Manuscript egusphere-2025-4712 by Oboh et al., "The Oxygen Minimum Zones (OMZs) in the Indian Ocean"

Dear editor and reviewers,

First of all, thank you very much for the opportunity to submit a revised version of our manuscript. We have revised the entire manuscript to address all comments by the reviewers and community, paying particular attention to four major concerns raised by the reviewers on a) the eOMP method, b) the selection of source water types (SWTs), c) the interpretation of two snapshots as long-term trends, and d) the presentation of the manuscript (title, scope, objectives, separation of results and discussion, etc.).

Below, we respond to the major concerns and hope to thereby resolve the issues raised by the reviewers. This is followed by point-by-point response to all comments.

a) The eOMP method.

In the previous version there were 2 concerns on the eOMP method (i) underdetermined solution and (ii) the robustness of the eOMP solution to variability in oxygen and nutrient properties of the SWTs, since we assumed constant oxygen and nutrient values for 1995 and recent years (2016–2024).

In the revised manuscript we now ensure a determined eOMP solution by reducing the number of SWTs and increasing the number of constrains as explained below:

"In order to achieve a determined eOMP solution, we reduced the number of SWTs to six by means of an exclusion process. First, we performed a preliminary underdetermined eOMP analysis using the entire (8) SWTs. Thereafter, we separated the SWTs into major SWTs (PGW, STSW, mAABW, ITFW) and minor SWTs (AAIW, ASHSW, RSW and BoBW) based on their contributions in the Arabian Sea OMZ. Then, we performed four sets of overdetermined eOMP analysis using five SWTs and seven constraints (temperature, salinity, oxygen, phosphate, nitrate, silicate and mass). The five SWTs included four major SWTs that were kept constant plus one minor SWTs at a time. Using this method, we identify PGW, RSW, STSW, mAABW and ITFW as the major contributors to the OMZ with the least residuals, and the combination of PGW, RSW, STSW, mAABW, ITFW and AAIW as the major contributors in the entire western Indian Ocean transect with the least residuals (Supplement Fig. S1, S2). Since these six SWTs resulted in even smaller residuals in the eastern Indian Ocean transect, we adopted them in the final eOMP analysis for all transects."

In order to take global change impacts into account when defining SWTs property we used the WOA 1995–2004 data as well as WOA 2015–2022 data to define the temperatures and salinity of the SWTs. Since there are no decadal averages in the WOA data for oxygen and nutrients, we defined the nutrient concentration of SWTs using the WOA "All time periods" data (1965-2022) for both 1995 and recent years eOMP analysis. This means that we assume that concentration of nutrients in the near-surface layer of the ocean, where SWTs form, has not changed. Although this assumption cannot be proven, it is supported by the lack of evidence of a change in the CO_2 uptake by the biological carbon pump, which is closely linked to the availability of nutrients. Furthermore, since primary SWTs are not formed by the mixing of different water types (such as ICW), changes in nutrient concentrations resulting from varying mixing ratios should also be negligible. Unlike nutrient concentration, changes in oxygen concentration can be assumed, as the solubility of oxygen depends on temperature and salinity. Therefore, we defined the SWTs oxygen concentration using the 1971–2000 WOA oxygen dataset for the 1995 eOMP analysis, while for eOMP analysis of recent years, we calculated the SWTS oxygen values in a manner that accounts for potential climate-driven changes in oxygen, as described below:

"Oxygen saturation concentrations for 1995 and recent years were calculated from the corresponding temperature and salinity of each SWT using the formulation of Benson and Krause Jr (1984). The resulting values were converted from µmol L^{-1} to µmol kg^{-1} using seawater density derived from the UNESCO equation of state at atmospheric pressure. Differences in oxygen saturation concentration between 1995 and recent years were interpreted as changes in oxygen. As most of these changes were negative (with the exception of AAIW), consistent with reported global ocean deoxygenation driven by warming (Schmidtko et al., 2017), we subtracted the calculated oxygen changes from the SWT oxygen values derived from the WOA 1971–2000 dataset to estimate SWT oxygen values for recent years. Accordingly, WOA (1971–2000) SWT oxygen values were used for the eOMP analysis of 1995, while the adjusted SWT oxygen values (i.e., 1995 values minus the calculated oxygen change) were used for the eOMP analysis of recent years (Table 2)."

This is not perfect, but in our opinion, it is a comprehensible approach that represents the influence of warming on oxygen concentration in a representative manner.

Additionally, we also tested the robustness of the eOMP solution to increase or decrease in oxygen values of SWTs in the later years. The oxygen variability test confirms the robustness of our eOMP solution to SWTs oxygen variability and thus our conclusions (Supplement Fig S3).

b) The selection of source water types:

Following the definition of water mass ages (Jeansson et al., 2021; Olsen et al., 2019), a primary SWT is formed when water is introduced into the ocean interior and becomes decoupled from atmospheric influence. Secondary SWTs are mixtures such as Indian Central Water (ICW) and Subantarctic Mode Water (SAMW). We have focused on primary SWTs since they are essentially responsible for the ventilation of the OMZs and carry the influence of global warming on their properties into the depths of the ocean. Nevertheless, secondary SWTs such as ICW and SAMW have not been excluded in our selection. Their influence is fully represented through their parent source components (i.e., the STSW and AAIW) (Tomczak and Large, 1989; You, 1998; Schott and Mccreary Jr, 2001; Sokolov and Rintoul, 2002; Vianello et al., 2017).

The number of SWTs we considered was reduced to six by means of an exclusion process, as explained earlier, for methodological reasons, as suggested by reviewer 1. However, in addition to four SWTs that outcrop at the surface in the Indian Ocean, the six water masses also include the ITFW and mAABW. These two water masses are exceptions in that they are not formed at the surface of the Indian Ocean but in the Indonesian Seas and the deep sea, respectively. Unlike reviewer 1, however, we also consider the Bay of Bengal Water to be a source water formed at the surface of the Indian Ocean (Shetye et al., 1996). In any case, both the ITFW and the mAABW are important water masses for the ventilation of the OMZ in the Indian Ocean. We have therefore taken them into account and assumed that they influence the ventilation of the OMZ. We have explained these reasons for the selection of SWTs and the exclusion process in more detail in the revised manuscript to make our approach more comprehensible to readers.

Another critical element brought up by the reviewer was that PGW and RSW were defined downstream within the Arabian Sea rather than formation or entry point. In defining source water properties, it is important to note that water masses are not exported to depth continuously from the surface, and therefore surface mean properties are often not representative of the water that ultimately ventilates the ocean interior. For this reason, SWTs are commonly characterized just below the surface mixed layer, where waters begin their subsurface pathways. In the case of PGW and RSW, both the outcropping regions and the depths at which these waters descend vary spatially, and source properties are therefore typically defined at their entry into the subsurface Indian Ocean, rather than at their outcrop sites. We agree that PGW and RSW properties are defined downstream within the Arabian Sea rather than at their outcrop site. According to the pathways described by Tchernia (1980), we have revised the definition regions for PGW and RSW to around the Oman slope and sill, respectively. These are the entry points into the Arabian Sea. The defined RSW and PGW represent slightly modified primary SWTs. As a result, oxygen concentrations increase and nutrient concentrations decrease, i.e., water masses more closely resemble the properties of surface water, as suggested by the reviewers. We have revised the methods section of the manuscript to clarify SWTs selection and definition.

c) The interpretation of two snapshots as long-term trends

We have adjusted the manuscript to remove the interpretation of two snapshots as long-term trends. We agree with the reviewers that such a comparison does not indicate a trend. However, the evaluation of the literature, in line with our previous results, clearly shows that the OMZ was in a phase of expansion in 1995 (Banse et al., 2014; Rixen et al., 2014, 2020; Goes et al., 2020). Argo float data show now that the OMZ in the Arabian Sea has been shrinking massively since around 2013 (Liu et al., 2024). The GLODAP database enables us to conduct OMP analysis for the years 1995 and 2016, representing periods of OMZ expansion and shrinking in the Arabian Sea.

Furthermore, the compared periods do not represent persistently opposite or extreme ENSO or IOD phases, but rather a mixture of weak, moderate, and neutral conditions, suggesting that pronounced climate anomalies are of little significance for the differences observed. Differences between 1995 and 2016 thus allow conclusions to be drawn about processes that could explain a phase transition from expansion to contraction of OMZs. However, we can just conclude that such a transition seems to have occurred and not that this is a trend which would point to an increasing contraction in future. We will express this clearer in the revised versions of the manuscript.

e) The presentation of the manuscript (title, scope, objectives, separation of results and discussion).

We appreciate the reviewers comment on the presentation of the manuscript, we have reviewed the presentation of the entire manuscript, separated the results and discussion, included a clear scope and objectives. Furthermore, as suggested by the reviewers we have revised the title of the manuscript to reflect the new contributions of the article to: "Response of the oxygen minimum zones in the Indian Ocean to changes in the meridional overturning circulation"

We hope that this has addressed the main points of criticism raised by the reviewers. Please find below the point-by-point response.

POINT-BY-POINT RESPONSE

Comments from Reviewer 1

Comments = black,

Response = blue

General Assessment

This is my first review of the manuscript "The Oxygen Minimum Zones (OMZs) in the Indian Ocean". The manuscript analyses shipboard transects from 1995 and 2018, along with available observation-based products, to assess recent changes in the Northern Indian Ocean OMZs. The study is a topical analysis of available datasets and aims to address gaps in the literature regarding recent variability in OMZ extent. However, I have several concerns about the implementation of the methodology, interpretation of the results, and presentation of the manuscript. The study requires major revisions before being considered for publication. I outline my major points of concern below.

Methodology:

1. Perhaps my biggest concern is with the implementation of the eOMP method. In this study, the authors define 9 unknowns (8 source water types plus remineralization). This requires at least ten constraints for an "optimum" multiparameter analysis or nine constraints for a simple determined set of equations. What are these constraints? I can only count 6 unique constraints – temperature, salinity, oxygen, phosphate, nitrate, and mass.

Thank you for this constructive comment. Regarding the formulation of the eOMP analysis. We agree that as originally presented, the inclusion of 8 SWTs plus a remineralization term leads to an underdetermined system when constrains are only 6.
We have addressed these concerns in the revised manuscript as explained in the response summary above.

2. The definition used for PGW in this study, which here has an oxygen concentration of 15.64 umol/kg, is misleading. PGW is generally considered to be oxygen saturated, and gets quickly diluted once it enters the Arabian sea (e.g. Font et al., 2024, *JGR:Oceans*). The definition here is thus of highly diluted PGW, which is essentially just the AS OMZ water (i.e., the water mass being evaluated). This is why PGW is so prevalent in Figure 3a. The definition of PGW here will make it difficult to connect changes in the source water fraction to changes in the actual PG outflow rates or properties.

We agree that the PGW used in the original analysis represents a higher nutrient form of PGW because its properties were defined closer to the Arabian Sea. As earlier explained in the response summary, we have revised the definition of PGW parameters to better reflect outflow properties. We re-defined the properties of PGW using the World Ocean Atlas (WOA) data around the Oman slope at depth between 100-300m, latitude between 23.6 -27.1°N and longitude 55.5 -57.5°E according to the PGW pathway described by (Tchernia, 1980). Defining the properties around this region helps us capture climate driven changes in the source region and outflow properties as observed in the increase in temperature value. Defining PGW properties closer to their entry regions therefore provides a more realistic representation of the oxygen and nutrient characteristics that actually ventilate the Indian Ocean/OMZs.

Interpretation:

3. A recurring point in the manuscript is that this study differs from past studies because it focuses on “surface-originated water masses”. This is quite misleading. First of all, some of the source water types are not really surface-originated at all, like ITFW or BoBW. But more importantly, for the source waters which do outcrop, the corresponding source water types are not defined at those outcrops. Rather, they are defined in regions where those waters enter the Indian Ocean (Table 3), after they have already experienced significant water mass transformation. So, changes in those defined source water types do not necessarily reflect changes in the formation rates/conditions of those water masses. This is especially evident in the oxygen properties used for the source water types (would be nearly saturated at formation for many of the source water types).

As earlier explained (kindly refer to response summary above), we have focused on primary SWTs since they are essentially responsible for the ventilation of the OMZs and carry the influence of global warming on their properties into the depths of the ocean. We intentionally defined SWT properties at the regions where these waters enter and influence the Indian Ocean/OMZs, rather than at their distant outcrop or formation sites. For SWTs formed within the Indian Ocean, Arabian Sea and Bay of Bengal (e.g., STSW, ASHSW, BoBW), we defined their properties based on the WOA data from the outcrop region and pathways described in literature. SWTs whose outcrop region/formation sites are not within the Indian Ocean (i.e., PGW, RSW, mAABW, ITFW), were defined in regions where they first enter into the Indian Ocean/Arabian Sea based on their pathways described in literature. We agree that SWTs are much more oxygen saturated at their outcrop region, however, defining SWT properties closer to their entry regions therefore provides a more realistic representation of the oxygen and nutrient characteristics that actually ventilate the Indian Ocean/OMZs. For example, we defined the PGW and RSW around the slope of Oman and around the sill respectively, where they have not undergone much mixing within the Arabian Sea, we also defined STSW in the formation region and AAIW around the entrance region into the Indian Ocean.

In the revised manuscript, we have replaced this term “surface-originated water masses” with “primary SWTs” the meaning of this term has also been defined within the manuscript and are distinct from secondary mixtures such as Arabian Sea Water (ASW) which is a strong mixture of Red Sea Water (RSW) and Persian Gulf Water (PGW), or Indian Central Water (ICW), which is a mixture of Antarctic Intermediate Water (AAIW) and Subtropical Surface Water (STSW).

4. The study is inferring climate-driven changes by comparing transects in 1995 and 2018, each providing a snapshot within a single year. The influence of interannual and decadal variability has to be discussed at some point in the manuscript. Limitations of the methodology in general need to be more clearly addressed.

As explained earlier (kindly refer to summary above) the compared periods do not represent persistently opposite or extreme ENSO or IOD phases, but rather a mix of weak, moderate, and neutral conditions, We have also discussed the influence of interannual and decadal variability in the revised manuscript and included possible limitations to the methodology.

Presentation:

5. The manuscript needs to be structured and presented as a research article, starting with the title. The title suggests that the article is a discursive piece, or even a review, which is misleading. Plenty of literature already exists on OMZs in the Indian Ocean and the purpose of this article is not to be a comprehensive resource (like a textbook or an article like Schott and McCreary, 2001). **Please choose a title that highlights the new contribution of the article**. Confusion about the article’s identity is also evident throughout the introduction. For example, Section 1.1 provides far too much information that is not relevant for the reader to understand the results of this study. Don’t be afraid to rely on existing literature to provide a comprehensive reference so that you can focus on details that the reader needs to understand the present results.

We agree that the original title and parts of the Introduction could give the impression of a broad review rather than a focused research article. To address this, we have revised the Introduction and the Study area to be more precise and changed the title accordingly.

6. Please separate results and discussion. As it is written, it is unclear what conclusions are drawn from the presented results and what conclusions are drawn from the literature. For example, Section 3.2 discusses connections to the literature but do not present any new results. While Figures 4 and 5 are technically presented in this section, they are not explained or analysed and their connection to the points discussed are unclear.

We have revised the manuscript and separated the 'Results' and 'Discussion' sections, with the aim of clarifying the conclusions.

7. In the manuscript, only Figures 3 and 6 are explained as results in any meaningful way. For figures 4, 5 and 7, either make them supplementary or actually explain them and how you interpret them.

We have revised figure 4 and 5 and included more lines of discussion. While figure 7 was moved to the supplementary figures.

Other points:

8. I would suggest using more specific language than "water masses" throughout. In the manuscript, water masses refer to both the source waters and the destination waters and it can get quite confusing. I would recommend using the conventional term of "source water types" or something like that.

Thank you for this suggestion, we have adopted the term "source water types" for source waters across the manuscript.

9. Equation 4: How does this compare to the remineralization term calculated within the eOMP which tracks the conversion from oxygen to phosphate? And why is the oxygen residual added here. Should it instead be set aside as an uncertainty bound?

Equation 4 :

oxygen consumption = measured oxygen – oxygen supply + oxygen residual

This equation represents biological oxygen consumption due to remineralization, and it is exactly equal to the remineralization term calculated within the eOMP framework which can be retrieved with the equation below:

oxygen consumption2 = -155*1 * phosphate remin

-155*1 is the conversion ratio of oxygen/phosphate

And "phosphate remin" is the phosphate remineralization values from the eOMP solution.

Both equations give exactly the same value for oxygen consumption, we have included the residual in equation 4 for this reason to make it equal to the term calculated using phosphate remineralization. The difference however without the residual is very small and negligible.

We have however, revised equation 4 and removed "oxygen residual" as this is an uncertainty term.

10. Add reference for Figure 1. Maybe use this figure to highlight your source water types and the pathways that they take to the OMZs. Help guide the reader, they are reading to understand the new result that you are contributing.

We have added a reference to Figure 1. And highlighted the region where the SWTs were measured in Figure 3 in the revised manuscript.

11. Line 668: "...increased physical oxygen supply via the inflow of oxygen-rich water masses from the southern Indian Ocean is able to compensate for the increased biological oxygen consumption." Careful when discussing the opposing roles of physical supply and biological consumption. It's likely that increased biological consumption is simply a response to increased physical supply or vice versa.

What we intend to imply is that despite increased oxygen consumption, there is an overall increase in dissolved oxygen. We have revised the section of the manuscript to read as follows:

"…the enhanced inflow of oxygen-rich remote SWTs increases physical oxygen supply. Although this is accompanied by higher biological oxygen consumption, the net result is an overall increase in dissolved oxygen..."

12. Line 680: "The current changes indicate the presence of a longer time-scale cycle in the Indian Ocean OMZs which aligns with paleoceanographic archives, however, this cycle seem to be reversing…" The connections to paleoceanographic signals come off as very speculative and unsubstantiated. Separation of results and discussion is crucial here to make this thought process more clear.

We have separated the results and discussion sections and emphasized the speculative nature of our connection. The text has been revised.

Referee 2 comments

Comments = black,

Response = blue

General Assessment

The manuscript by Oboh et al. examines recent changes in dissolved oxygen in the northern Indian Ocean, with a focus on the Arabian Sea and Bay of Bengal Oxygen Minimum Zones (OMZs). Using hydrographic observations from GLODAPv2 (1995–2024), the authors apply an Extended Optimum Multiparameter (eOMP) analysis to investigate the drivers of reported oxygenation trends in the Arabian Sea and the apparent lack of deoxygenation in the Bay of Bengal. They attribute oxygenation at intermediate depths primarily to weakened local ventilation and a reduced contribution from the Indonesian Throughflow (ITF), accompanied by increased inflow of oxygen-rich waters from the Southern Indian Ocean.

While documenting recent changes in Indian Ocean OMZs is scientifically important, and the manuscript is generally well written, I have serious concerns regarding both the methodology and the interpretation of the results. In particular, the application of the eOMP framework relies on assumptions of stationarity in source water properties that are unlikely to hold over decadal timescales. Moreover, attributing differences between two temporal snapshots to long-term climate trends without explicitly accounting for natural decadal variability is problematic. Owing to these fundamental issues, I cannot recommend publication in *Biogeosciences* in its current form. My major concerns are detailed below.

Major Comment #1: Methodological limitations of eOMP analysis under non-stationary conditions

My primary concern relates to the application of the eOMP analysis and its suitability for diagnosing decadal oxygen changes using the available data. The authors define source water mass properties using World Ocean Atlas (WOA) climatologies, implicitly assuming that the biogeochemical properties (particularly dissolved oxygen) of these source waters remain constant over time. **However, ventilation changes can manifest not only through variations in water mass fractions (mixing ratios), but also through temporal changes in the properties of the source waters themselves**.

Dissolved oxygen concentrations in source waters can vary substantially on interannual to decadal timescales. If, for example, a source water mass such as the ITF becomes more oxygenated during the study period relative to its climatological definition, the OMP algorithm—constrained by mass conservation—may artificially reduce the estimated fraction of that water mass (or increase the contribution of a high-oxygen end-member such as mAABW) in order to satisfy the oxygen budget. This can lead to spurious attribution of changes in water mass contributions. **The authors should explicitly test and demonstrate the robustness of their conclusions to plausible temporal variability in end-member properties.**

We have made appropriate adjustment to ensure the robustness of the eOMP method. Kindly refer to the response summary above on “the eOMP method”

Major Comment #2. Selection of source water masses and physical consistency

The selection of source water masses appears incomplete and, in some cases, physically inconsistent with the timescales considered. Key water masses known to ventilate the Indian Ocean thermocline and intermediate depths—such as Indian Central Water (ICW) and Subantarctic Mode Water (SAMW)—are not included in the analysis. Omitting these major ventilators may force the OMP solution to compensate by assigning unrealistic fractions to other water masses (e.g., STSW or mAABW), resulting in biased or physically implausible mixing ratios.

Attributing oxygenation of intermediate layers (upper ~1200 m) to Modified Antarctic Bottom Water (mAABW) is difficult to reconcile with physical constraints. Ventilation of OMZ cores by bottom waters originating at depths >3000 m within a ~20-year interval appears implausible.

The analysis compares two snapshots separated by approximately two decades. However, transit times for Southern Ocean-derived waters to reach the northern Indian Ocean via mean circulation are typically longer than this interval. It is therefore unclear how the reported changes between 1995 and 2018 can be attributed to remote Southern Ocean ventilation rather than to local or regional processes.

We have focused on primary SWTs since they are essentially responsible for the ventilation of the OMZs and carry the influence of global warming on their properties into the depths of the ocean. Nevertheless, secondary SWTs such as ICW and SAMW have not been excluded in our selection. Their influence is fully represented through their parent source components (i.e., the STSW and AAIW) (Tomczak and Large, 1989; You, 1998; Schott and Mccreary Jr, 2001; Sokolov and Rintoul, 2002; Vianello et al., 2017). Therefore, including both the ICW and the SAMW separately would probably introduce redundancy and potentially destabilise the eOMP solution. The methods and manuscript have been revised to fix other issues.

Major Comment #3. Distinction between decadal variability and long-term trends

The manuscript frequently interprets differences between the two analyzed periods as indicative of long-term climate trends. However, the analysis is based on comparisons between two discrete snapshots rather than continuous time series. Numerous studies (e.g., Long et al., 2016) have shown that internal climate variability can drive large-amplitude oxygen fluctuations that mask or even reverse long-term trends over multi-decadal timescales.

No justification is provided for assuming that the difference between the selected years represents a secular trend rather than a phase of natural variability. The discussion should explicitly address this limitation and more carefully distinguish between internal variability and forced trends.

We agree that comparisons between discrete periods must be interpreted with caution, particularly in a system where internal climate variability can generate oxygen fluctuations on interannual to multi-decadal timescales, however, as earlier discussed in the summary (kindly refer to summary above), we have revised the manuscript to remove the interpretation of a long-term trend.

Major Comment 4. Study objectives, scope, and presentation

The scope and objectives of the study require clearer definition. The title ("The Oxygen Minimum Zones…") is overly broad and does not reflect the limited spatial and temporal scope of the analysis. The introduction should more clearly state whether the primary objective is to evaluate the eOMP methodology, document a specific observational change, or attribute long-term trends.

Thank you for the constructive comments, we have revised the manuscript to clearly defined the scope and objectives and primary objective of the study in the last paragraph of the introduction.

Secondly, we agree that the original title give the impression of a broad review rather than a focused research article. We have also revised the title.

In addition, the eOMP methodology requires clearer documentation. The definition of properties A and B, the choice of weights (lines 255–258), and the selection of source water masses should be more thoroughly justified based on existing literature and physical constraints, including transport timescales. The spatial extents of the Arabian Sea (AS) and Bay of Bengal (BoB) OMZs, as well as the portions of the transects used to estimate OMZ oxygen budgets, should be explicitly indicated in Figure 2.

The description of the eOMP method has been expanded and clarified. We now explicitly define properties A and B as oxygen and nutrient tracers used to represent remineralization processes. The choice of parameter weights has been more thoroughly justified with reference to prior applications in the Arabian Sea and Indian Ocean (e.g., Acharya and Panigrahi, 2016). Additional explanation/justification has been added on the selection of SWTs.

The spatial extents of the Arabian Sea (AS) and Bay of Bengal (BoB) OMZs, as well as the portions of the transects used to estimate OMZ oxygen budgets has been indicated using Figure 2.

Some more specific comments

P4, line 111: Section 1.1 is awkwardly placed. Key background information on OMZs appears after the research questions are formulated. This material should be moved earlier in the Introduction and should not appear as a subsection.

We appreciate the referee's comment on the placement of Section 1.1.and will redesign the text accordingly.

P6, line 189: The steady-state assumption underlying the eOMP method—that source water biogeochemical properties (oxygen, nutrients) do not change over time—should be explicitly discussed. How do the authors address the possibility that source water oxygen concentrations themselves are different between the two snapshots?

We addressed this issue by calculating possible oxygen change due to climate change. (Kindly refer to response summary)

P11, line 310: Water mass properties are derived from WOA climatologies. In several Indian Ocean source regions (e.g., the Red Sea and Persian Gulf), oxygen and nutrient observations are sparse or absent in WOD. Consequently, WOA values may be poorly constrained or extrapolated from nearby regions, which could strongly bias the analysis. This limitation must be discussed.

To defined the SWTs, we used the World Ocean Atlas (WOA) annual statistical mean data, which represent average of all unflagged interpolated values at each standard depth level for each variable in each ¼° or 1° square which contains at least one measurement for the given oceanographic variable, this often offers higher accuracy and reliability compared to extrapolated data.

We agree that oxygen and nutrient data in some Indian Ocean source regions, particularly the Red Sea and Persian Gulf, are sparse /missing in the WOA Database. However, further analysis of WOA "number of observations" data shows high density of observations in the region where PGW and RSW were defined, as well as other SWTs.

We have included the text below in the methods section of the revised manuscript to clarify our methods.

"Water-mass properties were derived from World Ocean Atlas (WOA) annual statistical mean, represent average of all unflagged interpolated values at each standard depth level for each variable in each ¼° or 1° square which contains at least one measurement for the given oceanographic variable. We acknowledge that data interpolation can add some uncertainty. However, analysis of WOA "numbers of observation" dataset shows high density of observations in regions where the SWTs were defined."

P12, Table 2: Persian Gulf Water is known from local observations to be extremely oligotrophic. The nutrient concentrations assigned here are an order of magnitude higher than reported values, suggesting that what is referred to as PGW is mostly Arabian Sea waters.

We agree that the PGW used in the original analysis represents a higher nutrient form of PGW because it was calculated closer to the Arabian Sea. We have revised the definition of PGW parameters to better reflect outflow properties. We re-defined the properties of PGW using the World Ocean Atlas (WOA) data around the Oman slope at depth between 100-300m, latitude between 23.6 - 27.1°N and longitude 55.5 - 57.5° E according to the PGW pathway described by (Tchernia, 1980). Defining the properties around this region helps us capture climate driven changes in the source region and outflow properties as observed in the increase in temperature value. Defining PGW properties closer to their entry regions therefore provides a more realistic representation of the oxygen and nutrient characteristics that actually ventilate the Indian Ocean/OMZs. (We have updated Table 2 accordingly.). Kindly refer to the response summary above on selection of SWTs.

P13, Table 3: PGW and RSW properties appear to be estimated downstream within the Arabian Sea rather than at their formation or entry points, implying that these "water masses" are already mixtures. This should be discussed and the implications clarified.

Additionally, some water masses are defined using shallow layers (e.g., ASHSW, BOBW, STSW), while others are defined at depth. Since all water masses originate at the surface, a **consistent approach should be adopted and justified**.

As earlier discussed in the response summary above, we have revised the definition regions for PGW and RSW to around the Oman slope and sill, respectively.The defined RSW and PGW properties represents primary SWTs, and not secondary mixtures like Indian Central Water or Arabian Sea Water. This choice is intentional as properties defined here therefore represent the ventilating PGW and RSW as they actually enter and influence the Arabian Sea OMZ. We have revised the methods section of the manuscript to clarify SWTs selection and definition.

We also agree some SWT definitions were too shallow, we have therefore revised the definitions of Arabian Sea High Salinity Water (ASHSW), Subtropical Surface Water (STSW) and Bay of Bengal Water (BoBW) based on existing literatures. Table 3 has been adjusted likewise.

Lines 373–375: The phrase "similar climate anomalies" is unclear. The role of interannual and decadal variability between the two periods needs to be more explicitly examined or discussed.

We thank the referee for pointing out this ambiguity. We agree that the phrase "similar climate anomalies" was unclear. In the revised manuscript, we now explicitly discuss the role of interannual and decadal variability between the two periods, with specific reference to ENSO and Indian Ocean Dipole (IOD) events.

P14, lines 385–387: While redefining water mass properties for different periods may account for warming, similar temporal changes likely affect oxygen and nutrient content as well.

As earlier discussed in response summary, we have calculated possible oxygen change and performed oxygen variability test, using higher and lower oxygen scenarios and our conclusions remain valid. With respect to nutrients, we have also explained why changes in the period studied can be negligible.

P18, line 501: What are the expected ventilation timescales from the Southern Ocean (AAIW, mAABW)? Over a ~20-year period, the observed changes are more likely dominated by local ventilation and local biogeochemical processes.

Our results reflect the relative contribution of sourced waters in the Indian Ocean interior as they were in 1995 and ~2016. Changes of the relative contribution of sourced waters have been derived by comparing the results from 1995 and ~2016. Our explanation of these changes refers - as indicated by the reviewer - first of all on processes in upper ocean: an enhanced inflow of

STSW balancing the reduced contribution by the Indonesian Throughflow, PGW etc. The enhance contribution of deep water would primarily be seen as a consequence of the redistribution of water masses in the upper part of the water column. We will describe this better in the revised manuscript.

P20, lines 540–541: The study does not demonstrate trends over recent decades, but rather differences between two transects separated by two decades. Given strong interannual and decadal variability, conclusions about long-term trends are not supported.

We agree that comparisons between discrete periods does not indicate a trend, we have revised the manuscript to remove the indication of a long term trend.

EGUsphere Community comments from Yun Qiu,

Comments = black,

Response = blue

General Assessment

This study investigates the recent trend reversal of the Arabian Sea (AS) Oxygen Minimum Zone (OMZ) and the shrinkage of the Bay of Bengal (BoB) OMZ. By applying an extended Optimum Multiparameter (eOMP) analysis to historical data and cruise observations, the authors attribute these changes to a basin-wide shift in water mass composition. They found that increased ventilation by oxygen-rich water masses from the southern Indian Ocean (e.g., STSW, mAABW), which outpaced increased biological oxygen consumption, is the primary driver of the trend reversal of AS OMZ. The manuscript addresses a timely and important topic, and the data-driven approach provides valuable insights. However, several issues regarding the clarity of the mechanistic narrative and the presentation of evidence need substantial revisions. In addition, the manuscript still contains some grammatical errors and inappropriate use of transition words throughout (e.g., However, Nevertheless, In contrast), which significantly affect readability and the clarity of the scientific arguments. A thorough language revision is required. I therefore recommend major revision.

1. The current title is overly broad and does not reflect the actual scope of the study. A more specific, process-oriented title is recommended. For example: Recent changes in oxygen minimum zones of the northern Indian Ocean.

Thanks for your comments, we have revised the manuscript title to reflect our findings

2. The abstract is overly long. The authors are encouraged to streamline the abstract by focusing on the main findings and their key implications.

Thanks for this suggestion. We would improve the abstract in the revised manuscript.

3. The conclusions of this study about the water mass composition are impressive. However, the argumentation could be made more intuitive and compelling. I recommend that the authors include basic dissolved oxygen (DO) distribution maps in the revised manuscript. For instance, providing vertical section plots of DO concentration along key transects, comparing data from 1995 (or earlier) with recent years, would visually demonstrate the changes in oxygen content within the OMZ core and the migration of its boundaries. Furthermore, a direct comparison between the observed DO changes and the shifts in water mass composition identified by the eOMP analysis would significantly strengthen the paper. Correlating these spatial and temporal patterns would provide a clearer,more direct factual foundation for the subsequent complex mechanistic eOMP analysis, directly linking the physical oxygen supply to the documented oxygen changes.This would greatly enhance the clarity and persuasive power of the manuscript.

Thanks for your recommendation. We have included the suggested distribution maps.

4. About Figure 6: It is unclear how the observed dissolved oxygen changes shown in Figure 6 were calculated. If these values represent regional or OMZ-core averages, the corresponding spatial boundaries and averaging method should be clearly specified to ensure consistency with the oxygen supply estimates derived from the water mass analysis. Furthermore, the interpretation that the difference between mixing-derived oxygen supply changes and observed oxygen changes directly reflects variations in biological oxygen consumption requires further justification. In particular, this approach appears to implicitly assume that other processes affecting oxygen, such as local mixing or diffusive transport, remain unchanged or negligible. Please clarify and discuss the validity of this assumption.

Thank you for this comment, we have clarified the calculation and interpretation in the methods as well as in Figure 6 caption.

The measured oxygen values represent average dissolved oxygen within the OMZ core ($O_2 < 20\ \mu mol\ kg^{-1}$) over the same depth ranges used in the eOMP analysis (Arabian Sea: 150–1000 m; Bay of Bengal: 150–800 m). These spatial boundaries and the averaging method are now explicitly stated to ensure consistency with the oxygen supply estimates.

Oxygen supplied by mixing is calculated from the eOMP-derived water-mass fractions and their source oxygen concentrations, averaged over the same OMZ volume. Biological oxygen consumption is then estimated as the difference between mixing-derived oxygen supply and measured oxygen, following standard eOMP practice. Local mixing and diffusive processes are implicitly included in the mixing term, while unresolved small-scale effects are assumed to be minor relative to the basin-wide changes observed.

MINOR COMMENTS

5.Line 44: does not only affect -> not only affects. Changes implemented in the revised manuscript

6. Line 60: are the semi-enclosed basins …-> are semi-enclosed basins …

Changes implemented in the revised manuscript

7. Lines 59-65: This paragraph suffers from major language issues and an unclear conceptual framework. The definitions of hypoxia and microbial hypoxia are currently confusing and partially incorrect, and the sentence "Hypoxia defines the oxygen threshold…"is grammatically incorrect. Please revise for both language and conceptual clarity.

The paragraph has been revised to improve clarity.

8. Line 68: Nevertheless, …-> In particular, …

Changes implemented in the revised manuscript

9. Line 75: OMZ -> OMZs

Changes implemented in the revised manuscript

10. Line 89: close link between …to …-> a close link between … with …

Changes implemented in the revised manuscript

11. In Figure 3, the seven bar charts have very similar visual appearance, while the panel sizes and alignment are inconsistent. A reorganization of the figure layout, along with clear indication of the study region in each subpanel, would substantially improve clarity.

Thanks for your comment, the figure has been revised to improve clarity as suggested.