Author response to comments on “On Mode Water formation and erosion in the Arabian Sea: Forcing mechanisms, regionality, and seasonality" by Font et al., (Preprint egusphere-2025-468)

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 certain sections.

We respond to all issues addressed in their comments below, as well as we add the revised changes in the manuscript. The Reviewer comments are included here in black, and the answers below the 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 new manuscript version after the suggested changes.

Referee #3

Local Mode Water dynamics are critical for understanding the physical oceanography (and thus the biogeochemical oceanography) of the Arabian Sea. The understanding of these dynamics contributed by the manuscript is valuable for understanding ventilation of the Arabian Sea OMZ, and will help in evaluating future ocean and climate model representations of Arabian Sea ventilation. The manuscript highlights significant and bimodal contributions of the southern Arabian Sea to MW formation, when previous literature has focused on northern winter MW formation.

The analysis performed in this study is thorough, and draws upon the relevant physical frameworks for analyzing changes in stratification related to Mode Water Formation. The use of both observations and models is a strength of the manuscript. I believe that the comparison of the 1D and 3D model to infer the importance of horizontal dynamics is sound, though I have some questions about the role of turbidity in the comparison.

The takeaway messages are clear, but some sections could be more concise or clearly structured, and some figures more clearly introduced.

I recommend minor revisions, with specific points listed below. In the points below, I also include a one or two new pieces of analysis that I would be interested in seeing. However, the manuscript already contains sufficient results, and no new results are necessary for the manuscript to be fit for publication. Overall, I think that the manuscript makes valuable contributions to the topic.

Specific points:

Lines 135-145: To clarify, this algorithm is strictly one-dimensional, correct? So if a MWL exists at a given point, but was formed elsewhere, it would not appear in e.g. Figure 3c-n? Do you think that this may lead to cases where a MWL seems erroneously eroded, if it remains intact but advected to a location that did not have a MW formation event?

The MW detection algorithm is indeed strictly one-dimensional. However, since it is based on a density threshold, it allows for the identification of low-stratified water that has been advected from other regions. This means that while the algorithm does not explicitly track advection, MWL that has formed elsewhere and subsequently moved into a given location can still be detected, provided it meets the density criteria.

That said, we acknowledge that in cases where advection leads to MWL modification or mixing, there could be instances where the detected MWL appears thinner or more eroded than it actually is. We will clarify this point in the text to address potential misinterpretations. Lines 151-153: "*While the algorithm does not explicitly track advection, the density threshold allows for the detection of low-stratified MWL that has formed elsewhere and been advected into the region*."

Lines 144-145: In Figure 1, sometimes the MWD is above the bottom of the MWL, including in the schematic in Figure 1b. So, the physical intuition for these metrics is a bit fuzzy. Can you clarify this? How is it that the Mode Water layer can extend deeper than the deepest surface mixed layer? Is the 0.05 kg/m3 threshold universally justified, or does it break down in some regions?

We thank the reviewer for this comment and agree that further clarification is needed. The MWL can extend deeper than the deepest surface mixed layer due to the advection of denser MW formed in other regions, which can result in a thicker low-stratified layer at a given location. This is one of the reasons why we use a density threshold criterion. We clarified this point (see response to previous comment)

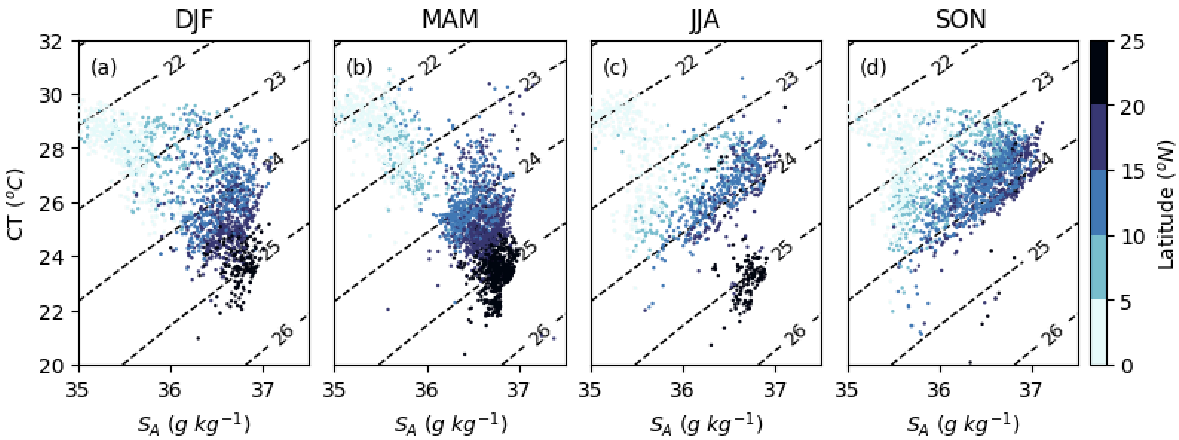
Regarding the choice of the 0.05 kg/m³ density threshold, we tested multiple criteria and found that this value provided the best balance between sensitivity to both Argo observations and model outputs, making it the most robust and universal choice across the basin. However, we acknowledge that regional variations may exist, but this threshold represents the best compromise between sensitivity and broad applicability across the basin.

Results 4.1: this section is quite hard to follow because the text jumps between Figures 2,3 and 4 without actually introducing any of them. I think that Figure 2 is a clear and simple starting point for contrasting the North, Central and South Arabian Sea, but it doesn't really get any attention or explanation. Also perhaps a separate color scheme for the samples in each section would highlight the regional contrast.

We acknowledge that the flow of this section could be improved by better introducing the figures and providing a clearer structure. To address this, we have made the following revisions:

- Revised the introduction of Figures 2, 3, and 4 – We now introduce Figure 2 at the beginning of Section 4.1 as the primary reference for contrasting the North, Central, and South Arabian Sea before discussing Figures 3 and 4. We have revised the text as follows: Lines 175-179: "*In this section, we characterize the timing of formation, erosion, duration, and thickness for the contrasting monsoons and regions. Figure 2 provides an overview of the MW T-S properties colored by latitude, serving as the basis for contrasting the North, Central, and South Arabian Sea. Building on this, Figure 3 further illustrates the duration of the MWL, MWT, and the seasonality of the MW volume, while Figure 4 highlights MW formation relation to atmospheric forcing.*"

- Updated the color scheme – We have implemented a segmented colorbar every 5 degrees instead of a continuous colorbar to highlight latitudinal contrast in Mode Water T-S properties.



These adjustments should improve readability and ensure a clearer narrative for the reader. We appreciate your suggestion and believe these changes enhance the clarity of our results.

Lines 303-310: May strengthen your argument to touch upon some things that the models represent well, in addition to biases. For example, the 3D model capturing the summer mixed layer depth

We thank the referee and have now modified the text to add this in Lines 329-332 "*The MLD bias is consistent across the basin during winter, posing a constraint to our analysis. However, as the seasonal cycle is well reproduced, and the 3D model accurately captures the deep summer mixed layer in the center of the basin, we consider the model suitable for assessing MW life cycle mechanisms*."

Lines 318-319: This is a really nice point. MLD is much easier to diagnose in models than the Mode Waters themselves, and I would be interested in seeing if any more quantitative connections can be made. For example, you could look at a few timeseries for different regions to show how the MLD biases lag the MWT biases and how the magnitudes compare. Is it pretty consistent? Does it change by region?

We thank the reviewer for raising this important point. While a detailed investigation is beyond the scope of this study, we acknowledge that it warrants further attention, particularly from a modeling perspective. We have added a sentence emphasizing the significance of this metric and the open questions that remain following our analysis in Lines 344-346 "*The relationship between MLD and MW biases warrants further investigation. As MLD is easier to diagnose in models, analyzing its regional biases and lagged relation to MWT biases could help refine model representation of this layer and better constrain MW ecological implications*."

Line 322: "major differences and various dynamics" reads a bit clunkily here

We have removed it to make it more straightforward, as it wasn't bringing much content.

Lines 356-357: I'm missing the connections here that lead to the takeaway that the MWs are "driven by the build-up of upper ocean stratification during spring-summer from ocean heat gain". The reasoning in this paragraph is a bit unclear

Thank you for your comment. We recognize that the reasoning connecting upper ocean stratification during spring-summer to MWL deepening was not explicitly stated in the original text. To clarify this, we have revised the paragraph to make the logical connections more explicit. Specifically, we now explain how ocean heat gain strengthens stratification, which increases the density contrast, inhibits vertical mixing, and progressively isolates the MWL, leading to its downward migration. The revised text at the end of the paragraph now reads: Lines 381-386 *"...The deepening of the upper boundary of the MWL follows this seasonal cycle: as ocean heat gain strengthens upper ocean stratification during spring and summer, the increasing density contrast inhibits vertical mixing and progressively isolates the MWL, leading to its downward migration. Despite the overestimation of the pycnocline intensity, both models reproduce this deepening trend of the MWL's upper boundary (Figure 4b, 5c, and 8bc), supporting the conclusion that upper ocean stratification buildup from heat gain is the primary driver of MWL deepening."*

We hope this revision makes the reasoning clearer and directly addresses the concern raised.

Line 369: "In the Central…"

Thank you, changed.

Lines 397-403: consider moving to Discussion

Thank you, we have moved it to the discussion (Lines 525-530)

Section 4.4.3: The argument based on water type representation should be set up more clearly earlier on in the manuscript. The water types and absorption coefficients are alluded to in the methods but not explained (Line 107). Up until this point, the reader is under the impression that comparisons between the 1-D and 3-D models basically isolate horizontal advection and eddies. However, suddenly there is this other factor that has not been considered in the previous comparisons, which leads to questions about whether water types can also explain differences in other three test regions. If the absorption coefficients are referring to chlorophyll shelf-shading effects, I would think they could be important in all of the regions. If it is referring to the suspension of sediments, etc., then I would be skeptical of their influence this far from the shelf.

Thank you for your thoughtful feedback. We acknowledge that the role of water types and absorption coefficients was not sufficiently introduced earlier in the manuscript, which may have made their influence in Section 4.4.3 seem abrupt. To address this, we have revised the Methods section to explicitly introduce how water types and absorption coefficients influence stratification and to clarify that their impact is considered in our discussion. Additionally, we have now explicitly stated the limitation of using a single best-fit water type for the entire Arabian Sea, given that optical properties can vary seasonally. The revised text in the Methods section now reads:

Lines 109-117: "*The optical properties of different water types influence the absorption of shortwave radiation, potentially modifying stratification. Absorption coefficients vary due to chlorophyll-driven light attenuation, which is relevant across all regions but depends on local phytoplankton concentrations. Sediment-related absorption is primarily a concern in near-shelf environments and is unlikely to significantly influence offshore regions. … However, this represents a limitation, as it may introduce biases in regions where optical properties change seasonally. The potential impact of these biases on vertical stratification is examined in our model comparisons.*"

These revisions ensure that the role of water types and their limitations are clearly introduced earlier, providing better context for their discussion in Section 4.4.3.

Lines 440-465: I would argue that oxygen content below the mixed layer is more relevant here for ventilation and biogeochemical impacts. Is there a reason why you don't use the results excluding the mixed layer for Figure 9, instead of the 0-250m results?

Thank you for this point that elevates the impact of the results. We have changed the figure to masking MLD oxygen content to show below the ML to make the discussion more relevant for ventilation. We have also reorganized the text, excluding some of the metrics for the estimates, including the surface mixed layer oxygen concentrations, and detailing better the estimates for the contribution to the subsurface oxygen budget. We have made changes accordingly to section 4.5. In Lines 461-465 "*MW contribution to the total oxygen content of the upper Arabian Sea (250 m) accounts annually for only 4±1% (3±1% in the southern AS, and up to 8±2% in the northern AS; not shown). We argue that the oxygen content below the mixed layer is more relevant for ventilation and biogeochemical impacts. Thus, if we only account for subsurface oxygen content (i.e., excluding surface mixed layer saturated or oversaturated waters in contact with the atmosphere), …region where the anoxic waters (upper oxycline of the Arabian Sea OMZ) are close to the surface.*". We also changed the abstract percentages and the caption of Figure 9 accordingly.

Lines 523-537: It may be worth clarifying here that you are only discussing the impacts of locally formed MWs in the Arabian Sea. This may cause some confusion because remotely formed mode waters in the southern hemisphere likely join the Central Waters in the western boundary current and play a significant role in ventilating the AS OMZ as well.

We have now clarified that our discussion specifically refers to the impacts of locally formed MWs in the Arabian Sea. Additionally, we have explicitly mentioned that remotely formed mode waters also contribute to the ventilation of the Arabian Sea OMZ and have added a recent relevant citation to support this in Lines 553-555: "*Mode waters, both locally and remotely formed (e.g., Subtropical Underwater, Indian Central Water, and Subantarctic Mode Water) influence oxygen distribution in the Indian Ocean (Ditkovsky et al., 2023). Here, we focus specifically on the locally formed Arabian Sea Mode Water and its potential impacts on regional oxygen dynamics.*". We hope this revision improves clarity and properly acknowledges the role of remotely formed mode waters.

Line 562: Briefly, what are the expected precipitation changes in the AS with warming? Does it follow the "wet gets wetter, dry gets drier" paradigm (e.g. Held and Soden 2006).

Thank you for your insightful comment. You raise an important point regarding the potential impacts of climate change on the MW life cycle. While this region is predominantly driven by heat fluxes (Figure S2), changes in freshwater inputs could also play a significant role. We agree that this is a crucial question that warrants further exploration, though it is beyond the scope of the current study, which focuses on the seasonal characterization and mechanistic understanding of MW dynamics. To address this in the discussion, we have clarified the precipitation trend, noting: Lines 595-597 "*Moreover, freshening of surface waters due to increased precipitation is anticipated (Sharma et al., 2023), which will likely lead to changes in upper ocean stratification and subsequently affect MW formation - we have shown how salinity can change MW volume..*"

Sharma, S., Ha, K. J., Yamaguchi, R., Rodgers, K. B., Timmermann, A., & Chung, E. S. (2023). Future Indian Ocean warming patterns. Nature Communications 2023 14:1, 14(1), 1–11. https://doi.org/10.1038/s41467-023-37435-7