We thank the editor 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 Editor 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

EC

Dear authors,

The three reviewers were very positive. They provide several points for the authors to consider in their revised manuscript. In addition, I added my own comments below after reading of the manuscript. I'm also positive about this manuscript. I encourage the authors to consider all the comments and provide a point by point response to authors comments and revised manuscript to Ocean Sciences.

L85 - It is written as Conservative Temperature and Absolute Salinity. Both are capitalized. Please also refer to the right papers that define these variables. For Conservative Temperature this is McDougall 2003, Graham and McDougall2013. For Absolute Salinity McDougall et al 2011.

- McDougall, T. J.: Potential Enthalpy: A Conservative Oceanic Variable for Evaluating Heat Content and Heat Fluxes., Journal of Physical Oceanography, 33, 945-963, https://doi.org/10.1175/1520-0485(2003)033<0945:PEACOV>2.0.CO;2, 2003.
- Graham, F. S. and McDougall, T. J.: Quantifying the Nonconservative Production of Conservative Temperature, Potential Temperature, and Entropy., Journal of Physical Oceanography, 43, 838-862, https://doi.org/10.1175/JPO-D-11-0188.1, 2013.
- McDougall, T. J., Jackett, D. R., Millero, F. J., Pawlowicz, R., and Barker, P. M.: A global algorithm for estimating Absolute Salinity., Ocean Science, 8, 1117-1128, 2012.

Thank you, we fixed the capital letters and also cited these references.

L85 - Please provide a definition/equation for spice.

We use the definition from McDougall and Krzysik (2015), and we implemented it through the TEOS-10 routine *spiciness0*. The text in the manuscript has been modified to explicitly reference the function on the TEOS-10 routine:

Line 86. "Spice (τ; kg m^{-3}) was computed from Conservative Temperature (Graham and McDougall, 2013; McDougall, 2003) and Absolute Salinity (McDougall et al., 2012) following the TEOS-10 routines (gsw_spiciness0; McDougall and Barker, 2011). Potential density and spice are referenced at 0 dbar."

and in L132: "....spice is interpreted as a measure of thermohaline variability along isopycnals, reflecting isopycnally-compensated temperature and salinity changes associated with the spreading of distinct water masses (Jackett and McDougall, 1985; McDougall and Krzysik, 2015). "

L87 - Please define details about calculating EKE.

Thank you. We have defined and clarified EKE calculations. See comment L87-RC1.

L95 - The current information is not enough for proper reproducing the data. Some questions I was left with are for example: You select all Argo floats within hundreds of km, as given by fig 1a? Then you take the gray line as center and bin selected Argo floats and take the median. But this selection, is this from a circle with a radius from the center? Or is this some lines perpendicular to the transect? Is this averaged on pressure surfaces or on isopycnal surfaces, and what are the consequences of this choice. What does the 3km horizontal scale have to do with this, as it is projected on a line?

We thank the Editor for highlighting the need for clearer methodological detail. We have now substantially expanded the description of how Argo profiles were selected, projected, and remapped onto the across-Gulf transect following your suggestion and RC1 and RC3. The 3 km horizontal grid is to have the same grid for all months to be able to perform the WMT analysis. The revised text appears in Section 2.1.

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."*

L155 - The description requires a bit more explanation. For example, what are the coefficients in A? They can't be derived from the given information here. Perhaps provide a short appendix that the reader can refer to for a summary of the method. How sensitive are your results to choices within the inverse machinery (see Groeskamp et al 2017 as an example).

- Groeskamp, S., Sloyan, B. M., Zika, J. D., and McDougall, T. J.: Mixing Inferred from an Ocean Climatology and Surface Fluxes, Journal of Physical Oceanography, 47, 667-687, https://doi.org/10.1175/JPO-D-16-0125.1, 2017.

Thank you for this suggestion. We agree that the method needs more description (as also suggested by Reviewer 2). We thus have added an extended explanation addressing these concerns and provided a step-by-step description in the Supplementary Information. Please see RC2 - WMTF description.

We are currently uncertain about how best to apply the sensitivity of the inverse framework used in our water mass transformation (WMT) analysis. Our approach uses a simplified formulation of the WMT framework compared to Groeskamp et al. (2017), in which we include only mixing contributions to the transformation. Specifically, we adapted the code from Evans et al. (2014) to our dataset, which we made available on GitHub. This implementation constructs the matrix A and solves the linear system using a least-squares approach, without applying additional weighting or preconditioning. We are therefore unclear on how to adapt this simplified inverse setup to directly address the sensitivity issue raised in your comment. We feel that the manuscript is sufficient without including a sensitivity analysis at this point but we are open to discussing the issue further if you could provide further clarification on an approach applicable to our implementation.

L170 - How do you define a surface area from a transect? This surface area is needed in eq 1.1 and 1.2.

A hydrographic transect is 2-D (distance x depth). Each measurement can be assigned to a σ-τ class. For each class, you look at the portion of the transect cross-section occupied by water in that class. This gives you an area in units of m^2, not m^3 or m^2 per meter of width. That area is what enters your isopycnal/isospice integrals.

We have rephrased the definition in L159: *"...and dA is the cross-sectional area of the transect occupied by water within the specified σ-τ class. .."* and added details on the calculation in the Supplementary Information 1 following Evans et al., 2014.

Section 3.1 and figure 2: Spice is not defined. WMT is in units of m2/s which is a flux. It is unclear from the text or equations how these units come to be and why it is not kg/s or m3/s.

Following your previous comment, now Spice has been defined. There is also an explanation of the units of the transformation fluxes in the new detailed Supplementary Material: *"These dia-surface transformations should be interpreted as volume fluxes of water and have units of m3 s-1. They cannot be practically diagnosed from velocity measurements and must therefore be determined indirectly from changes in the volumetric distribution of water projected into σ-τ coordinates. In the case of a two-dimensional ocean transect, as per this study, the method is identical; however, the inferred transformations are area fluxes and have units of m2 s-1."*

Fig 2e shows potential density anomaly. Please clarify the following things: 1) Sigma is already a symbol for potential density anomaly (1000 is subtracted, see TEOS-10). 2) please provide equations defining spice anomaly and potential density anomaly. Please clarify this statement to make it clear how anomalies are calculated: "the glider after applying a 10-day rolling mean (solid), and the climatology (dotted)

We apologies for the confusion. In this study, σ refers to potential density ($\sigma = \rho - 1000$ kg m^{-3}), following TEOS-10 conventions. To avoid confusion, we explicitly define potential-density and spice temporal anomalies as: $\sigma'(t)=\sigma(t)-\bar{\sigma}$ and $\tau'(t)=\tau(t)-\bar{\tau}$ where $\bar{\sigma}$ and $\bar{\tau}$ are the time-means computed over the analysis period (mid-March to July). These temporal anomalies quantify deviations from the mean state of the mode water layer.

We have changed the description in the caption of Figure 2e (now 3e) accordingly to clarify: *"Temporal anomalies of potential density (σ') and spice (τ') computed as deviations from their time-mean over the March–July period as $\sigma'(t)=\sigma(t)-\sigma$ and $\tau'(t)=\tau(t)-\tau$ where σ and τ are the time-means computed over the analysis period. Solid light lines show the 3-day glider anomalies; solid dark lines show the glider anomalies after applying a 10-day rolling mean; dotted lines show the monthly climatological anomalies. "*

How does cabbeling and thermobaricity affect density changes in this method?

Cabbeling and thermobaricity are accounted for implicitly. Because all σ-τ calculations use TEOS-10 CT, SA, and σ_0, the nonlinear equation-of-state effects that arise from cabbeling and thermobaricity are already resolved in the density tendencies. The WMT framework interprets any resulting cross-density-surface flux as part of the diapycnal transformation.

Thermobaric effects are expected to be minimal in our domain (upper 500 m, σ_0), but cabbeling may contribute to the diagnosed diapycnal term; however, the method does not allow isolating this from turbulent diffusivity.

L226-245. This is an interesting analysis and relevant. However, I have a lot of trouble understanding exactly what is done, from the explanation given. Its probabaly all there, but I would encourage the authors to find a clearer way of explaining and presenting these results.

Thank you for this comment. We acknowledge that this section presents a more technical analysis, and we carefully re-evaluated the text for clarity. We have opted not to restructure the section but have made several targeted edits to improve readability:

L262: "*..This approach provides a distribution of possible means for each effective sampling resolution. To illustrate how smoothing influences variability, we also applied rolling means of increasing window length to the 3-day series (shown as violin plots in Figure 4). As the window size increases, extreme values in both isopycnal and diapycnal transformation are progressively damped (Figure 4).".*

Moreover, we changed the previously defined *"true mean"* as *"3-day mean" as it is more explicit and clear (following RC2).*

Please repeat the meaning of ADT in caption of figure 4

Done.

L375 - in this paragraph, it is said that climatological WMT will miss peaks. However, to what extend is this related to the method applied here? This is not a statement that can easily be broadened to all WMT as these rely on different approached to do the actual calculations. Maybe adapt this paragraph to the specific method used. Be careful or specific about the broader statements.

The comment refers to applications to climatological data which by nature of its delta(time) cannot resolve sub-monthly variability and will instead present a time-integrated result. This independent of WMTF method choice. What we meant was that monthly averages will miss higher frequency variability, therefore we have modified the text and referenced the figures supporting our statements:

L405: "As a result, climatological approaches miss sub-monthly variability and underestimate both the intensity and variability of transformation processes (Figure 4), particularly the contributions from isopycnal stirring and advective exchange (Figure 6)."

L390 - How would microstructure provide lateral mixing estimates?

We thank the reviewer for putting attention into this statement. The original phrase was *"..Including turbulence measurements, such as microstructure-derived diffusivities, would help disentangle the relative roles of vertical mixing, lateral stirring, and advection...".* We agree that it was confusing as per the enumeration of processes after microstructure observations. With this sentence we wanted to emphasize that the WMTF provides an indirect measurement of diapycnal mixing, but this vertical mixing could be highly influenced by transient processes like lateral intrusions that could enhance/supress gradients, and due to the intrinsic way to compute the WMTF we can not capture this variability. Moreover, this region is characterized by double diffusive convection instability (Font et al., 2024) and has been shown that this process can enhance more than 50% the local diapycnal mixing diffusivity (Fischer et al 2013; Pinto-Juica et al. *accepted for Nat. Comms. E&E*), playing an important role in the oxygen redistribution below the MLD. Therefore, we want to be explicit that there is an important contribution that we can not capture due to methodological constraints, but we know it's important. We have rephrased the statement to remove the ambiguity to:

L417: "...Including turbulence measurements, such as microstructure-derived diffusivities to resolve the role of vertical mixing, could allow for disentangling those from lateral stirring, and advection, and thus refine the interpretation of transformation processes in dynamic regions like the Sea of Oman."

Fischer, T., Banyte, D., Brandt, P., Dengler, M., Krahmann, G., Tanhua, T., and Visbeck, M.: Diapycnal oxygen supply to the tropical North Atlantic oxygen minimum zone, Biogeosciences, 10, 5079–5093, https://doi.org/10.5194/bg-10-5079-2013, 2013.

Juica, M., Pizarro, O, Santana, A., Valencia, L., Ulloa, O., Figueroa, P., Ramos, M. & Queste, B.: Salt Fingers Contribute Substantially to Diapycnal Oxygen Transport into the Oxygen Minimum Zone of the Eastern South Pacific. 10.21203/rs.3.rs-7151709/v1. (2025 - accepted).

I think it would help the manuscript if Figure 6 becomes figure 1.

Thank you for the suggestion. We have moved Fig. 6 to Fig. 2 and accordingly edited the text (see RC3).