

Referee # 2/1: Annie Bourbonnais

Dear Annie Bourbonnais,

thank you very much for your constructive and valuable comments, which helped us a lot to further improve the manuscript. The point-by-point reply to your latest comments follows below.

With best regards,
Tim Rixen, also on behalf of all coauthors.

General comment:

In their revised manuscript, Rixen et al. addressed most of my concerns. The revised manuscript is greatly improved, and was completely re-structured. Although quite dense and not always concise, this manuscript will represents a great contribution to the special issue "Understanding the Indian Ocean system: past, present and future" in Biogeosciences. I recommend minor revisions after addressing the few comments below.

Overall, some confusion still persists regarding their O₂ threshold used to define hypoxia. In their response, they explain that they considered 20 μ M as a upper threshold below which denitrification and anammox occurs, i.e., fixed nitrogen is transformed to N₂. However, such a high O₂ threshold is not supported by recent studies (including Dalsgaard et al. (2014) and Bristow et al. (2016)). For instance, Dalsgaard et al. (2014) reports O₂ threshold in the nmol range for conversion of nitrate to N₂. Other recent studies (e.g., Frey et al. 2020) report a much higher O₂ threshold (up to 10 μ mol) for nitrate reduction to N₂O, yet lower than the suggested value of 20 μ mol. Furthermore, later on (response to my comment, line 471 of the original manuscript), they admit using a O₂ threshold of 6 μ M for denitrification in the PISCES model, which makes it even more confusing. I recommend the authors to lower their upper O₂ threshold for denitrification (and hypoxia) to at most 10 μ M, which would be more in line with the value suggested by most recent studies as well as the value used in the PISCES model.

In principle, we see here no contradiction to the text in our manuscript, but maybe we did not express ourselves clear enough. What we wrote was:

'According to experiments and *in situ* observations, anammox sets in when oxygen concentrations drop below \sim 20 μ M, while denitrification occurs at oxygen concentrations of approximately $<$ 6 μ M (Fig. 2, Bristow et al., 2016; Dalsgaard et al., 2014; Kalvelage et al., 2011).'

Since we defined microbial hypoxia as the range within which anoxic microbial processes can occur, and anammox is one of these, we set the threshold for microbial hypoxia to an oxygen concentration of \sim 20 μ M. However, we also pointed out that anoxic processes gain importance and slowly outcompete oxic processes at lower oxygen concentrations. As shown e.g. in Fig. 3 by Dalsgaard et

al. (2014), a decreasing oxygen inhibition seems to cause this, which includes also a stepwise onset of further anoxic processes such as denitrification. We tried to clarify this in the revised ms.

PISCES, which is an often-used biogeochemical ocean model, considers the impact of denitrification on the marine nitrogen cycle starting at oxygen concentrations below a threshold of 6 μM . Full denitrification sets in at 0.05 μM . This is state of the art. However, considering the latest results, one might even suggest to use 0.05 μM as an upper oxygen threshold in future studies because only at such low oxygen concentration the reduction of fixed nitrogen to N₂ becomes significant. However, this opinion is not yet commonly accepted and we see our ms as a contribution to change this.

Additionally, sulfate reduction could theoretically only occur after the development of anoxic conditions, hence anoxic waters are not necessarily sulfidic (for instance, if nitrate concentrations are high, nitrate will first be used as the terminal electron acceptor during respiration). Defining anoxia as purely sulfidic conditions is consequently misleading.

We agree and wrote: 'the appearance of hydrogen sulfide is generally considered as an indicator of anoxia'

Abstract:

Lines 20-22: This sentence is confusing. What do they mean by "... which includes negative feedback mechanisms reducing the oxygen consumption at decreasing oxygen concentrations."? I assume denitrification/anammox and reduced respiration are the negative feedback mechanisms? I would change for: "which includes negative feedback mechanisms reducing oxygen consumption at decreasing oxygen concentrations (e.g., reduced respiration)."

To clarify the text we added 'which includes negative feedback mechanisms reducing oxygen consumption at decreasing oxygen concentrations (e.g., reduced respiration)'

Text:

Lines 123-135: I would consider a lower O₂ threshold for microbial hypoxia of 10 μM (see my comment above).

As stated above, we defined microbial hypoxia as the range within which anoxic microbial processes can occur and anammox is one of these, and occurs apparently already at higher oxygen concentrations.

Lines 415-416: Why is the reliability of the older data set questioned?

As far as we understood because there have been no other reports on H₂S in the open Arabian Sea and the Bay of Bengal thereafter. Our statement is based on the

work of Sen Gupta, R., Naqvi, S.W.A., 1984. Chemical Oceanography of the Indian Ocean, North of the Equator. Deep Sea Research, 31, 671 - 706.

Lines 575-578: I would cite Fassbender et al. (2018), which offers a concise reviews of mesoscale and sub-mesoscale circulation in the ocean.

Thanks , was done

Lines 618-623: I don't think this mechanism is well represented in Figure 11. Export production seems to be equal in both panels (with and without eddies). **Following the suggestion from Reviewer #4, this and two more figures have been removed from the ms.**

Technical corrections:

Line 254: change to " in combination with the a strong inflow..."
was changed

Lines 924-925: This sentence needs to be revised: "from the lack of oxygen the tolerance to decreasing oxygen, critical concentrations..."

Figure 6: change to: "meridional overturning circulation in the Indian Ocean according to..."

Following also the suggestion from Reviewer #4 this paragraph and figure 6 have been deleted.

Additional references:

Fassbender, A. J., Bourbonnais, A., Clayton, S., Gaube, P., Omand, M., Franks, P. J. S., ... & McGillicuddy Jr, D. (2018). Interpreting mosaics of ocean biogeochemistry. Eos, 99(10.1029).

Frey, C., Bange, H. W., Achterberg, E. P., Jayakumar, A., Löscher, C. R., Arévalo-Martínez, D. L., ... & Oleynik, S. (2020). Regulation of nitrous oxide production in low-oxygen waters off the coast of Peru. Biogeosciences, 17(8), 2263-2287.

Referee # 1

We thank the reviewer for the suggestions, which helped to streamline the ms.

Rixen et al. assemble a comprehensive and useful overview of the state of knowledge of the OMZ in the northern Indian Ocean, specifically the Arabian Sea (AS) and Bay of Bengal (BoB). They address a wide range of topics related to biological and physical dynamics, paleo records, and model results. The authors addressed most of the 3 anonymous reviewers' comments on the original manuscript. Overall, the revised manuscript is more comprehensible, however the revisions did not unify the different sections of the manuscript - the overall structure and content of each section remained the same, which results in repetition and disjointedness.

Some examples:

Sections 2.2 and 3.1 both discuss the link between OMZs and upwelling and parts could be combined;

Sections 2.3 and 7.1 both discuss impacts of OMZ expansion and intensification on biology;

Sections 2.1 and 4 both discuss the physical processes that ventilate the OMZ – from large scale circulation to mesoscale eddies – and these could be tied together somehow.

Combining some of these sections and eliminating repetition would streamline this manuscript and help the reader understand the points the authors' are trying to make.

We agree and

1) eliminated repetition in 7.1,

2) merged section 2.1 and 4, and

3) selected more suitable heading for section 2.2, which was divided in two parts:

2.2 Spatial and temporal variability of the Arabian Sea OMZ

2.3 The Bay of Bengal OMZ.

4) Section 3 was also renamed and includes now both the biological and physical drivers.

All over these changes reduced the length and streamlined the ms.

Specific comments:

Lines 58-60. "The transition from anaerobic...hydrogen sulfide." From the first part of this sentence I expected to read about the steps from anaerobic to aerobic. The second part of this sentence seems to be a non sequitur.

Additionally, sulfate reduction can be coupled to the oxidation of multiple types of reduced carbon, not only methane.

This sentence was deleted

Section 2.1. This entire section is confusing and needs work. The authors first state that both primary production and flux from the atmosphere supply oxygen to the surface (lines 171-172), but then make no mention of the role of primary productivity in affecting the gradient/flux between the OMZ and the surface (lines 194-205). Then it seems like the authors are arguing that vertical mixing ventilates the OMZ (lines 194-205), but then they discuss the importance of lateral mixing (lines 210-211).

Line 172. After "sources of dissolved oxygen", insert "to the surface".

In a first step toward clarification we followed the suggestion to insert 'in surface waters', and secondly the section was shortened and, as suggested, merged with section 4.

Line 253. "Fig. 4c" should be Figs. 3a and 3b.

ok thanks!

Lines 503 and 506, Fig. 7a. "deepening" and "OMZ depth" Do the authors mean "thickening" and "thickness"?

Yes, we changed it accordingly

Line 540. "low oxygen consumption" Should this be "high oxygen consumption"?

Yes, was changed

Lines 622-623. "The consequence is an expansion of the volume of the OMZ." Fig. 11 shows the opposite – eddies decrease the size of the OMZ. I understand that the effect of eddies can be to both increase and decrease the size of the OMZ but Fig. 11 only shows eddies decreasing the OMZ by increasing ventilation and the juxtaposition with the text is confusing.

Figures 4 and 6. These figures do not add much information beyond what is stated in the text. I suggest removing them to help shorten this manuscript.

Figures 11, 5 and 6 were deleted.

Line 728. "Fig. 6a" should be 12b and 12c.

ok

Section 7.1. There is no mention of the BoB.

We added the following sentence:

little is known of its effect on zooplankton distribution and vertical migration and this also holds true for the Bay of Bengal OMZ. ' to the summary of section 7.1 which is now section 6.2.

Line 1291. "section 7.3" should be 7.2.

Great, we changed it

Figure 7a and b. Please provide maximum and minimum ranges for these mean values.

Figure 7b. "mean OMZ oxygen concentration" How is this value calculated?

Volume-weighted average of water <20 micromolar oxygen?

Figure 8a and b. Same as with Fig. 7 – please provide ranges

The data on the aerial extend, OMZ thickness and oxygen concentrations were obtained from Table 5 in Acharya, S.S., Panigrahi, M.K., (2016) and not calculated by us. These authors provide only the standard deviations for the mean oxygen concentrations, which in addition to those derived from the primary production rates were included into the respective figures.