Response to reviewers

We appreciate the time and thoughtful comments that the reviewers have provided for our submission to *Geoscientific Model Development*. Please below see for a point by point response to your comments. Comments are noted in blue, our responses are noted in black.

Sincerely,
Kyle Hinson

Comments to the author

Reviewer 1

RC: ***The manuscript presents a test case and compares the performance of ROMS and MPAS-O. The test could be of interest for other groups, as it explores model performance at rather small scales where dynamics is very much different from the dynamics dominated by mesoscale baroclinic instabilities. I therefore recommend this manuscript for the journal. Below are some suggestions/remarks that could be helpful.***

AR: Thank you very much for your comments and support of this paper for publication.

RC: ***The Introduction is too MPAS-centric in my opinion. The value of this manuscript is in presenting a properly documented test case and simulation results. The test case and documented diagnostics can be used by other groups. Place some focus on this. Staggered discretizations on unstructured meshes have spurious modes, and therefore need a certain level of dissipation. Test cases that explore fine-scale dynamics which might be affected by dissipations or by modes are indeed welcome. You show that for MPAS all this does not present a problem. Other groups can, perhaps, follow.***

AR: Thanks for the comment. Part of the focus of this paper and its companion is to better evaluate the ability of MPAS-O to represent fine spatial scales and, if not, identify its main limitations. Still, the point is well taken, and we have added a paragraph in the introduction to emphasize some of the generalizable aspects of these analyses.

RC: ***145 'enstrophy-dissipating' – since the Leith viscosity enters the momentum equation, it serves to dissipate kinetic energy dissipating. (It is the derivation of this viscosity that is based on the idea of enstrophy dissipation).***

AR: Thanks for the comment. We have corrected the sentence as follows "An option for the grid-scale and flow-aware Leith viscosity (Leith, 1996) is also available; however, it has not been tuned and is beyond the scope of the current study."

RC: ***Section 2.3.5. Formula (8) gives an estimate only for 1D uniform velocity. In a general case the third order scheme will introduce viscosity in the direction of velocity, while the one in MPAS-O is isotropic. So it is difficult to say which viscosity is larger. A better way***

to judge would be to estimate the KE dissipation due to horizontal advection in ROMS and explicit biharmonic viscosity in MPAS-O (This would be of interest for others too).

AR: Thanks for the comments, we agree that Eq. 8 only provides an estimate for 1D uniform velocity. For this reason, we have removed Eq. 8 from the paper and the corresponding values in Tab. 1 so we do not make claims we cannot support. The start of Section 2.3.5 now reads as

"ROMS' third-order upwind momentum advection scheme (Shchepetkin and McWilliams, 1998, 2005) introduces implicit fourth-order biharmonic dissipation. This dissipation is grid-scale and flow-aware such that it depends on the local velocity, grid spacing, and flow direction relative to the grid. For this reason, high-resolution limited domain and estuarine ROMS configurations usually employ a small amount of explicit horizontal mixing to reduce grid-scale noise not damped by the implicit dissipation (Schlichting et al., 2023, 2024)."

We agree estimating that estimating the KE dissipation due to horizontal advection would be a useful analysis, but our online numerical mixing diagnostics based on Burchard and Rennau (2008) are currently developed only for the active tracers. The proposed analysis could be done based on the methods following Klingbeil et al. (2014). However, we would need to modify the source code and think that this is beyond the scope of this paper.

RC: ***230 The relative vorticity is, strictly speaking, not an invariant.***

AR: Thanks for the comment, we agree with the reviewer. We intended "invariant" to refer to independence from the choice of coordinate system, rather than to a dynamical invariant. We have revised the text to clarify this distinction.

RC: ***265 This diagnostics works only for the first order upwind advection schemes. For higher-order schemes, the estimate it provides locally are contaminated by much larger dispersive errors, see Banerjee et al., 2024. For bulk volume estimate one can look at the change of s^2 due to advection of s without running any special diagnostics.***

AR: Thank you for the comment. We agree that the Burchard and Rennau (2008) diagnostic can be locally contaminated by dispersive errors at the grid scale when applied to higher-order advection schemes. While the local estimates from Burchard and Rennau (2008), Klingbeil et al. (2014), and Banerjee et al. (2024) can differ substantially at the grid scale, the volume-integrated estimates are expected to be substantially more robust. In Appendix B, we show that the volume-integrated numerical mixing converges to the volume-integrated tracer variance tendency for a linear-advection test. Since the primary goal of our analysis is to compare the total numerical mixing between models, rather than to characterize its detailed spatial distribution, we believe the BR08 diagnostic is sufficient for this purpose. We have clarified the limitations of the BR08 diagnostic in the revised manuscript, particularly when interpreting the surface $\mathcal{M}_{num}$ fields shown for each model in Fig. 8. The purpose of Fig. 8

is to qualitatively demonstrate that numerical mixing is generally strongest near the fronts in both models, consistent with the results of Schlichting et al. (2023, 2024), rather than to make a quantitative comparison of the local numerical mixing rates.

We are unsure what specific analysis is being referred to by the suggested change in s^2 due to advection. If this refers to computing an offline tracer squared or tracer variance budget, Schlichting et al. (2023) showed that these offline budgets calculated from ROMS output do not converge to the BR08 diagnostic, even when using an output frequency of 10 minutes. If this refers to looking at the changes to s^2 distributions over time, this is difficult to interpret because the uniform gradient in s won't allow us to examine convergence of numerical diffusive fluxes robustly. We therefore do not consider an offline s^2 budget to provide a sufficiently accurate alternative for the present analysis and have left this approach beyond the scope of the study.

RC: ***389 ' ... of antidiffusion ...' One cannot call this antidiffusion, as the negative DVD may reflect dispersive errors of the method.***

AR: Thank you for the comment. We agree and now state that "' Significant patches of negative numerical mixing are visible in both models, with MPAS-O showing slightly more organized patches. The negative patches indicate local tracer variance production, rather than destruction, and may reflect either anti-diffusive or dispersive numerical errors associated with the advection schemes."'

Throughout the manuscript, we now describe negative values as local tracer variance production rather than anti-diffusion.

RC: ***407 ' ... anti-diffusive response ... ' I find this result to be strange. If the third order advection is used in MPAS-O together with flux correction method, both are dissipative. Even if the third-fourth order advection is applied, it is still dissipative. The standard FCT algorithm is dissipative.***

AR: Thanks for the question. We agree that this is strange and is one of the outstanding questions resulting from the paper. To our knowledge, no studies of numerical mixing in dynamically-complex domains (e.g., estuaries, river plumes, eddying regimes) have shown that the volume integral of numerical mixing can become strongly variance producing.

RC: ***485 ' is more anti-diffusive' — or less diffusive***

AR: Good point, we have amended the text as suggested.

Reviewer 2

RC: ***This manuscript presents a systematic comparison of MPAS-O and ROMS simulations over a wide range of horizontal resolutions. The experiments are computationally substantial, and the comparison of frontal statistics and physical and numerical mixing***

is potentially useful for the development of high-resolution unstructured-grid ocean models. The manuscript is generally well organized, but the central claim concerning the representation of submesoscale baroclinic instability requires stronger dynamical support. I recommend revision before publication.

AR: Thank you for your review and comments on the manuscript, we have responded to each of the points of feedback below.

RC: ***The title and stated objective emphasize coastal submesoscale baroclinic instabilities, whereas most analyses focus on kinematic and statistical characteristics, including surface kinetic energy, vorticity, divergence, strain, frontal velocity gradients, and tracer mixing. Similar submesoscale intensity or flow statistics do not necessarily imply that the two models represent the underlying instability dynamics similarly. In particular, comparable kinetic energy may result from different balances among available-potential-energy release, baroclinic conversion, wind input, and physical or numerical dissipation. This possibility is especially relevant because the two models show different instability timing and mixing behavior. The authors should therefore provide a more direct comparison of the instability energetics, such as the evolution of available potential energy and eddy kinetic energy, baroclinic conversion or vertical buoyancy flux, and relevant dissipation terms, at least for representative resolutions.***

AR: Thank you for this comment and suggestion. This work is in many ways a continuation of earlier research conducted by Hetland (2017), which more conclusively demonstrates that this class of instabilities is definitively baroclinic. Further elaborating upon this point in this manuscript would be somewhat didactic, and would also contribute less to the main thesis of the research. Still, the comment was interesting to consider and to more fully address it we have undertaken additional analyses to compute time series of eddy kinetic energy, the results of which have been added to Figure 3 (for formatting purposes, we have now also chosen to split out the KE spectra into a separate, smaller Figure 4). The methodology for computation of both time series have also been added to the methods (Section 2.4.1). These findings support many of our previous results, with important differences noted in the results and discussion sections, especially regarding the apparent spurious mode in the MPAS-O 100 m solution activated at approximately day 25 and its correspondence with a large spike in negative numerical mixing (now Figure 10c).

RC: ***Line 52: "ROMS; Shchepetkin and McWilliams, 2005".***

AR: This has been corrected.

RC: ***Line 192: "tracer advection schemes".***

AR: Thank you for catching this error, which has been corrected.

RC: ***Line 203: it should be Table 1.***

AR: Thank you for catching this error, this has been corrected.

RC: ***Line 227: "vertical velocity cell phases" should likely be "vertical velocity cell faces".***

AR: Thank you for catching this error, this has been corrected.

RC: ***Line 241: Please define explicitly how the frontal angle α is determined and how points with weak gradients are treated.***

AR: We have further noted in the text that the frontal angle α follows the definition and schematic of Figure 1 in Hetland et al. (2025). Points with weak gradients are treated equivalently because the purpose of this analysis is to determine the direction of frontal dynamics. The text has also been modified to remove the phrase "largest positive gradients" in order to avoid confusion.

RC: ***Figure 4 caption: please explain "Negative relative vorticity values correspond to a greater number of eddies, while positive vorticity values indicate a greater number of fronts."***

AR: Thank you for pointing this out. The text has been amended to, "Negative relative vorticity values correspond to a greater amount of anticyclonic flows characterized by eddy activity, while positive vorticity values are associated with frontal processes." which we believe more accurately describes the results. The baroclinic instabilities in the idealized simulations mimic the frontal eddies found on the real Texas-Louisiana shelf. These frontal eddies have anticylonic cores and cyclonic fronts, see Hetland (2017) and Schlichting et al. (2023) for examples.

RC: ***Lines 353–355: it would be better to use $|\delta/f|$, with convergence and divergence treated separately.***

AR: Thank you for this comment, this appears to have been a typo and has been corrected in the manuscript such that it now shows the absolute value of δf^{-1} rather than the absolute value of 1. As seen in (now) Figure 5e-f, we do in fact treat convergence and divergence separately in our analyses.

RC: ***Figure 6 caption: The ROMS panels should be cited as (e–h).***

AR: This has been corrected.

RC: ***Line 378: "principal component".***

AR: This has been corrected.

RC: ***Line 473: "certainly due to".***

AR: This has been modified as suggested.

RC: ***Figure A1 caption: "and (b) MPAS-O".***

AR: This has been modified as suggested.

References

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