We thank the referee for critically reading this manuscript and providing helpful feedback, which has added a great deal to improve the manuscript and clarify the text. We respond to all issues addressed in their comments below, as well as adding the revised changes in the manuscript. The Reviewer comments are included here in black, and our answers below their respective comments in blue. The text that has been modified in the manuscript according to the reviews is presented in *italic*. The line numbers in the answers refer to the marked-up manuscript version with tracked changes

RC1

General Comments

This is a well-written and scientifically rigorous manuscript that addresses the spatiotemporal scales of mode water transformation in the Sea of Oman. The authors combine Argo climatologies with high-resolution underwater glider observations to investigate the relative roles of isopycnal and diapycnal processes in modifying mode water volume and properties. The methodological framework, based on σ-τ coordinates, is innovative and provides new insight into how mesoscale eddies influence transformation rates.

I commend the authors for the clarity of their writing, the thoroughness of their analysis, and the careful integration of climatological and glider datasets. The paper convincingly demonstrates the added value of high-resolution glider observations for capturing episodic and small-scale processes that are missed in climatologies. The conclusions about eddy-driven intensification of both diapycnal and isopycnal transformations are compelling and make a valuable contribution to our understanding of mode water variability.

Overall, this is an excellent paper that makes a valuable contribution to the field. I recommend publication after the authors consider the following specific and technical comments.

- Joe Gradone

Specific Comments

Line 10: The second sentence of the abstract is a bit of a beast to read. I only became oriented once making it to the end where you state “deeper oxygen minimum zone”. I suggest moving the word “deeper” to the first mention of the oxygen minimum zone and then consider breaking this sentence up into two sentences.

Thank you for the suggestion. We applied the suggested changes and now it reads in L10 as *“This capped and well-mixed oxygenated layer decouples the oxygen minimum zone from ocean surface processes. It also provides a space for remineralisation, reducing oxygen demand in the oxygen minimum zone.”*

Line 13-14: Initially, when I read the abstract, I was questioning how you could do this analysis on a 3-day temporal scale with a monthly climatology. The use of glider data in the analysis is clear in the main paper. I suggest maybe something like “higher resolution underwater glider observations” to distinguish the difference.

Thank you. We swapped the order of the sentence, so now it reads:

L12-15: *“We perform a volume budget analysis to investigate the mechanisms driving mode water volume change in the Sea of Oman from monthly to 3-day temporal scales using* *monthly climatologies derived from profiling floats and high-resolution underwater glider observations."*

Line 81: Observations projected onto an orange line, not a blue line, correct?

Thank you. After modifying the figure, the line is black. We have modified the text (L81, 125 & 187).

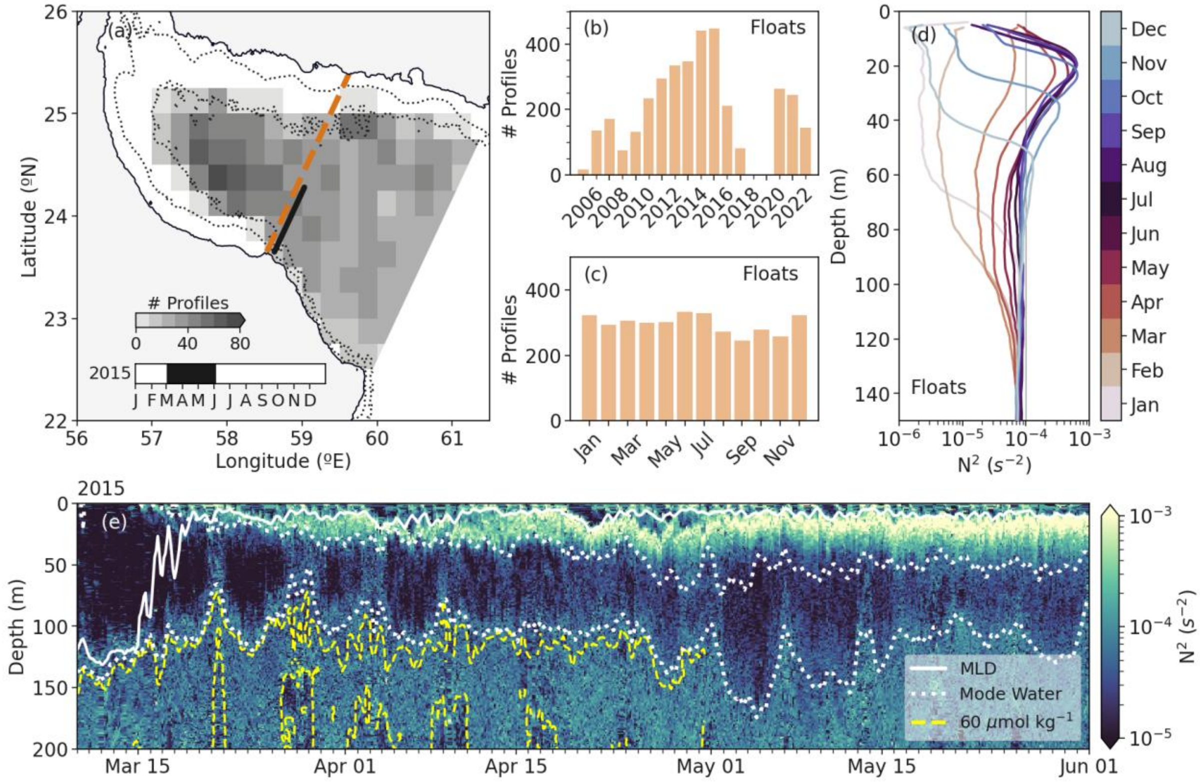
Line 82: Is 2 km horizontally not a bit too fine for glider data?

Thank you for this comment. We agree that glider sampling does not intrinsically resolve 2-km horizontal scales, since the distance between successive profiles is typically several kilometres, depending on platform speed, dive geometry, etc. Our intention was not to imply true 2-km resolution but to choose a bin size small enough that profiles were not spatially aliased. Near the shelf, as topography changes rapidly, binned mean can skew towards shallow profiles due to the greater number of profiles.

To ensure that we did not introduce artificial high-wavenumber variability, all sections were subsequently smoothed using a 6-km horizontal running mean, which lies above the submesoscale range, while still allowing mesoscale gradients to be captured. The 2-km binning therefore, acts only as an intermediate gridding step to minimize data gaps and spatial aliasing before the physically meaningful smoothing is applied. Moreover, this choice is also coarser than the gridding strategy used in previous work in the region (e.g, Font et al. (2024) applied a 1-km grid followed by a 3-km rolling mean).

Figure 1e: I recognize there is a lot of information on this plot but as someone who is colorblind, I cannot fully understand what is going on. The red dotted line is difficult to see and the white and pink lines blend together.

Thank you. We have applied the changes you suggested in Figure 1. We made the MLD white solid, MW white dotted, and the oxygen contours yellow dashed. We have removed the gray density contours to simplify panel e).



Line 184: Text says Figure 1e-g, but those subplots do not exist in Figure #1

Thank you. Apologies for the mistake, those panels existed in a previous version of figure 1. Changed to Fig 1e.

Line 186: Define what mintier means here, not necessarily standard oceanographic knowledge. You define it well on line 201, so just consider some language to that effect here.

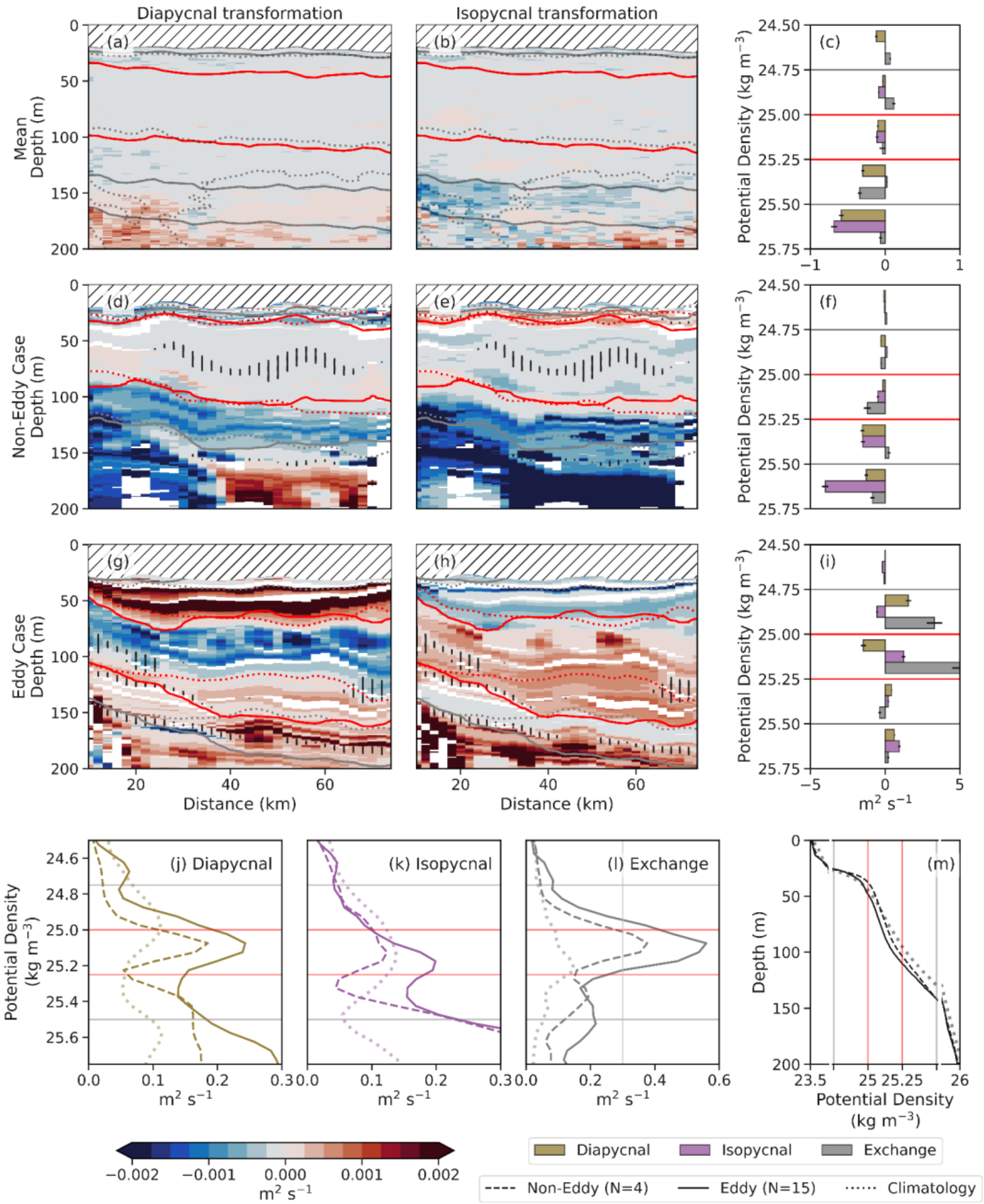
Thank you. We have added *"(i.e. fresher and colder)"* after mintier to explicitly state the definition (L209).

Figure 5: If this a pain, don't sweat it, minor comment here. It would help to further orient the reader if you could make panels c, f, and i have the 24.5 isopycnal surface approximately in line with the depth where the hatching stops in the corresponding plots to the left.

Thank you for the suggestion. We have tried your suggestion, but it squeezes the rest of the density bands too much, reducing their visibility, so we have decided to keep it as it was.

Figure 5 cont.: I cannot tell the difference between the colored bars for "diapycnal", "isopycnal", and "exchange" in panels c, f, and i. **Figure 5 cont.:** Helpful for the reader if you could put either a title or a small line of text, sort of like a legend, showing j-l correspond to diapycnal, isopycnal, and exchange, though the change in colors will also likely help this a ton.

Thank you. We have applied the changes you suggested in Figure 5 (now figure 6) and changed the colors consistently across the other figures to be colorblind friendly. We apologize for not having taken this into account. We have also added a small title in each of the panels j-l.



Lines 316: Since the exchange term is computed as a residual, uncertainties in isopycnal and diapycnal terms will propagate directly into this estimate. Could you expand on how robust this separation between mixing and advective exchange is, and whether the relative magnitudes may be sensitive to error?

We agree that diagnosing the exchange term Ψ as a residual would imply that uncertainties in the isopycnal and diapycnal transformation terms will propagate into this estimate. In our implementation, however, Ψ is not computed after the fact from a difference, but is solved simultaneously with U_σ and U_τ in the linear inverse system $dV/dt=Ax$, where $x=(U_\sigma,U_\tau,\Psi)$. This means that all three components are constrained jointly by the σ-τ volume tendencies and by the spatial coherence imposed by the least-squares solution, rather than Ψ acting as a simple residual/noise .

Two facts give us confidence that the separation between mixing and exchange is reasonably robust:

1) Small residuals of the inverse problem: Order of 10^{-5} for both the glider and climatological datasets, indicating that Ψ is not dominated by numerical noise or large unresolved imbalances; and

2) Consistent spatial and dynamical structure: The largest Ψ values occur where we independently expect strong advection: in the eddy case, the exchange term peaks within the mode-water density range and co-locates with strong ADT anomalies, enhanced EKE, and intensified isopycnal/diapycnal transformations. In contrast, during non-eddy conditions, Ψ is much weaker and comparable to the climatological mean. The eddy vs. non-eddy differences in Ψ are larger than the typical spread associated with our inversion residuals, suggesting that the relative magnitudes we report are robust even if the absolute values of Ψ carry some uncertainty.

We now explicitly state in the detailed Supplementary Information that Ψ should be interpreted as an *effective* advective exchange term that may also include any small residual imbalance between mixing and volume tendency: *"To note, because the exchange term Ψ is obtained as part of the least-squares solution to [5] rather than by a simple difference of diagnosed terms, it is constrained jointly with Uσ and Uт. We therefore interpret Ψ as an effective advective exchange term."*

Line 365: Consider rewording/expanding to include a more general term, such as tracers. Maybe adopt the wording from line 398-399.

Thank you for this suggestion. We agree that the sentence can be broadened to reflect that eddy-driven transformations influence not only oxygen but also a wider suite of tracers. We have reworded the sentence to adopt terminology consistent with Lines 398-399 (now L426) and to generalize the statement beyond oxygen alone.

L391: *"'Mode waters play a crucial role as pathways connecting the surface and subsurface ocean, influencing the distribution and transport of tracers, including heat, salt, oxygen, and other biogeochemical properties."*

Technical Comments

Line 84: I would expect a 6 km running mean to filter out submesoscale variability. With the Rossby radius of deformation at this latitude (O) 20 km, can you comment on whether this reduces your ability to resolve the lower end of the mesoscale as well?

Thank you for raising this point. Our intention with the 6 km running mean was specifically to suppress submesoscale variability and small-scale noise, rather than to fully resolve the smallest mesoscale features.

A 6 km window strongly dampens structures with horizontal scales $\lesssim$ O(5-10 km), i.e. submesoscale variability that we do not aim to interpret with the present framework. Mesoscale features with scales comparable to or larger than the Rossby radius ($\gtrsim$ 20 km) remain resolved by several independent grid points after smoothing. In other words, the lower end of the mesoscale band is somewhat smoothed but not removed, and the eddy signatures we analyze (radius and deformation scales well above 20 km) are still clearly captured in the glider sections and in the derived transformation fields. Our conclusions regarding enhanced transformation during eddy passages are therefore based on features that are comfortably larger than the effective smoothing scale.

We have added a brief clarification in the Methods to state that the 6 km running mean is chosen as a compromise: it removes submesoscale variability and sampling noise while retaining the mesoscale structures that are the focus of this study.

L84: "The 6 km running mean is selected as a compromise between filtering out submesoscale variability (≲ O(10 km)) and preserving mesoscale structures (≳ O(20 km), comparable to the local Rossby radius)."

Line 87: An equation would be nice for both EKE calculations.

Thank you for the suggestion. We have expanded on the definition. Now it reads as:

L89-94: "Eddy kinetic energy (EKE) was estimated from two independent sources: (1) the depth-averaged currents (DAC) derived from the glider flight model as $EKE = (DAC - \underline{DAC})^2$, where $\underline{DAC}$ is the mean DAC during the glider campaign (Frajka-Williams et al., 2011); and (2) from sea surface height-derived surface geostrophic velocities from satellite observations. EKE is defined as $u'^2+v'^2$, where $u' = u - \underline{u}$ and $v' = v - \underline{v}$ are respectively the zonal and meridional velocity anomalies, where u and v are the surface geostrophic velocities on the glider transect, and $\underline{u}$ and $\underline{v}$ are the mean surface geostrophic velocities during the glider-sampling period. "

Line 88: Can you elaborate/clarify on what you mean re: "anomalies of the dive-averaged currents derived from the glider flight model"? Anomaly relative to what?

We have removed "the anomaly of…" as adds confusion and now is explicitly stated in form of equation how we derive EKE from DAC. We have modified the text in L94 to "*(1) the depth-averaged currents (DAC)...".*

Line 100: A 200 km buffer from the across-Gulf transect for remapping seems very wide.

We agree that 200 km may appear large relative to the width of the transect itself. Our rationale is that the goal of the Argo-based climatology is *not* to reproduce finescale cross-shelf structure, but to obtain a robust monthly mean thermohaline structure representative of the broader Sea of Oman. Argo coverage in this region is sparse and unevenly distributed (as shown in Figure 1a), and a narrower buffer (e.g., 50-100 km) results in strong spatial aliasing and insufficient profile density for several months. The 200 km radius ensures adequate sampling density across all months of the climatology while still being small enough to avoid drawing profiles from outside the Sea of Oman basin. Thus, this radius is chosen to maximize sampling coverage while retaining basin-level representativeness rather than local fidelity

To note: 1) The profiles are first remapped onto the across-Gulf transect using median binning, which strongly limits the influence of outliers or spatial inhomogeneities. 2) The climatology is then binned horizontally at 3 km, which removes residual small-scale variability and ensures that basin-scale gradients (rather than local anomalies from individual Argo floats) dominate the mapped fields. 3) Our intention is not to interpret cross-shelf signals from the climatology, but to compare large-scale, monthly transformation tendencies with the high-resolution glider data.

We have modified the description of how we produce the climatology (in response to Editor Comment) and added a clarification in the Methods to explain more explicitly why this threshold of 200km is used in response to your comment.

L102: *"…Argo profiles within a 200 km distance from the across-Sea of Oman transect (Figure 1a, orange dashed line) were selected. This strategy ensures sufficient monthly sampling coverage in this sparsely observed region to construct an across-gulf monthly climatology…"*

Line 122: The spice coordinate captures the isopycnal change more so than using potential density as a coordinate, no? Consider rewording this sentence.

Thank you, we agree. We have removed the "isopycnal nature of mechanisms" so now it only refers to the diapycnal mechanisms (L132).

Line 190: I am a little confused at how the thinning of mode waters results in a densification, but the signs of both the isopycnal and diapycnal transformation are negative (Line 192-193), implying a reduction in density.

We recognize that the relationship between thinning, mintification, and densification may not have been sufficiently clear. The key point is that the instantaneous signs of the isopycnal and diapycnal transformations (negative values) refer to net mixing tendencies at the boundaries of the σ-τ classes, not the depth-mean changes of σ and τ within the mode-water layer. In the climatological analysis, the integrated tendency over the full April-June period produces: net mintification ($\Delta\tau < 0$), and net densification ($\Delta\sigma > 0$), as shown in Fig. 2e (now 3e). By contrast, the negative isopycnal and diapycnal transformation values in Fig. 2b-c (now 3a,c) indicate loss of volume from the lighter/spicier edges of the mode-water layer, not that the whole layer is becoming lighter. Because mode water is simultaneously thinning, this preferential loss of lighter/spicier classes results in a net shift of the remaining volume toward slightly denser classes, even though the instantaneous transformations point toward lighter isopycnals.

In other words: Transformation signs describe the direction of water-mass fluxes into/out of σ-τ bins, whereas $\Delta\sigma$ and $\Delta\tau$ describe the trajectory of the remaining volume-weighted mean properties. We have reorganized that paragraph and clarified this in the text to avoid confusion.

L211: *"...On average, the isopycnal transformation dominates (-0.015 ± 0.009 m2 s-1), with a magnitude approximately three times larger than the diapycnal transformation (-0.005 ± 0.008 m2 s-1) (Figure 3a, c, and f). The signs of the transformation terms reflect the direction of the fluxes between density and spices classes, whereas the net Δσ and Δτ reflect the evolution of the volume-weighted mean properties.… "*

Timescale of transformation of mode water section: I found the inclusion of both the climatological analysis and the higher resolution glider data on the same plots in Figure 2 to be a lot to unpack. Similarly, while I think the title of this section is a nice description, the first paragraph could use some additional language to highlight the time period it refers to. Similar to how the second paragraph highlights how the glider data allows for a higher resolution analysis. I don't necessarily think the two paragraphs warrant their own section, but the differences in the findings are noteworthy enough to warrant additional descriptive text, at a minimum. Initially, I was going to suggest breaking Figure 2 up into two different figures, but I do find the comparison to the climatology to be helpful. The additional text in the results section will likely make the figure more digestible.

Thank you for this thoughtful comment. We agree that Figure 2 (now Figure 3) contains a large amount of information, and that the contrast between climatological and high-resolution glider transformations merits clearer framing in the text. We chose to keep both datasets in a single figure because the side-by-side comparison is central to demonstrating how temporal averaging shapes the interpretation of mode-water transformation. However, we have revised the text to better guide the reader through the distinct timescales represented and to clarify that the first paragraph refers specifically to the *monthly climatological* perspective, while the second paragraph addresses the *intraseasonal* variability resolved by the glider.

To improve clarity, we have added explicit transitions outlining: (1) the temporal window and resolution addressed in each paragraph, (2) why the climatological and glider analyses differ. This additional contextual text helps make Figure 2 more digestible without requiring a split into two separate figures.

L202: "We examine the seasonal-scale evolution of mode-water properties and transformations using the monthly Argo climatology (April–June). This climatological view captures the broad, low-frequency changes as the mode water evolves through late spring and early summer, but necessarily smooths over shorter-term variability..";

L221: "In contrast to the climatological perspective, the high-resolution glider time series resolves submonthly variability and therefore reveals the episodic, intraseasonal changes in mode water structure and transformations that are absent from the monthly climatology. This allows us to directly quantify short-lived events associated with transient processes, such as mesoscale eddies."

Line 368: While I understand a large aspect of the importance of Arabian Sea mode waters is their influence on subsurface oxygen concentration, I find the discussion around your results in the context of prior oxygen-focused literature to be too direct, as it does not actually utilize any oxygen data in your analysis. Simply, the last sentence of the first paragraph in the Discussion section can either be reworded or expanded to better reflect which aspect of Jutras et al. (2025)'s study your results expand. Then, more explicitly, how one might infer the resulting changes/implications in oxygen concentration from your findings. It is clear how your findings are focused on shorter timescale changes in mode waters, but I find this important paragraph in need of larger clarification.

We agree that our original wording placed too strong an emphasis on oxygen given that our analysis does not explicitly use oxygen observations. Our intention was to situate the physical transformation processes we diagnose within the broader biogeochemical context established by Jutras et al. (2025), who quantified how mixing along mode-water ventilation pathways shapes long-term oxygen changes. We have therefore reworded and expanded this part of the Discussion to (i) clarify exactly how our results complement Jutras et al. (2025), and (ii) more explicitly outline how changes in physical transformation at short timescales could affect oxygen without overstating what we quantify directly.

L392: "For instance, in the Arabian Sea, mode waters bound the upper oxycline of the Arabian Sea oxygen minimum zone, thus playing a central role in shaping regional oxygen distributions (Font et al., 2025). As shown by Jutras et al. (2025), at long-term and large spatial scales, more than 50% of oxygen changes along mode water ventilation pathways can be attributed to mixing with surrounding oxygen-poorer waters. While we do not diagnose oxygen directly, our results extend this understanding by showing that the physical drivers of such mixing are strongly intensified at short timescales during eddy activity. These episodic but vigorous transformation events likely modulate ventilation efficiency and tracer redistribution within mode waters."