The authors would like to thank anonymous reviewer #2 for the constructive and valuable comments, which will help to improve the manuscript. A point-by-point reply to the comments follows below. The author responses are marked in red.

Review of Rixen et al.

General comment

The manuscript by Rixen et al. gives a good overview of the development of OMZs and recent trends in the Arabian Sea and the Bay of Bengal, and discusses impacts from ocean circulation, export production and mesoscale eddies.
The authors would like to thank anonymous reviewer #2 for this comment

The paper also reviews past and potential future OMZ strength as inferred from the sediment $d^{15}N$ and model predictions and looks at the pelagic and benthic ecosystem responses.
The paper is generally sound and has the potential to present a much needed comprehensive review of the Arabian Sea OMZs. It is however, quite apparent that the different sections were written by different authors as the writing style and the transitions from one section to another are incongruent. The manuscript should be at least partially rewritten to improve the flow. Some sections clearly need editing by an native English speaker (in particular the abstract, introduction, and conclusion). Some sentences are unclear or repetitive. I found several typos through the manuscript. See my technical corrections below for some suggestions on how to improve these sections.
We agree that a restructuring and unified writing style would improve the flow and make our view and arguments more obvious. The referees provided extremely constructive and detail suggestions of how to restructure the manuscript and the native speakers among the authors will unify the writing style.

On another note, the adopted O_2 thresholds defining hypoxia and anoxia are confusing. Several papers use a much higher O_2 threshold to define hypoxia (e.g., >63 µM, Vaquer-Sunyer and Duarte, 2008 and reference therein). The presence of H_2S should rather be referred to as sulfidic conditions, as sulfate reduction does not necessarily occur under anoxic conditions. A clear distinction between Oxygen Minimum Zones (OMZs) and Oxygen Deficient Zones (ODZs) should also be made.
We are aware that in fisheries and in ecology an upper threshold concentration of 60 – 63 µM for hypoxia is well accepted (Ekau et al., 2010; Vaquer-Sunyer et al., 2008). However, from a biogeochemical point of view a 20 µM threshold concentration appears to be more suitable as it marks the concentration below which fixed nitrogen is transformed into N_2. Furthermore, it was used to map the volume of OMZ in the ocean (Acharya et al., 2016). Therefore, we considered 20 µM as upper threshold and anoxia as the lower threshold of hypoxia (zero oxygen). Because oxygen detection limits of classical Winkler titration (~1 µM), seabird sensors (0.09 µM) and the newly developed switchable trace oxygen sensors (STOX, 0.01 µM) is too high to prove anoxia (Thamdrup et al., 2012; Ulloa et al., 2012) the occurrence of hydrogen sulfide was considered as an indicator of anoxia. According to our understanding sulfate reduction is a strict anoxic process. However, we would be very grateful if the reviwer#2 could provide us a reference so that we can integrate this aspect.

Finally, some figures should be added or improved for clarity. For instance, a figure explaining the development of an OMZ in relation to ocean circulation and seasonal monsoons would be helpful (Background, section 2.2).
This will be done.

Specific comments:

In the following we respond to content issues and with short statements to comments and suggestions regarding grammar, spelling, and unclear explanations.

Abstract: The abstract will be rewritten.

Overall, the whole abstract ought to be rewritten to summarize the main points of the manuscript. The current version is confusing, and at times vague. Also, the abstract should follow a more logical order following the order of the different sections as presented in the text.

Line 4: Nitrate loss is only a problem if it is limiting (i.e., in a non-eutrophic system).

Lines 14-16: This sentence is confusing and needs clarification. It should be rephrased to emphasize that, based on previous studies (e.g., Aumont *et al.*, 2015), decreasing oxygen concentration slows down respiration and thus decreases oxygen demand. The following sentence is also unclear as it is.

Lines 19-21: This sentence is too vague. Effects on benthic and pelagic ecosystems should be better summarized.

Introduction

Lines 27-30: This sentence is ambiguously worded.
This will be clarified

Lines 30-32: N_2O is also produced as an intermediate during denitrification and is a by-product of nitrification. N_2O is a greenhouse gas 300 times more potent than CO_2 and an ozone destructing substance and should also be included here.
This will be changed.

Lines 46-48: The availability and quality (organic matter stoichiometry) of organic material is a key control on denitrification versus anammox (Babbin *et al.*, 2014).
The reference will be included.

Lines 60-61: Their definitiona of hypoxia and anoxia are a bit confusing since most studies define hypoxia at O_2 concentrations >63 µM (Vaquer-Sunyer and Duarte, 2008 and reference therein). Anoxia (O_2 concentrations close to zero or in the nmol range) can also occur without hydrogen sulfide production.
See authors comment above

Lines 66-67: I don't quite understand this sentence. Marine ecosystem services need to be defined earlier in the text. I suggest removing this sentence as the next sentence (lines 67-70) articulates the same idea better.
We will rephrase this sentence.

Lines 80-82: The more recent estimates by Eugster and Gruber (2014) of 52 Tg N yr^{-1} for water column denitrification and 93 Tg N yr^{-1} for benthic denitrification should be referenced. The distinction between water column and benthic rates should be made more explicitly.
Lines 84-85: Considering a mean sedimentary denitrification rate by Eugster and Gruber (2014) of 93 Tg N yr^{-1}, the proportion of sedimentary denitrification at the Pakistan continental margin could be even higher.
This reference will be included.

Main text

Lines 124-151: A figure showing the impact of ocean circulation in relation to seasonal monsoon on OMZ expansion in the eastern and western Arabian Sea would be helpful.

Lines 144-146: Is this low areal extension associated with increased thickness of the ODZ, as shown in Figure 4? This should be clarified here.
In summer the low areal extension is associated with deepening of the OMZ. This is caused by the enhanced carbon export and reflects also the deepening the mixed layer due to downwelling, which occurs in the central Arabian Sea during the summer. This will be clarified.

Lines 166-168: Why this ballast-effect mostly occurring in the Bay of Bengal and not the Arabian Sea?
It operates in both basins but since the lithogenic matter content in sinking particles is higher in the Bay of Bengal than in the Arabian Sea, the ballast effect is stronger in the Bay of Bengal.
We will change 'The ballast-effect' into 'A stronger ballast effect'.

Lines 180-182: A distinction should be made between human-induced coastal eutrophication and coastal dead zone development due to the imbalance between higher O_2 consumption from primary productivity (upwelling) relative to O_2 supply from physical circulation.
This will be change to:
The spatial expansion of hypoxia, which seems to be an increasingly common feature in coastal waters, is called the "spreading of dead zones" (Altieri et al., 2017; Diaz et al., 2008) which expresses the threat of oxygen-depletion to oxygen-breathing organism, and more specifically to fisheries. This global phenomenon is a consequence of eutrophication and global warming whereas eutrophication increases oxygen consumption by enhancing the production of organic matter and global warming decreases the oxygen supply due to a reduced solubility of oxygen in warmer waters.

Lines 198-199: A reference is needed to support this O_2 threshold.
We will delete this part of the text.

Lines 191-193: How does the relatively low denitrification rate estimated by Bristow *et al*. (2017) compares to the denitrification rate (including anammox) measured in the Arabian Sea using ^{15}N-labeled incubations by Ward *et al.* (2009)?
Bristow *et al*. (2017) measured a rate of 0.9 nmol L^{-1} day^{-1} at one site while Ward et al. (2009) measured rates of > 20 nmol L^{-1} day^{-1} (denitrification) and up to approximately 5 nmol L^{-1} day^{-1} (anammox). These numbers will be included into the ms.

Lines 215-217: This is an important point that should be described better in the abstract. Ok

Line 229: The term (central Indian Ocean) is already defined in the previous section. Ok

Lines 267-268: I don't quite understand this sentence either. Do they mean in contrast to the upper part of the SNM?
This sentence will be rephrased.

Lines 277-279: How does figure 3 support this point?
It supports this point in so far as it illustrates fluxes of free and protected organic carbon as calculated according the equations introduced by Armstrong et al. 2002 and our sediment trap data. However, this will be clarified.

Lines 319-323: The roles of coastal mode water anticyclonic eddies as N-loss hotspot in the Peru upwelling system should also be referenced (Bourbonnais *et al*., 2015; Altabet and Bourbonnais, 2019). The paper by Fassbender *et al.* (2018) also provides a good review of the effects of mesoscale and submesoscale features on ocean biogeochemistry.

These references will be included.

Lines 339-342: On which timescale are these feedbacks expected to occur?

These feedbacks occur on a relatively short timescale (i.e., years) as the reduction in the suboxic core volume and denitrification happens mostly in the upper 200-400m.

Lines 379-381: These two terms "eddy-driven isopycnal tracer mixing" and "isopycnal flattening" need to be explained.

The isopycnal tracer mixing refers to the mixing of tracers by eddies along isopycnal (or neutral) surfaces. This is the diffusive part of eddy mixing that is typically parameterized in coarse-resolution models following the Redi scheme (Redi, 1982). On the other hand, the isopycnal flattening refers to the advective effect of eddies that acts to adiabatically flatten the slope of isopycnal surfaces. The latter is typically represented in models following Gent and McWiliiams parameterization (1990). The explanation of the difference between the two components will be added in the revised manuscript.

Lines 419-420: The authors should be more specific about which results they are referring to (Bristow *et al*., 2017).

Ok , this will be clarified.

Line 471: A reference is needed to support this O_2 threshold for denitrification. Dalsgaard *et al.* (2014) report an O_2 threshold in the nmol range for denitrification.

The mentioned O_2 threshold for denitrification refers simply to the value used in the PISCES model (erroneously put at 5 rather than 6 µM – this will be corrected). However, a discussion about thresholds will be included into the background section. It will be pointed out that denitrification sets in at oxygen concentrations > 6 µM but becomes significant for the N-cycle only at oxygen concentrations of about 0.05 µM. A discussion of how models deal with these thresholds will be included.

Lines 486-488: The model's results do not seems to support denitrification during the Holocene.

As stated in lines 467-469 (see also Segschneider et al. 2018 in references) the Kiel Climate Model/PISCES, as all GCMs, simulates higher oxygen concentrations than observed at present in the Arabian Sea. Despite this difference in total oxygen concentrations, the Holocene trends (oxygen decreases) and the spatial differences can be reproduced by KCM/PISCES. We therefore surmise, that the model results can give us hints at the driving mechanism of oxygen decline (increasing age of OMZ water mass).

Lines 497-498: Was a relationship between orbital forcing (i.e., Milankovitch cycles) and the development of the OMZ ever investigated in the region? A reference should be added.

Bopp et al. 2017 investigated this for LGM – 6k time slice experiments (relation to our experiment discussed in Segschneider et al., 2018), but to our knowledge there are no further transient experiments but ours that are investigating this over the Holocene for the Indian Ocean. We will add Bopp et al. 2017 to the references.

Lines 515-519: Submesoscale processes, which are ephemeral and take place over lengths of about 1-10 km lasting several days, are also poorly represented (see Fassbender *et al*., 2018).

This is true. Processes that are even smaller and more short-lived than mesoscale processes are also poorly represented in such coarse resolution models. We will address them too, as they are connected to eddy transport and play a crucial role in distributing nutrients in frontal zones.

Lines 600-616: What is the effect of these larges blooms on OMZ expansion?
This is an interesting aspect, which will be integrated into the discussion but feedbacks of these blooms on declining oxygen concentrations have so far not been studied.

Lines 702-705 and 715-720: At which oxygen thresholds are these community composition and faunal abundance changes observed?
Clear changes in community composition and abundance have been reported across oxygen gradients both on OMZ margins (in the Indian Ocean and elsewhere, e.g., Gooday et al, 2009; Levin et al, 1991, 2000, 2009a,b), as well as in hypoxic basins such as the Baltic Sea, and at sites impacted by excess organic matter input (e.g. Rosenberg, 2001). Systematic changes occur in the lifestyles and feeding modes of the benthos across these gradients, sometimes showing depth ranges/areas with exceptional abundances of as few as one species (e.g., Jeffreys et al, 2012). This has been attributed to the combined effects of oxygen (differing tolerance to slight changes in low oxygen levels) food availability, and predation avoidance (e.g. Levin et al). Consequently, and because of common lack of *in situ* bottom-water oxygen measurement, or differences in the sensitivity/precision of oxygen measurement across studies, specific oxygen thresholds have been difficult to constrain. Further, while similar general patterns are observed (e.g., on different margins of the Arabian Sea), local differences occur in the individual species that show abundance peaks across oxygen gradients. However, the threshold observed for a change from foraminifera to metazoan macrofauna in the uptake and processing of organic matter, based on isotope tracer studies across the Pakistan margin of Arabian Sea, occurred at an oxygen concentration of 5-7 μM (Woulds et al 2007).

Rosenberg, R (2001) Marine benthic faunal successional stages and related sediment activity. *Scientia Marina*, 66, 107-119. Other cited references are in the manuscript bibliography.

Lines 745-748: What is the N:P ratio in the overlying ODZ versus the sediments? Lower N:P ratios than expected based on NO_3^- loss and biogenic N_2 production during denitrification are often observed in coastal ODZs due to the preferential release of PO_4^{3-} following iron and manganese oxyhydroxide dissolution in anoxic sediments (Noffke *et al*., 2012).
It is not clear what information is being sought, or how it would be relevant to the statement made in the text (which refers to the N:P stoichiometry of benthic nutrient fluxes). The N:P ratios in these fluxes differ due to the net effects of multiple benthic processes occurring across the cross-margin gradients in bottom-water redox conditions (as per the referee's comment). To what ODZ sample type, and from what depth, would one compare sediment N:P ratios (in solids or porewaters?); water or suspended or sinking particles? While water-column nutrient analyses have been commonplace in both the Arabian Sea and Bay of Bengal, this is less true for particulates. There have been various sediment trap deployment programmes in the Arabian Sea over the decades, and also some in the Bay of Bengal, and some of these have included deployments within the OMZ. However, these have generally been at different (single) OMZ depths. There has been even less systematic sampling of suspended particles, and neither sample type has had routine determinations of both N and P.

Line 750: Define "dark" carbon.

Dark carbon refers to the carbon fixed in aphotic zones by autotrophic organisms. This will be defined in the manuscript.

Lines 797-800: Higher oxygen concentrations are more likely the results of the development of a sharper pycnocline (from higher freshwater fluxes) and lower primary productivity in the Bay of Bengal.

Conclusion: The conclusion will be rewritten

Lines 800-802: This sentence is unclear. Do they mean that mesoscale eddies sustain higher O_2 concentrations in the OMZ than expected in their absence?

Figures: AOU should be showed instead of Δoxygen (∝mol kg^{-1}) in this figure since this is what is discussed in the text. Something must be wrong with the scale for Δoxygen (∝mol kg^{-1}). The Δoxygen (deviation from O_2 concentrations at saturation) should be much higher than 10 ∝mol kg^{-1} to cause hypoxic/anoxic conditions.
Why is the figure broken into two panels (a, b)? Another suggestion is the break the axis for depth >500 m.
Doxygen is not the AOU, it is the change of oxygen concentrations with depth. It will be removed from the figure and the axis will be broken.

Figure 4. It is unclear how to reconcile data in Figure 2 - showing that overall a decrease in the OMZ area seems to correspond to a decrease in the mean OMZ oxygen concentrations (at least during summer monsoon when POC flux is highest) and Figure 4 - showing a negative correlation between OMZ max thickness and the mean OMZ oxygen concentration.

The circulation mainly affects the areal extension while the carbon export into the deep sea and the oxygen concentration seem to exert the main influence on the thickness. Due to the different drivers the thickness does not correlate with areal extension. This will be clarified in the text and the figures will be changed accordingly.

Figure 5. This figure is difficult to read (white font on light blue background). Font size should be bigger. Isopycnal mixing by mesoscale eddies could be emphasized in a.
This will be changed

Figure 6. Make d as a symbol (y axis): "δ^{15}N"
Figure will be changed.

Technical corrections:
The abstract will be rewritten

Line 4: Change "increases the loss nitrate" to "increases nitrate loss"

Lines 4-5: Change to "Nitrate is a macronutrient limiting primary productivity in most of the ocean."

Lines 7-10: This sentence seems to be out of context and repetitive considering the following sentence. I suggest rewriting:
"The main control on oxygen concentrations in the Arabian Sea and the Bay of Bengal is the balance between physical oxygen supply and biological oxygen consumption from respiration. Mesoscale eddies greatly enhance mixing and advection of O_2-rich waters, which compensate biological consumption and overall reduces ODZ expansion."

Lines 12-14: Change to: "However, due to slightly higher oxygen concentrations, aerobic nitrite oxidation outcompete anaerobic nitrite reduction and thus limits denitrification in the Bay of Bengal"

Line 39: Replace "At" with "Under" at the beginning of sentence. Ok

Lines 62 and 64 and 806: Change "hyp-" for hypoxic here and everywhere else in the text.Ok

Line 74: Remove "is": "..., with a much smaller proportion ~~is~~ located in the Bay of

Bengal..."Ok

Line 83: Replace "to this data" with "published data". Ok

Line 90: Change to: "one of the least understood

OMZs"Ok

Line 183: Replace "conational" for "continental" Ok

Line 190: Replace "nitrite oxidization" with "nitrite oxidation" Ok

Line 201: Replace "this is with about 0.7 ∝M much higher" by "it is about 0.7 ∝M higher". Ok

Line 202: Remove the at beginning of sentence: "However, ~~the~~ in comparison to the Arabian Sea..." and remove "as in the Arabian Sea" at the end of sentence. Ok

Lines 208-209: Replace with: "Subsequent studies also reported decreasing oxygen concentrations in the western and northern Arabian Sea." Ok

Line 255: Replace with: "in the upper part of the seasonal thermocline..." Ok

Line 258: Replace "stabile" with "stable" Ok

Line 270: Replace "SNN" with "SNM"

Ok

Lines 279-281: Therewith is used twice within the same sentence. Ok

Line 286: Remove "in": "... is mostly remineralized within ~~in~~ the upper 300 m..." Ok

Line 292: Remove "also" Ok

Line 297: Replace "the hypothesis" by "this hypothesis" Ok

Line 439: This term (ICW) is already defined earlier in the text.

Ok

Lines 534 and 539: Change for 80 ∝M O_2 and 50 ∝M O_2. Ok

Line 573: Replace for: "... can survive at O_2 concentrations down to 4.5 ∝M" Ok

Line 593: Remove one "waters": "... nutrient-enriched ~~waters~~ subsurface waters..." Ok

Line 617: Add a space after Gomes *et al.* (2014).
Ok

Line 630: Remove one "of": "... the capacity of the ~~of~~ endosymbionts..."

Line 685: Replace with: "... will have implications for the cycling of nutrients and oxygen..."

Line 797: Change to:"... to a degree that ~~is~~ prevented denitrification..."

Lines 797: Start new sentence with "In": "~~The~~ In comparison to the..."

Additional references:

Altabet, M. A., & Bourbonnais, A. (2019). N-loss stoichiometry in a Peru ODZ eddy. *Journal of Marine Research*, *77*(2), 169-189.

Babbin, A. R., Keil, R. G., Devol, A. H., & Ward, B. B. (2014). Organic matter stoichiometry, flux, and oxygen control nitrogen loss in the ocean. *Science*, *344*(6182), 406-408.

Bourbonnais, A., Altabet, M. A., Charoenpong, C. N., Larkum, J., Hu, H., Bange, H. W., & Stramma, L. (2015). N-loss isotope effects in the Peru oxygen minimum zone studied using a mesoscale eddy as a natural tracer experiment. *Global Biogeochemical Cycles*, *29*(6), 793-811.

Eugster, O., & Gruber, N. (2012). A probabilistic estimate of global marine N-fixation and denitrification. *Global Biogeochemical Cycles*, *26*(4).

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).

Noffke, A., Hensen, C., Sommer, S., Scholz, F., Bohlen, L., Mosch, T., ... & Wallmann, K. (2012). Benthic iron and phosphorus fluxes across the Peruvian oxygen minimum zone. *Limnology and Oceanography*, *57*(3), 851-867.

Vaquer-Sunyer, R., & Duarte, C. M. (2008). Thresholds of hypoxia for marine biodiversity. *Proceedings of the National Academy of Sciences*, *105*(40), 15452-15457.