Response to Editor Comments

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: ***A number of referencing issues identified during citation verification checking. These are minor but please correct them where possible.***

AR: Thank you for pointing this out. We have gone through each all references individually and made corrections as needed.

RC: ***In your Code and data availability section, you identify two datasets used and direct the reader to the institutions that provide these with weblinks. Please amend these links so that they point at the pages where the datasets are available, and include a "data last accessed on: . . . " statement.***

AR: We have uploaded all ROMS input forcing files used to produce the simulations to the Zenodo repository. This is now stated in the code and data availability section. Otherwise, we would have to provide over nine individual links to the various streamflow gauges, which the user would still have to process. By providing the ROMS river forcing file directly, the simulations are fully reproducible and easier to run if a reader is interested. As a result, we have removed the referenced links to the ROMS river forcing data sources. For the same reason, we have removed the link to the HYCOM output used to force the ROMS simulations since the climatology, boundary, and initial files are provided directly instead.

Response to Reviewers (copied from previous submission)

We appreciate the time and thoughtful comments that the reviewers have provided for our submission to *Geoscientific Model Development*. Please see below for a point by point response to their comments. Comments are noted in blue, our responses are noted in black. Note that this document contains the combined responses to both reviewers, which was originally uploaded separately on the Discussion page.

Sincerely,
Dylan Schlichting

Response to Reviewer #1

RC: ***Using a novel global MPAS-O configuration with regional grid refinement, the authors investigate the impact of river runoff on submesoscale dynamics in a realistic estuarine environment and compare the results with those from ROMS. Overall, the manuscript is well written and well structured. The model configurations are described in sufficient detail, and the results are presented and discussed clearly.***

AR: Thank you for the constructive comments and positive assessment of our manuscript. We appreciate your review, which has improved the manuscript's quality. For clarity, we note that our study focuses on a river plume rather than an estuary. An estuary is generally a partially enclosed coastal body of water where freshwater from rivers mixes with seawater, whereas a river plume is the buoyant freshwater outflow that spreads into the coastal ocean after exiting the estuary.

RC: ***However, the manuscript lacks a clear motivation and a discussion of the current state of the art. In particular, the authors should provide more context on how river runoff is currently represented in global ocean models, and why this becomes a limitation in high res models and why these improvements are scientifically relevant. For example, the implications for biogeochemical processes, carbon uptake in estuaries, or ecosystem studies could be highlighted. It would also be useful to discuss alternative approaches for improving the representation of estuarine dynamics, such as regional models (e.g., GETM) or global models with coastal grid refinement.***

AR: Thanks for these suggestions. We have expanded §1 to better motivate the study, in particular by discussing the biogeochemical relevance of coastal-open ocean exchange due to submesoscales. We now state the study's primary objectives more explicitly: to test whether regional refinement with MPAS-O can produce a dynamically credible representation of realistic coastal submesoscale processes, and to identify the limitations that emerge as increasingly fine scales are resolved. For reference, we include a more thorough discussion of using grid refinement or model-model nesting to seamlessly represent coastal processes within a global framework in Part I.

We added a paragraph to §2.2 discussing how river runoff is often represented in global ocean models. Global models typically remap river discharge from river-routing models or observational datasets onto the ocean grid and spread it across many coastal cells — a practical approach at coarse resolution ($\mathcal{O}$(10–100 km)) that distorts plume dynamics once resolution becomes sufficiently fine. We also discuss point source alternatives used by some global models (e.g., ICON-Coast, ECCO-Darwin) (Mathis et al., 2022; Savelli et al., 2026), noting that these remain less flexible than regional models, in which the vertical distribution of runoff can be individually prescribed and rivers impart horizontal momentum flux upon entering the ocean. We chose to place this in the model setup section, where it can be presented concretely alongside each model's specific river configuration, rather than in the lengthy introduction.

RC: ***Furthermore, the authors should clearly define what they mean by "submesoscale dynamics." (see e.g. McWilliams 2016, Mahadevan 2016 Gula 2021) The manuscript would benefit from a discussion of the horizontal resolution required to adequately resolve submesoscale processes throughout the seasonal cycle, both on the shelf and in the open ocean. In addition, the authors should clarify which submesoscale processes are expected to be resolved by the model and which remain under-resolved (e.g., gravitational, baroclinic, and symmetric instabilities).***

AR: Thanks for the comments. We have revised the introduction to define submesoscales to be flows with $\mathcal{O}(1)$ Rossby and Richardson numbers and strong lateral density gradients.

We also believe it is important to consider the resolution required to resolve various submesoscale processes. Let's first focus on the TXLA shelf. One way to estimate the relevant horizontal length scales of various instabilities is using the first deformation radius $R_d = NH/f$, where N is the buoyancy frequency, H is the full water column depth, and f is the Coriolis parameter. As done in previous studies (Hetland, 2017; Hilditch et al., 2025; Schlichting et al., 2024), this is preferable to using the mixed layer deformation radius during summer because baroclinic instabilities often span the entire water column and the mixed layer can be very shallow (i.e., < 2 m) . As shown in Fig. 1 (a) of this document, R_d is generally $\mathcal{O}$(10-15 km) during summer and is associated eddies with characteristic diameters of $\mathcal{O}$(20-50 km), which is in good agreement with the studies mentioned above.

During winter, downcoast winds confine the plume closer to the coast, prohibiting the formation of larger eddies discussed above. We intuitively expect the deformation radius to shrink during winter as the shelf becomes less stratified (Zhang et al., 2014), also shown in Fig. 1 of this document. However, less is known about the types of submesoscale instabilities and their vertical structure during winter. Previous work has

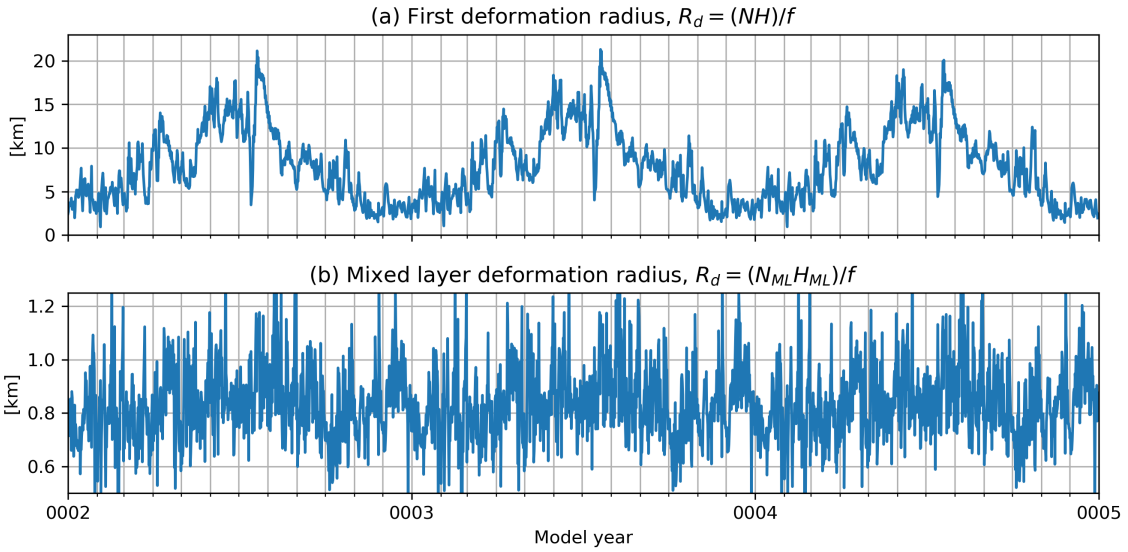


Figure 1: (a) First deformation radius estimate averaged from ROMS: $k-\omega$ from years 2-5 over the magenta box in Fig. 1 of the revised manuscript. (b) Same as (a), but for the mixed layer deformation radius.

mostly focused on characterizing submesocales during summer because they modulate the spatial extent of the hypoxic zone on the shelf (Bianchi et al., 2010; Ruiz-Xomchuk et al., 2021). For this reason, it is unclear if the mixed layer deformation radius $R_d = N_{ML}H_{ML}/f$, where the N_{ML} is the buoyancy frequency averaged over the mixed layer and H_{ML} is the mixed layer depth, is a more suitable diagnostic, which we show in Fig. 1 (b) of this document. The mixed layer deformation radius is unsurprisingly noisier and likely requires filtering to remove diurnal/inertial variability. It ranges from roughly 0.6-1.2 km and exhibits weaker seasonal dependence than the full water column deformation radius.

Additional diagnostic criteria based on the Richardson number could be performed to better characterize the types of instabilities found on the shelf and their associated spatial scales (Thomas et al., 2013). Although this would be an interesting analysis, we believe it is out of scope because it shifts part of the paper's focus to process discovery. Since our focus is primarily on the models' representation of baroclinic instabilities during summer, we believe it is suitable to call the models submesoscale eddy-resolving in summer and submesoscale-permitting during winter by citing previous studies. Previous studies using sub-kilometer modeling and high-resolution in situ observations (Körner et al., 2026; Qu et al., 2022) suggest that processes such as symmetric and gravitational instabilities are not resolved by the models. We have incorporated the previous two sentences into the revised introduction.

Regarding the open ocean in the GoM, we believe it is sufficient to call MPAS-O submesoscale-permitting year round. With a 500 m resolution nested ROMS simulation of the GoM, Barkan et al. (2017) showed that the offshore mixed layer deformation radius is 2.2 km during winter and 1 km during summer. We would like to repeat this analysis with MPAS-O. However, LANL Institutional Computing experienced a catastrophic data loss where our high-resolution 3D variable snapshots were stored. We only have weekly 3D snapshots, which we believe is too coarse to meaningfully capture the daily timescales associated with submesoscale variability. Instead, we directly cite Barkan's paper at the start of §3.1 to support the claim that the model is submesocale permitting.

RC: ***The authors argue that submesoscale eddies are suppressed during winter, (either due to wind forcing or insufficient model resolution?), and that this affects the estuarine volume transport. However, the figures appear to show more eddy activity during winter than during summer. This apparent contradiction should be resolved and discussed in greater detail.***

AR: Thanks for the comments. This was incorrect wording on our part, as we used "eddy-suppressed" to describe the loss of $\mathcal{O}$(20-50 km) submesoscale shelf eddies, rather than a reduction in overall submesoscale activity. This has been corrected throughout the manuscript. In the open ocean, submesoscale activity is typically enhanced during winter due to stronger mixed layer instabilities associated with deeper mixed layers, allowing more energy to be extracted from lateral density gradients (Callies et al., 2015).

One way to quantify the temporal variability and prominence of submesoscale eddies would be through the eddy kinetic energy (EKE). However, the characteristic horizontal scale of submesoscale motions changes seasonally, as indicated by the deformation radius (Fig. 1 of this document), making EKE difficult to quantify. EKE can include substantial contributions from wind-driven flow that are unrelated to the frontal eddies. For example, Kobashi and Hetland (2020) demonstrated reasonable agreement between modeled and observed EKE on the shelf during summer, but also showed that non-summer EKE contains prominent contributions from transient, depth-dependent currents associated with atmospheric fronts. They used a wind-only, constant-density simulation to reduce the wind-driven contribution when interpreting the EKE associated with frontal eddies. With this correction, they found that the EKE associated with frontal eddies is stronger in summer than in winter. However, because this approach does not unambiguously separate eddy-driven and wind-driven variability, we have removed our broader claim that submesoscale activity is suppressed during winter and instead refer specifically to the reduced prominence of the $\mathcal{O}$(20–50 km) frontal eddy field.

We have reorganized the start of §3.2 to reflect this updated discussion of seasonal variability. We qualitatively show the scale transition by adding in a winter snapshot of the surface vertical vorticity normalized by the Coriolis parameter so it can be directly compared with summer (Figs. 7-8 of the revised text). We also included this comparison here for convenience in Figs. 2-3 of this document. This figure also serves a secondary purpose of demonstrating MPAS-O's better qualitative agreement with ROMS during winter.

RC: ***Finally, the manuscript lacks an evaluation against observations, which is important for assessing the realism of the simulated dynamics. While ROMS is used as the reference model, it would strengthen the manuscript if the authors briefly discussed how well ROMS reproduces observations in this region, drawing on previous validation studies where available. For example; do we expect a brackish lense, as seen in MPAS-O, in the estuary or not?***

AR: Thank you for this comment. We agree that an evaluation against observations is important for assessing the realism of simulated shelf dynamics. The most recent validation by Kobashi and Hetland (2020) showed that the model reproduces the seasonal and interannual variability of the horizontal salinity gradient magnitude on the shelf. See also above for our comment about the EKE. We now mention these points in §1 and at the start of §3.2.

We chose not to include a new skill assessment primarily because the MPAS-O: USGS simulation does not reproduce the characteristic summer submesoscale eddy field that is present in the ROMS simulation. Qualitatively, MPAS-O: USGS lacks the coherent anticyclonic eddy cores bounded by cyclonic vorticity associated with frontal eddies; instead, relative vorticity over the Louisiana shelf is weaker, noisier, and less spatially organized than in ROMS. In addition, a quantitative validation against observations is complicated by the use of 2003–2004 repeat-year forcing, which combines forcing from two different calendar years into a single annual cycle and therefore does not correspond to a single observed year. Thus, it would be incorrect to compare simulated fields to in situ observations.

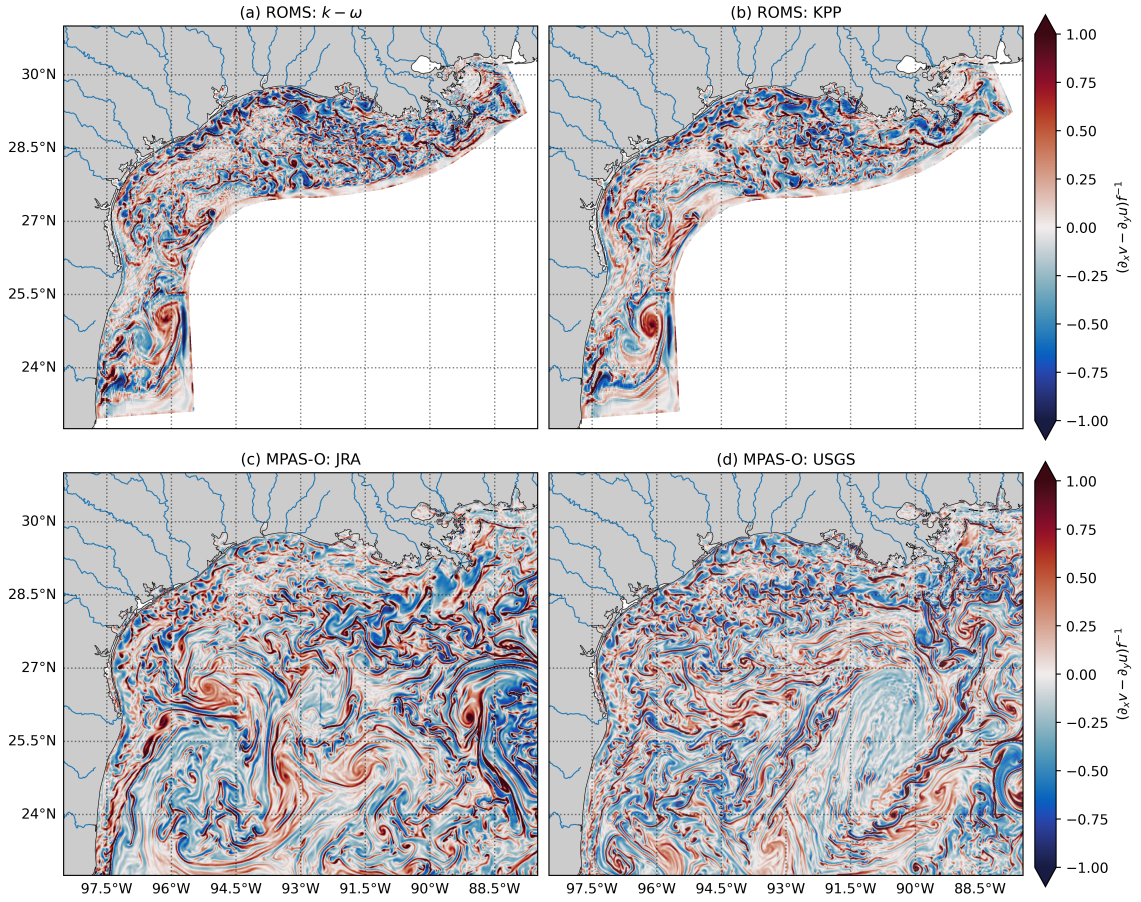


Figure 2: Zoom in of ζf^{-1} snapshots for (a) ROMS: $k-\omega$, (b) and ROMS: KPP, (c) MPAS-O: JRA, and (d) MPAS-O: USGS during winter. Time is the same as Fig. 5 (a) of the revised text.

We do not expect the persistent brackish lens to form over the TXLA shelf as seen in MPAS-O. The model has salinity differences relative to ROMS of 5-10 g kg^{-1} over much of the shelf during summer. Coupled with the fact that the model does not produce the same frontal sharpness ($\zeta f^{-1} > 1$), we hypothesize the salinity gradient magnitude on the shelf would also be significantly different and thus misrepresented. We state this more clearly now in §3.2.

RC: ***L26: There are also studies using unstructured-grid models to investigate submesoscale processes, for example, the work by Uchida et al. (2022) (including FESOM) and Epke et al. (2025).***

AR: Thanks for the comment. We have incorporated this into the revised introduction.

RC: ***L31: Please clarify what is meant by a surface-advected submesoscale eddy field. It would also be helpful to briefly describe the background coastal circulation.***

AR: This was a typo, we meant surface-advected plume. This means that the plume is generally wind-driven and that the isopycnals generally do not interact with the seafloor, see Horner-Devine et al. (2015). For this study,

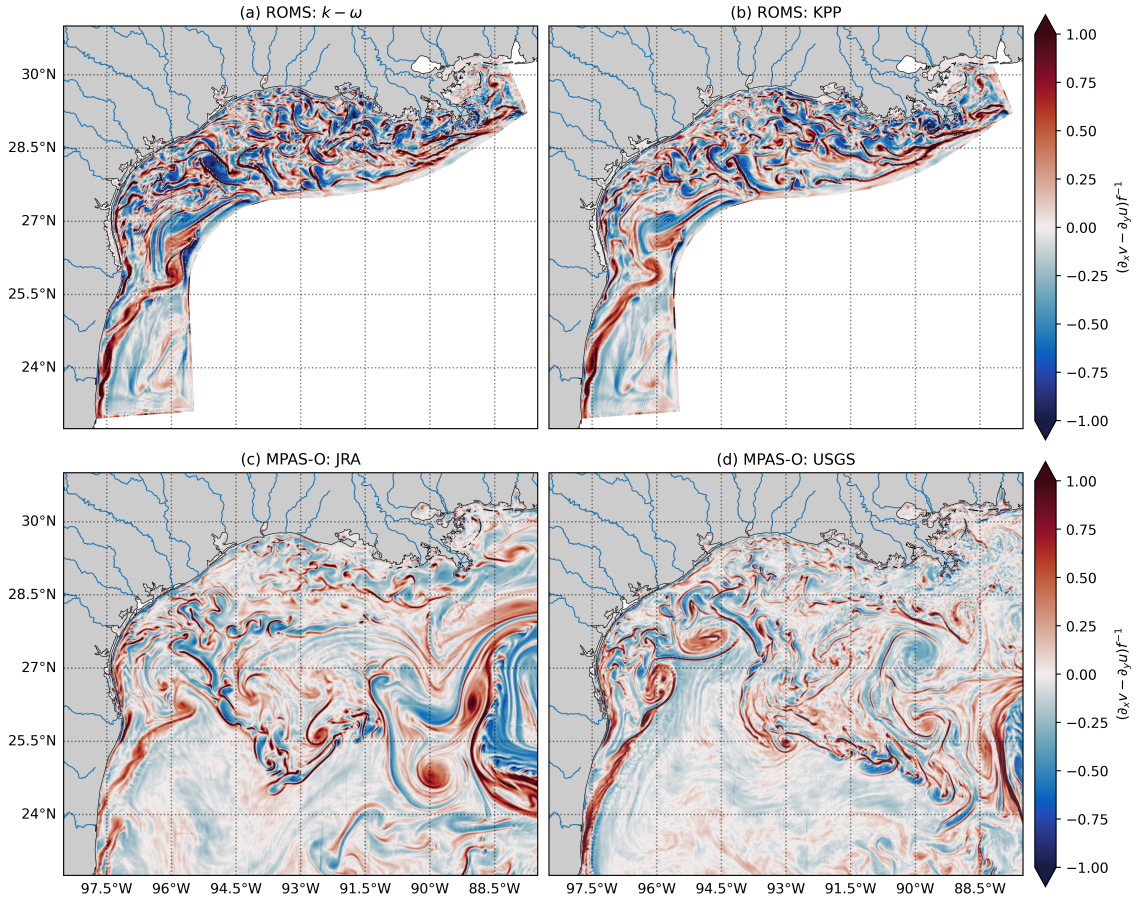


Figure 3: Same as Fig. 2, but for summer. Time is the same as Fig.5 (b) of the revised text.

we believe that our description of the coastal circulation is sufficient since our analyses do not go beyond a statistical description of the flow (i.e., the velocity gradient tensor PDFs, seasonal averages of surface active tracers).

RC: ***L36: Please mention that the growth rate of mixed layer instabilities scales with mixed layer depth (MLD) and that enhanced submesoscale activity during winter is a well-established result, e.g. see Mahadevan (2016.)***

AR: Thanks for the suggestion. We respectfully decline to mention this in the introduction due to the regime shifts between summer and winter on the shelves as discussed above. Rather, we mention this point in §3.1 when discussing submesoscales in the the open ocean.

RC: ***L50: Consider mentioning GETM here as an alternative approach for representing rivers and estuaries, and briefly discuss how similar concepts could be incorporated into global ocean models.***

AR: Thanks for the suggestion. As discussed above, we have included a broader discussion of river forcing in regional and global models in §2. We now mention GETM in §2.1. However, we believe that a detailed

discussion of how the greater flexibility of regional models could be incorporated into global ocean models is beyond the scope of this study. In global models, river runoff is typically remapped from a river-routing model or observational data source to the ocean grid using predefined mapping files, which provide limited flexibility for modifying individual river sources. Providing the level of control available in regional models would require additional model infrastructure to allow individual rivers to be specified and modified, including their source locations, salinity, and vertical distribution of freshwater input. Developing such a framework is a non-trivial model development problem that is outside the scope of this study.

RC: ***L56: Please clarify what is meant by coastal processes being "ill-posed." Which specific processes or limitations are being referred to?***

AR: This sentence has been removed from the revised text.

RC: ***L57: Please be more specific about the numerical errors and physical processes being discussed. The explanation should be sufficiently self-contained so that readers do not need to consult the cited paper.***

AR: This sentence has been removed from the revised text.

RC: ***L60: This paragraph seems out of place. The section should first introduce its objectives before discussing the model setup.***

AR: Agreed, we now list the paper objectives before describing the model setup.

RC: ***L102: Please refer to Fig. 4 here so that readers can directly assess the seasonal variability.***

AR: Done as recommended.

RC: ***L108: It would be useful to briefly explain how temperature is prescribed in this approach.***

AR: Thanks for the suggestion. Stefan and Preud'homme (1993) estimate the water temperature as $T_w = 5.0 + 0.75T_a$, where T_a is the air temperature from the nearest weather station (often measured at the streamflow gauge). The coefficients in this relationship are derived empirically from a linear regression between observed stream and air temperatures. It is important to note that the ROMS river temperatures are therefore estimated from in situ air temperature observations using this empirical relationship, whereas MPAS-O directly prescribes river temperature using JRA air temperatures.

RC: ***L162: What is the vertical distribution of the river runoff in MPAS-O? Is the runoff applied only at the surface, or is it distributed over multiple vertical levels?***

AR: Thanks for the question. The vertical distribution of river runoff follows an exponential decay with depth of the form $\exp(z/0.75)$. We have clarified this in §2.2.

RC: ***L167: Please clarify how river runoff is implemented in MPAS-O. Is the runoff applied to individual coastal grid cells or several? Does it mean there is no spreading of the river runoff in MPAS-O: USGS simulation?***

AR: Thank you for the comment and questions. This highlights an important distinction between localized point sources and prescribed spreading, which can become less clear at high resolution. In the MPAS-O: JRA simulation, river runoff is initially assigned to the ocean grid cell nearest to the location specified by the runoff dataset and is then horizontally distributed over several surrounding grid cells according to the remapping file. In the MPAS-O: USGS simulation, the JRA river runoff fluxes and their source locations are explicitly removed within the spatial area that corresponds to the ROMS model domain and replaced with the fluxes and source locations from the ROMS configuration, with the latter mapped to the nearest MPAS-O grid cells.

Thus, the MPAS-O: USGS simulation does not use the JRA spreading within the ROMS domain; instead, it uses localized “source" cells corresponding as closely as possible to the ROMS point sources given the MPAS-O grid. Outside the ROMS domain, the JRA runoff forcing and its prescribed spreading are retained.

In both cases, a single river may be represented by multiple model grid cells. For example, to represent the complex geometry of river outflow at high resolution, the ROMS configuration represents the Mississippi River using 43 localized point sources (cells) and the Atchafalaya River using 8 point sources, while each of the remaining rivers is represented by a single source. This distribution captures the observed geometry of each river's discharge relative to the model resolution and is also necessary for numerical stability, as concentrating $\mathcal{O}(10^4\ \mathrm{m}^3\ \mathrm{s}^{-1})$ of discharge into a single cell would cause stability issues. We have revised §2.2 to clarify these distinctions and to explicitly describe how river runoff is initially located and subsequently distributed in each simulation.

RC: ***L198: Changing the temperature of the run of to (10 m JRA air temperature) is only performed to reduce spurious spikes in the sst? Is this change applicable to other regions (e.g. high latitudes) or is it specific to this estuary?***

AR: Thanks for the questions. Yes, changing the river runoff temperature to the 10 m air temperature is done to mitigate spurious spikes in SST. It is applied to *all* rivers in both MPAS-O simulations. We could not determine the cause of these spikes after ruling out obvious configuration issues (e.g., CFL-induced errors) and attribute it to a bug/deficiency in the SST-based interpolation scheme that prescribes river runoff temperatures. We acknowledge this is unsuitable for other regions (high latitudes) and did not pursue a more permanent solution given the limited scope of this study.

RC: ***L201: Please clarify this statement. Does the model fail to produce these features only during the final simulation year, or throughout the entire simulation period?***

AR: Thanks for the question. We have revised the sentence as follows: “MPAS-O: JRA was run for four years to conserve computational resources because it did not produce summer shelf eddies comparable to ROMS throughout the entire simulation (discussed in §3.2)."

RC: ***L208: Relative vorticity is typically normalized by the Coriolis parameter, yielding the Rossby number. Large Rossby numbers indicate ageostrophic motions and are commonly used as an indicator of submesoscale dynamics. It is therefore unclear what the authors mean by normalized relative vorticity. Since the Rossby number is already nondimensional, the current terminology is potentially misleading. Please clarify the definition and explain whether an additional normalization has been applied.***

AR: Thanks for the comment. The sentence has been amended as follows: “Fig. 5 shows winter and summer snapshots of the relative vorticity normalized by the Coriolis parameter (i.e., the Rossby number), $\zeta f^{-1} = (\partial_x v - \partial_y u)/f$, from the MPAS-O: JRA simulation." We believe this is a matter of personal preference, as previous studies (McWilliams, 2016; Schlichting et al., 2023) commonly use the ζf^{-1} notation or use “normalized relative vorticity" as shorthand.

RC: ***L211: The wording in this paragraph is imprecise, and several terms are used inconsistently. For example, what is meant by a chaotic front - rather unstable fronts, however this needs to be shown... The authors should use more precise terminology throughout this discussion. A more convincing analysis would combine the Rossby number with a snapshot of the lateral buoyancy gradient. The authors could then state, for example: "Large Rossby numbers are found throughout the domain, indicating ageostrophic motions that are commonly associated with submesoscale dynamics. This is consistent with the presence of numerous fronts visible in the lateral buoyancy gradient field (Fig. XX)." However, while the presence of fronts and elevated Rossby numbers is consistent with submesoscale activity, it does not***

demonstrate the underlying generation mechanism. The manuscript currently infers that these features arise from instabilities, but this is not shown. To support this interpretation, the authors could evaluate additional diagnostics, such as the Richardson number or criteria for mixed layer/baroclinic instabilities, to demonstrate that the observed structures are indeed generated by baroclinic instability. Finally, based on the figures presented, it is not obvious that submesoscale dynamics are more prevalent on the continental shelf than offshore. This conclusion should either be supported with quantitative diagnostics or stated more cautiously.

AR: Thanks for the comments. We have reformulated §3.1 to focus on assessing whether MPAS-O reproduces the established seasonal variability of submesoscale activity, including the enhanced prevalence of submesoscales during winter associated with deeper mixed layers. We acknowledge that this provides only a basic assessment of the model's ability to represent submesoscale dynamics in the open ocean. However, one reason we chose not to pursue a more process-based analysis is that the model configuration was developed and tuned primarily to provide the best possible results on the TXLA shelf, potentially at the expense of its representation of open-ocean submesoscale processes. Rather, we view §3.1 as a baseline assessment of the model's broader submesoscale behavior and a starting point for future work.

We have also replaced imprecise terminology, such as "chaotic fronts," with more specific descriptions of the observed flow structures.

We agree that diagnostics such as the Richardson number or explicit criteria for mixed layer or baroclinic instability would provide a more mechanistic assessment of the processes responsible for generating individual features. However, demonstrating the generation mechanism of specific structures is beyond the scope of this study.

Finally, we agree that our original statement claiming that submesoscale activity is more prevalent over the continental shelf than offshore was not sufficiently supported by quantitative diagnostics. We have removed this claim and revised the discussion to make only comparisons that are directly supported by the diagnostics.

RC: ***L213f: An assessment of the relevant horizontal length scales required to resolve mixed layer baroclinic instabilities is missing. In the open ocean, the characteristic scale is typically given by the mixed layer deformation radius (see e.g. Dong (2020) et al., Epke (2025) et al.). It is unclear whether this scaling also applies in the coastal and estuarine regions considered here, where stratification and topographic effects may modify the relevant length scales. The authors should provide a clear scale analysis and compare the resulting length scales to the effective horizontal resolution of the model. This is important, as insufficient resolution may lead to a suppression or underrepresentation of submesoscale dynamics. The authors might include this diagnostic in the manuscript to support their conclusions regarding the presence or absence of submesoscale features in different regions and seasons.***

AR: Thanks for the suggestions. Please refer to our responses above regarding the horizontal resolution required to resolve submesoscales both in the open ocean and on the shelf.

RC: ***L224f: The mechanism by which river discharge suppresses mixed layer instabilities is not clearly explained. Please clarify how freshwater input modifies the stratification and mixed layer depth, and how this in turn affects instability growth. The phrase interplay between stratification and mixed layer depth is too vague and should be made more explicit in physical terms.***

AR: Thanks for the comments. We have removed this point from §3.1.

RC: ***L236: The statement is not consistent with what is shown in Figs. 5. The figures appear to indicate more eddy activity on the shelf during winter than during summer. This discrepancy should be clarified and***

discussed. In addition, the change in colorbar scaling between panels is not helpful for comparison. For consistency, the Rossby number should be shown using a fixed range (e.g. -1 to 1) across all relevant figures.

AR: Thanks for the comments. Please refer to our response to you previous comments about eddy activity in summer versus non-summer. We have updated the colorbar as recommended.

RC: ***L256: Again, the terminology should be revised to use Rossby number consistently rather than normalized relative vorticity (see comment on L208).***

AR: Please refer to our previous response regarding normalized relative vorticity.

RC: ***L261: The term realistic eddies is unclear and should be defined more precisely. Also mention that the reference meant here is ROMS. L270: The definition of surface boundary layer depth is not provided. It should be specified whether this corresponds to the mixed layer depth or another diagnostic quantity, and how it is calculated. The same clarification is needed in L272. A comparison of the mixed layer depth with available climatologies would be very helpful to assess realism. In addition, it would be useful to discuss whether the simulated freshwater lens in the estuary is realistic and how it compares to observational estimates.***

AR: Thanks for the comments. We have replaced the term "realistic eddies" in this case with more precise descriptions of the $\mathcal{O}$(20–50 km) frontal eddies produced by the ROMS simulation. The surface boundary layer depth is calculated online by the respective KPP implementations at each model timestep and is not the mixed layer depth, which we have clarified in the caption of Fig. 9. The ROMS implementation is described in Appendix B of McWilliams et al. (2009), whereas the MPAS-O implementation is described in Van Roekel et al. (2018).

Please refer to our response to your major comment about including a comparison of model diagnostics with observations and whether the simulated freshwater lens is realistic.

RC: ***L281: Eddy suppression is not clearly defined. It is unclear whether this refers to suppression of mixed layer eddies due to insufficient horizontal resolution in MPAS-O, due to atmospheric forcing (e.g. winds), or due to other physical or numerical factors.***

AR: Thanks for the comment. We have clarified the definition of eddy suppression in §3.2: “A key result is that the MPAS-O simulations (Fig. 3c-d) do not produce a submesoscale eddy field with the same complexity or qualitative characteristics as the ROMS simulations during summer (Fig. 3a-b). We refer to this reduction in submesoscale activity as eddy suppression, meaning a reduction in the growth and persistence of the frontal instabilities."

RC: ***L283: The main conclusion of this paragraph is not clearly stated. If the authors are suggesting modifications to the KPP scheme in MPAS-O based on ROMS behavior, this recommendation should be made explicit and supported more clearly.***

AR: Thanks for the comment. We refrain from making a specific recommendation for modifying the KPP scheme because it is unclear why MPAS-O develops the freshwater lens associated with the reduced vertical mixing. Although the weaker vertical mixing is a potentially important contributor to the contrasting behavior, the present model-model comparison does not establish whether it is a cause or consequence of the suppressed eddy field, or whether other configuration choices are involved. Another problem is that the CVMix implementation of KPP contains over a dozen tunable parameters. If we assume that KPP is the dominant cause of eddy suppression, it is still unclear which parameters must be tuned or what further

modifications to the scheme are required.

RC: ***L302: It is unclear which model exhibits a larger freshwater volume—MPAS-O or ROMS. The authors suggest that reduced freshwater volume in MPAS-O may be linked to fewer mixed layer eddies, but this relationship is not demonstrated. This could also be influenced by differences in the vertical mixing parameterization, in particular the KPP scheme. Please clarify.***

AR: Thanks for the comments. MPAS-O exhibits a larger freshwater volume than ROMS, as shown in Fig. 10 of the revised manuscript. We agree that our explanation was incomplete and have revised the manuscript as described below: eddies generated at the plume front stir and export freshwater offshore (Thyng and Hetland, 2018). Once freshwater crosses the 100 m isobath, it seldom returns to the shelf, as strong bathymetric gradients at the shelf break constrain flow through potential vorticity conservation and generally isolate shelf and open-ocean dynamics (Thyng and Hetland, 2018). A weaker eddy field in MPAS-O should thus retain more freshwater on the shelf rather than exporting it, consistent with its larger volume. If KPP is a significant cause of eddy suppression, it therefore contributes to a larger volume of freshwater on the shelf.

RC: ***L375: The statement regarding eddy suppression in winter is not consistent with the diagnostics shown earlier. This needs to be reconsidered in light of the Rossby number fields (see related comments above).***

AR: We have corrected this as discussed in our responses to previous comments.

RC: ***L385: This interpretation appears incorrect. The figures of Rossby number indicate the opposite trend, namely stronger eddy activity during winter compared to summer. The same concern applies to L404. This discrepancy should be resolved.***

AR: We have corrected this as discussed in our responses to previous comments.

RC: ***L419: It would be useful to show instantaneous or spatial fields of surface density gradients, rather than only domain-averaged quantities, to better support the interpretation of the results.***

AR: Thanks for the comment. We are unsure exactly what you are requesting with respect to this discussion point. We include the snapshots of the surface Rossby number in §3.2 to demonstrate MPAS-O's lack of eddies during summer, then use averaged quantities in §3.3 to demonstrate the impact on the surface active tracers. We respectfully decline to include snapshots of the surface density (salinity) gradient magnitude because it would not change the conclusion that MPAS-O has significantly more freshwater on the shelf year round than ROMS.

Response to Reviewer #2

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.

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