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

RC3

This study employs glider transect observations from the Sea of Oman to quantify diapycnal and isopycnal mixing within the mode water layer across multiple timescales. The authors compare these estimates with monthly climatologies derived from Argo float data, demonstrating the limitations of coarser temporal resolution in capturing mixing variability. Their analysis underscores the need for additional glider-based field campaigns and direct turbulence measurements. The higher-resolution glider observations provide a more complete characterization of mixing processes under both eddy and non-eddy conditions, revealing that 40-60% of transformation variability is obscured by climatological averaging.

The novelty of this work lies in its use of high-frequency observational data to estimate mode water transformation rates, offering an observational perspective that complements traditional climatological approaches. I recommend this manuscript for publication. It presents a rigorous and well-articulated analysis that advances understanding of mode water transformation processes and remains accessible to readers less familiar with water mass transformation frameworks.

L44: What is the volume, what is the residence time? Provide relevant values for quick mental reference for readers.

Thank you. We provide a range of MW thickness and residence time from Font et al., 2025:

L43: *"...(thickness > 50 m and residence time > 4 months; Font et al., 2025),..."*

L48-50: What does respiration within the water mass mean? Can you be more explicit; for ex., does respiration in this context mean physical transport of the MW? If so, how does transport within the WM lead to oxygen changes if the WM is defined to be a homogenous parcel?

We apologise for the confusion. We meant "*net community respiration*", i.e., biological consumption, not physical transport). We have clarified in the text L48.

L53-55: If you can't provide explicit values for volume and residence time, then in that first sentence maybe quickly mention the poorly constrained nature of that info.

Thank you. Font et al., 2025 provide explicit estimates of mode-water volume and residence time. Yet, the interannual and sub-monthly variability of these quantities due to limited high-resolution observations remains unconstrained. We have rephrased to be explicit:

L51: *"Understanding the physical processes that mix and stratify mode water across scales, and how these processes interact with biogeochemical dynamics, is critical. While Font et al. (2025) provide valuable estimates of mode-water volume and residence time, the subseasonal and small-scale variability that governs changes in its properties remains poorly constrained, largely because observations at the necessary spatiotemporal resolution are still scarce."*

L60: Can you elaborate on why you chose potential density as your coordinate? I don't know what "natural" means here. You explain spice well in L63, please apply this level of explanation for why you chose sigma as your other coordinate - why not neutral density?

Thank you for pointing this out. We agree that the term "natural" was not sufficiently clear.

Choosing a density-spiciness framework we are able to address isopycnal and diapycnal transformations, and to identify changes of water masses spreading along isopycnals which would be otherwise hidden. Potential density is used because it is materially conserved under adiabatic and isohaline motions and therefore provides a practical approximation to isopycnal surfaces in the upper ocean. Neutral density is more exact in theory, but is not well defined near the surface and requires full 3-D fields; even though 2D approximations can be made. On the spatial and density scales of this study (upper 200 m, O(0.1 kg m^{-3}) density range), thermobaricity (the pressure-dependence of the thermal expansion coefficient that causes potential-density surfaces to diverge from true neutral surfaces) is weak, so potential and neutral density differ only minimally and would not alter the transformation results.

We have now clarified in the text L60: "*Potential density is used because it is materially conserved under adiabatic and isohaline motions and therefore provides a practical approximation to separate isopycnal and diapycnal fluxes, while spice is...*"

L69: Strong motivation for this paper!

Thank you.

L73: Great list of questions, organized!

Thank you again!

L82: typo: "at"

Changed. Thank you.

Fig 1a: Grey dashed line that float data are projected onto...does that mean farthest points on the map are also projected onto that line? The farther ends have little to no float data, and some cross shelf from 100-1000m, is it representative of the space (in x,y and z) MW would expect to occupy?

Thank you for raising this point. The orange dashed line is used solely as the reference transect onto which Argo profiles are horizontally projected to build the across-Gulf climatology. We acknowledge that some offshore points lie far from the line, but two factors ensure that the projected climatology remains representative of the water masses sampled by the glider (See response to RC1-L100, and Editor Comment).

We restrict Argo profiles to those deeper than 1000 m and within 200 km of the transect, which eliminates shallow shelf profiles and limits contributions to deep offshore profiles that are representative of the open Sea of Oman. The resulting climatology represents a large-scale, basin-averaged cross-section rather than a precise reproduction of the glider geometry, which is exactly what we require for comparison to seasonal-scale Argo-based transformations.

We have added a short clarification in the manuscript regarding your point in the RC1 and Editor Comment. :

L101: *"...Argo float coverage spans the entire domain, with an average density of 28 profiles per 0.25° × 0.25° grid cell (Figure 1a) and a relatively uniform monthly distribution (Figure*

1c). 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. Moreover, to avoid the influence of shallow profiles on the continental shelf, profiles shallower than 1000 m were excluded, so the transect remains representative of the environment where mode water forms and persists. Each profile was then orthogonally projected onto the nearest point along the transect (the orange dashed line in Figure 1a), which provides its along-transect coordinate. Profiles were vertically interpolated onto a uniform 2-m pressure grid, and all projected profiles were median-binned into 3-km horizontal bins along the transect. Averaging was performed on pressure levels. Monthly climatologies were produced by taking the median across all profiles within each (depth, distance) bin for each month between 2000 and 2023. This gridded product is then used as input for the σ-τ water-mass transformation calculations."

Fig 1d: stability of upper 100m highly variant esp during the transition from spring to summer. Flips sign sometime in Feb/Mar. What determines the shape of that seasonal spiral (when tracking the vertices of the thermohaline stability)?

Thank you for this interesting question. The spiral-like shape in Fig. 1d arises directly from the seasonal evolution of upper-ocean stratification (N^2) during winter mixing and subsequent restratification. The key drivers are winter convective mixing (Jan-Feb) (strong surface cooling and wind-driven mixing homogenize the upper layer), spring restratification (late Feb-March) (as surface heating begins), onset of strong thermal stratification (March-June; Surface warming dominates), and capping of mode waters beneath this strong surface stratification. This has been described in Font et al. 2022 and Font et al. 2025.

Fig1e: Grey lines are very hard to discern, please consider a different color that will pop out from the noisy background (cyan?). Can you tell from this view of the chances the MW will be subducted or mixed back up into the surface? Perhaps the time of year indicates the likelihood skewed towards mixing with deeper water masses?

Thank you. We have removed the density contours in grey to simplify the figure and have applied the changes in Figure 1 that RC1 suggested (See RC1 and Fig1). This panel primarily illustrates vertical displacements and eddy-driven modulation of the capped layer, rather than active subduction or re-entrainment. However, it is possible to infer that when the mode water layer is well beneath the MLD, and that the stratification increases, the likelihood of being re-entrained into the mixed layer is low. This occurs progressively from the end of March (capping). By the end of April the seasonal thermocline is already established, the stratification has substantially increased, and the observed variability is dominated by mesoscale-induced variability.

L122: Can you say the information succinctly instead of saying the rest of this sentence?

We have modified the statement (L131-134) in response to RC1-L122 and EC-L85. Further details provided in the respective responses to RC1 and EC.

L129: why formation and not transformation? it is the sum of formation and destruction (i.e. transformation). Later in L131 you say it's the convergence/divergence represented by the sum, so to also consider destruction/divergence it is more apt to say transformation.

Thank you for the clarification. We agree that “transformation” is the more accurate term here, since $\sum U(\sigma,\tau)$ represents the net effect of both formation and destruction (i.e., convergence and divergence) within a σ-τ class. We have replaced “formation” with "transformation" to maintain consistency with our definitions and with the wording used later in the section (L138).

L134: What about the southeast part?

Thank you for raising this point. In our formulation, we allow exchange only through the northern boundary of the transect. The southern end lies close to the continental shelf, where flow is topographically constrained and cross-shelf exchange is expected to be strongly limited. For this reason, and because the northern boundary sits in the open interior of the Sea of Oman, we assume that the majority of through-section exchange occurs there. We now state this explicitly as a caveat in the manuscript.

L144: *"The southern end is shelf-constrained, cross-section exchange there is assumed to be negligible. "*

L138: good summary statement

Thank you.

Eq1.1: (looking for clarification here) Sum of (sigma bins x cumulative product of spice bins x sigma velocity)?

Thank you for pointing this out. We realize the notation in Eq. 1.1 (now 2.1-2.2) may give the impression of a cumulative product of σ and τ bins, which is not the case. The expression defines the flux across a σ-surface, integrated over the area of all grid cells that fall within a given σ-τ class. We had a mistake in the notation (the equality was in the denominator of the integral), which we have fixed for eq. 2.1 and 2.2, which now read as:

$U_\sigma(\sigma,\tau) = \int_{\sigma'=\sigma} \Pi(\tau,\tau') \cdot u_\sigma \cdot dA$ and $U_\tau(\sigma,\tau) = \int_{\tau'=\tau} \Pi(\sigma,\sigma') \cdot u_\tau \cdot dA$

We have also explicitly stated the definition of the Π indicator (eq. 3).

Moreover, we have added a detailed description of the method in the Supplementary Information following the suggestion of RC2.

L151: what assumptions?

The phrase "following our assumptions" referred to our definition of spice, but we agree with the reviewer that is confusing and not explanatory. We have clarified the definition of spice explicitly in the manuscript following Editor Comment L85 but also expanded in the definition of the terms and the methods of the water mass transformation description in the Supplementary information. We have then removed "following our assumptions" and just left "diaspice".

L162: Do you mean that you used ERA5 temp/salt data to find isotherms/isohalines that outcropped and used those values to identify the classes on your sigma-tau plot? If so, can you say that to be clear?

Thank you for the opportunity to clarify this. We did not use ERA5 temperature and salinity to identify outcropping classes directly. Instead, we applied the water-mass transformation (WMT) method in T-S space including transformation via air-sea fluxes. We used T-S observations from gliders and Argo, and ERA5 air-sea buoyancy fluxes, to determine which T-S classes experience surface buoyancy-driven transformation (following Evans et al., 2014, 2023). We then converted those transformed T-S classes that are affected by surface buoyancy-driven transformation into σ-τ space and compared them with the σ-τ domain of the mode water. As shown in Fig. S2, the σ-τ classes affected by buoyancy fluxes lie well above the mode-water σ-τ range, confirming that air-sea fluxes do not directly influence the mode-water classes during our analysis period. We have modified the manuscript to explain this more clearly:

L175: "*To assess the role of surface forcing, we applied the water mass transformation framework in temperature-salinity (T-S) space (Evans et al., 2014; 2023). Using glider and Argo T-S observations, together with ERA5 air-sea buoyancy fluxes (Hersbach et al., 2020), we diagnosed the surface transformation of distinct T-S classes, thereby identifying which classes are actively transformed by air-sea fluxes. These classes were mapped into σ-τ space and compared with the σ-τ domain of the mode water (Figure S2), showing no overlap. The classes influenced by surface fluxes lie well above the density range of the mode water (Figure S2), indicating that surface buoyancy forcing does not influence the observed mode water transformations*"

L184: There are no panels for Figure 1 (f) and (g).

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

L185: Reference Figure 2 in this sentence.

Added.

L195: cite please

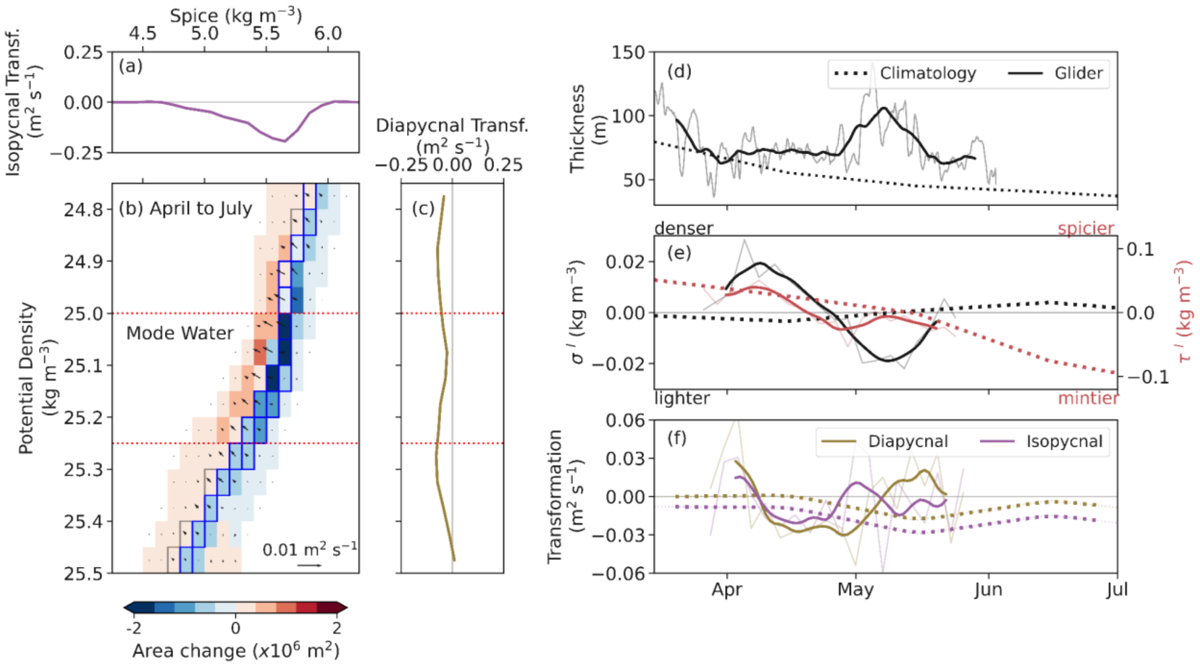
"The previous analysis..." does not referee to an independent study. To remove the ambiguity, we start the sentence as "Over shorter timescales, the..." in L224.

L200: typos - 2f; "transf."

We have changed all of the "tranf" to "transf."

Fig2: (a) and (b) order should be switched in this figure along with the corresponding changes in text. **Fig2e**: "Denser" "lighter" should be inside the panel, it is visually busy/confusing the way it is currently placed. Same for "spicier" "mintier" **Fig 2f**: Make sure the colors chosen for the lines are accessible to readers with color vision deficiencies

Thank you. We have applied the changes you suggested in Figure 2 (now Fig 3). We haven't put the denser and lighter inside the panel, but put them closer so they don't feel that visually detached from panel e. We changed the line colors of figure f and accordingly changed the rest of the figures where diapycnal and isopycnal transformations are plotted.



L220: Can you explain why you integrated over spice class for isopycnal transformation and potential density for diapycnal transformation? This goes back to my comment in L60

Thank you for this comment. In the σ-τ framework, isopycnal and diapycnal transformations represent fluxes across τ and σ surfaces, respectively. Isopycnal transformation ($U\tau$) quantifies mixing along density surfaces, i.e., the redistribution of water masses within the same σ but across different τ. Because this mechanism acts horizontally in density-spiciness space, the natural way to express a bulk transformation is to integrate over τ within the mode water density band. Diapycnal transformation ($U\sigma$) quantifies mixing across density surfaces, i.e., vertical exchanges that move water into lighter or denser σ classes. This mechanism acts vertically in density-spiciness space, so the appropriate bulk representation is an integral over σ within the mode water τ range. This approach follows the standard interpretation of σ-τ transformations described in Evans et al. (2014) and Portela et al. (2020b), where integrating along the “inactive” coordinate isolates the component of the transformation driven by fluxes across the “active” coordinate. It also ensures that the resulting integrated values reflect the net tendency acting within the full mode water layer, rather than focusing on any single σ-τ bin.

L234-241: Very cool calculation to justify high sampling frequency!

Thank you.

L246: Figure 1a used gray for argo climatology and orange for glider - i suggest flipping the colors here (or in fig 1) to be consistent with the colors representing which dataset

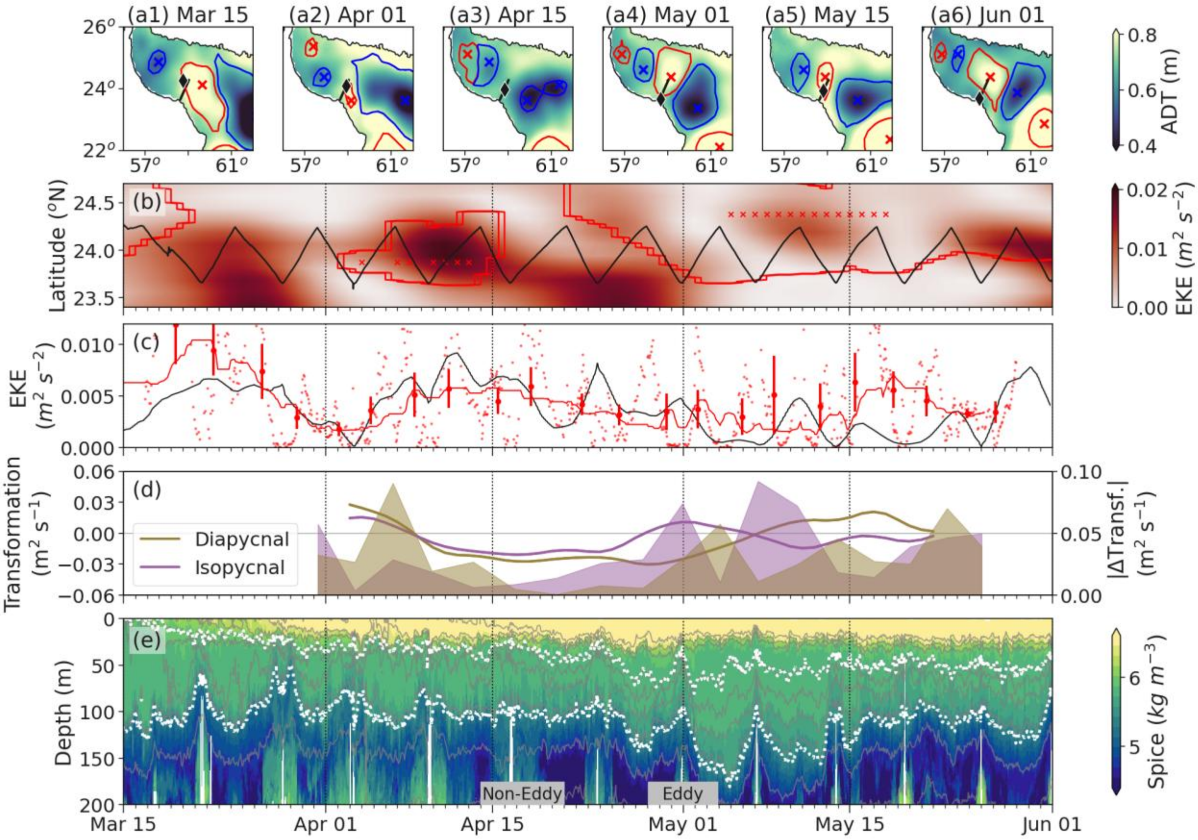
Thank you. We have applied the changes you suggested in Figure 1 (orange for climatology and black for glider). See RC1 - Fig1.

L283: spell it out since this is the first mentioning in captions

Done.

L284: The small yellow diamond is hard to see, can you choose a different color (like, cyan or hot pink).

Thank you. We have changed the color of the diamond to black with a white edge for consistency with Figure 1 and 4 (previous Fig 3). Moreover, we removed the MLD for simplicity and changed the color and linestyle of the MW boundaries following Figure 1 to white dotted. Finally, we changed the color of the transformations to be consistent and colorblind friendly. We have changed the figure caption and the text accordingly.



L285: typo

Corrected.

L295: Paragraph explanation of fig 5 should come before the referencing of fig 5. Before L278.

Thank you for the suggestion. To improve clarity and maintain a consistent narrative flow, we removed the early reference to Figure 5 rather than relocating it.

Fig6: Perhaps this figure would be helpful to the reader before the other figs. consider placing this schematic as your fig 1 or 2.

Thank you for the suggestion. We have moved Fig. 6 to Fig. 2 and accordingly edited the text and all figure numbers. We used it in the description in Section 2.2:

L 147: *“A diagram illustrating how changes in water characteristics are represented in σ-τ space is shown in Figure 2a. The processes that modify the volume of a σ-τ class are depicted in geographical coordinates in Figures 2b-c. The σ-τ class highlighted with a square in Figure 1a is marked with dots in Figures 2b-c.“*