we agree that the use of Forward Euler is not ideal, and recommend that higher-order schemes be used in Omega, the next-generation successor to MPAS-O currently under development (Petersen et al., 2026). However, we do not find evidence that Forward Euler time stepping is a dominant source of the weaker summer eddy activity in the simulations presented here. In the idealized M-A plume experiments of Part I (Hinson et al., 2026), MPAS-O and ROMS use the same numerical schemes as in the present study. Across most of the horizontal resolutions tested (0.1–10 km), MPAS-O generally exhibits less numerical salinity mixing than ROMS based on volume-integrated discrete variance decay (Part I, Figs. 10–11). Thus, although these results do not isolate the contribution of the time-stepping scheme, they do not indicate that MPAS-O has systematically greater numerical tracer mixing than ROMS. We acknowledge, however, that the online mixing diagnostics by Burchard and Rennau (2008) were not included in the realistic simulations presented in this paper, and therefore we cannot rule out a significant contribution from numerical mixing to the differences in fidelity in the realistic simulations.

We also considered the suggested kinetic energy spectral analysis following Soufflet et al. (2016). We agree that such an analysis could provide useful information about the effective resolution and dissipation characteristics of the two models. However, we do not think that it would provide a definitive answer to whether numerical dissipation is responsible for the weaker submesoscale variability in MPAS-O. MPAS-O already exhibits substantially weaker summer submesoscale eddy activity over the shelf. Consequently, differences in the kinetic energy spectra would be expected regardless of whether numerical dissipation is the underlying cause of the discrepancy or instead results from the absence of the corresponding eddies.

The spectra would therefore characterize the effective resolution of the resulting solutions, but would not distinguish between a loss of variability caused by numerical dissipation and a difference in the resolved flow arising from other aspects of the model formulation.

RC: *Another important aspect that deserves more discussion is the representation of bathymetry using the z^* vertical coordinate. This coordinate system may significantly affect shelf dynamics because current-topography interactions are generally less accurately represented than in terrain-following coordinate systems. A useful sensitivity test, even on a shorter simulation and perhaps with a coarser horizontal mesh, would be to substantially increase the number of z^* levels (for example, to around 200) to evaluate the impact of vertical resolution on shelf circulation and submesoscale dynamics.*

AR: Thanks for the comment and suggestion. We agree that z^* coordinates generally represent flow over topography less accurately than terrain-following coordinates (Barron et al., 2006; Wise et al., 2022), and that this limitation may be important in regions with steeply sloping bathymetry. We initially suspected that the z^* vertical coordinate might be a dominant contributor to eddy suppression on the shelf. However, we think this is unlikely for several reasons. The TXLA shelf has a gentle mean slope of approximately 10^{-3} (Hetland, 2017), and the summer plume is surface advected, with brackish water typically confined to the top few meters of the water column (Zhang et al., 2014) – limiting direct interaction between the plume and bottom topography. As shown in Appendix A of Part I, MPAS-O and ROMS produce similar representations of submesoscale baroclinic instabilities over sloping bathymetry despite their differing vertical coordinates, with comparable velocity profiles and volume-averaged kinetic energy despite MPAS-O using a constant 2.5 m layer thickness and ROMS a terrain-following coordinate ranging from approximately 0.3–2.5 m. We have expanded the corresponding paragraph into §4 to better reflect this information.

To further examine the role of vertical resolution, we performed a trial simulation identical to MPAS-O: USGS but with 80 vertical layers and a minimum layer thickness of 1.0 m (i.e., the same vertical grid as MPAS-O: JRA). The resulting PDFs of surface Rossby number were effectively unchanged from the standard MPAS-O: USGS simulation, suggesting that increasing vertical resolution alone does not substantially alter simulated submesoscale activity in MPAS-O. Unfortunately, this simulation was lost following a major data loss on the LANL institutional computing scratch file system, preventing us from including these results here or in the manuscript.

A more comprehensive sensitivity study with substantially increased vertical resolution would also be prohibitively expensive. The MPAS-O: USGS simulation already achieves only approximately 0.5 simulated years per day using 5,120 cores (40 nodes), and because performance scales approximately linearly with the number of vertical layers, doubling vertical resolution would nearly double computational cost. Even shorter experiments (i.e., yearly) would be infeasible within our current computing allocations.

RC: *Similarly, bathymetric smoothing, commonly applied in terrain-following models such as ROMS, may also contribute to some of the differences between the simulations. This aspect deserves additional discussion and, if feasible, dedicated sensitivity experiments.*

AR: Thanks for the comment. We agree that bathymetric smoothing, and its absence in our MPAS-O configuration, deserves additional discussion, and have expanded §4 as discussed above. As noted there, we examined the model output for evidence of spurious mixing or flow impedance associated with the interaction of unsmoothed bathymetry and the vertical layers and found none. As discussed above, we also note that the TXLA shelf's gentle mean slope limits the extent to which unsmoothed bathymetry could generate large local gradients. The ROMS smoothing is primarily done for stability purposes over the shelf break because the computational domain extends to over 2500 m depth. A dedicated sensitivity experiment that reruns MPAS-O with a smoothed bathymetry field would also be prohibitively expensive with our current computing

allocations.

RC: *Throughout the manuscript, a large number of acronyms are introduced (e.g., RYF, SYPD, etc.). Reducing the number of acronyms to only those that are essential would improve readability.*

AR: Thanks for the suggestion. We have removed unnecessary acronyms.

RC: *Table 1: Thank you for providing this clear and detailed summary of the model configurations. I was particularly surprised to see that MPAS-O uses a first-order Forward Euler scheme for tracer time stepping, as this choice may have important implications for the representation of submesoscale dynamics.*

AR: You are welcome! Please refer to our response above regarding Forward Euler.

RC: *Line 113: "five years on 120 CPU cores" is this correct? Could this instead refer to 120 compute nodes?*

AR: Thanks for the questions. No, the ROMS model is indeed only run on 120 CPU cores, compared to 5120 for MPAS-O. This is partly because ROMS has roughly ten times fewer horizontal cells, fewer than half the vertical levels, and twice as large as a timestep as MPAS-O.

RC: *Line 157: I would expect the influence of sea ice on the representation of the Loop Current (LC) to be largely indirect, through large-scale circulation changes (e.g., the MOC), rather than through local processes. This point may be beyond the scope of the present study.*

AR: Thanks for the comment. We agree that the influence of sea ice on the Loop Current is likely to be primarily indirect, through changes in large-scale circulation, and that this is beyond the scope of the present study. We have therefore removed this statement from the revised text. We have also moved the discussion of model coupling to the beginning of §2.1, where we believe it provides a more natural transition into the model configurations.

References

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