The authors would like to thank anonymous reviewer #1 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 'Present, past and future of the OMZ in the northern Indian Ocean'

Rixen et al provide a review on the comparison of the two oxygen minimum zones in the northern Indian Ocean, located in the Bay of Bengal (BoB) and the Arabian Sea (AS). The two basins are compared from an oceanographic and biogeochemical point of view. This is obviously a challenge and I acknowledge that it is never an easy task to synthesize results from different disciplines and authors into a coherent piece of writing. To me the manuscript is a valuable contribution, however, it needs some more streamlining and integration of the different sections.
That said, I have some very general remarks, which I hope will help to streamline the manuscript:
I am not sure what is meant by hypoxic, in order to be able to stick with it may be helpful to define a range of oxygen concentrations you are referring to.
Upper threshold concentrations of dissolved oxygen, which are used to define hypoxia vary. In fisheries and in ecology upper threshold concentrations of 60 – 63 µM are well accepted (Ekau et al., 2010; Vaquer-Sunyer et al., 2008). However, from a biogeochemical point of view, 20 µM appears to be a more suitable threshold concentration as it marks the concentration below which fixed nitrogen is transformed into N2. Furthermore, it was used to map the volume of OMZ in ocean (Acharya et al., 2016). Therefore, we considered 20 µM as the upper threshold of hypoxia and anoxia as the lower threshold of hypoxia. 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 has been considered as an indicator of anoxia. This will be specified in the revised version.

I am not quite sure what the aim of the study is, and what it specifically contributes as a stand-alone publication. From the title, I would expect to learn about potential expansion and intensification patterns of the two OMZs based on an assessment of past developments on geological timescales. This can, to a certain extent, be distilled out of the paper but I could imagine that if the authors take some effort and work through the paper once more, it would be more obvious.
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 detailed suggestions of how to restructure the manuscript and the native speakers among the authors will unify the writing style.

I also expected to learn about why those two basins behave so differently- there are different reasons given, including stratification, which is credible for a certain part of the BoB but not visible anymore in the offshore OMZ as presented by Bristow, further a ballasting effect by riverine particles present in the BoB and absent in the AS. The latter doesn't convince me, because it seems to be a coastal phenomenon only.
Since in comparison to the Arabian Sea, the Bay of Bengal is poorly studied it is difficult to compare these two basins process by process. However, we agree that a restructuring and a unified writing style would make our current understanding of mechanisms causing the differences between these two basins much clearer.

Bristow et al. (2017) presented data from seven stations in northern Bay of Bengal, which were obtained from one cruise carried out in January 2014. We doubt that this data suffice to prove that the offshore OMZ in the Bay of Bengal is no more stratified. Furthermore, the oxygen profiles presented in Figures S1 by Bristow et al. (2017) show to our understanding a clear stratification within the upper water column. This is less evident from sigma-t due to scale used, which ranged from 0 and 40 while within the regarded T/S range sigma-t varies approximately between 21 and 26 (see e.g. Schott et al. 2001).

We also do not agree that the ballasting effect by riverine particles is only a coastal phenomenon in the Bay of Bengal. The lithogenic ballast effect operates everywhere where lithogenic matter is incorporated into sinking particles. It is stronger in the Bay of Bengal than in the Arabian Sea beyond shelves and slopes as indicated by a higher contribution of lithogenic matter to the total flux in the Bay of Bengal. This was shown by sediment trap studies carried in the Bay of Bengal and the Arabian Sea. Rixen et al. (2019) provides more detailed information on these sediment trap studies.

Regarding the different oxygen concentrations in the two basins, a steady state between physical oxygen supply and biological oxygen consumption is also given. The slightly higher oxygen concentrations in the BoB is suggested to promote a feedback between nitrate reduction and nitrite oxidation. This is based on Bristow et al mainly, which

is one study with 5 stations during one time of the year. In order to strengthen your case, it may be beneficial to also consider Canfield et al. (2019) and Löscher et al. (2020), both of which propose alternative feedbacks possibly stabilizing the BoB's remaining oxygen traces. I understand that those studies may have come out after the presented paper was submitted and may have not been visible enough.
Other results on OMZ oxygen production as suggested for other regions (Garcia-Robledo et al., 2017) may be worthwhile considering given the abundance of small unicellular cyanobacteria as described for the BoB. As for the assessment of deep time changes, reference to the work of Orsi et al. (2017) could be helpful.
Thank you very much for providing these references which will complement our review.

In addition, but this indeed may go beyond the scope of the manuscript, a discussion on possibly changing monsoon intensities and atmospheric dust inputs could be interesting for a future assessment.
This is a very interesting aspect but we agree with the reviewer that these topics are beyond the scope of our current work.

Right now, it is obvious that the sections have been written by different authors, with sections 1-3 needing a native speaker to improve the language. I understand that the first author coordinated the writing and I know that this is an ungrateful job. However, there needs to be some more coherence regarding the writing style, the level on which the different topics are presented, and again, some more integration of the different sections to improve the reading flow.
As mentioned before we agree with the reviewer and the native speakers among the authors will unify the writing style.

I also have some specific comments and suggestions:
In the following we respond to content issues and with 'ok' to comments and suggestions regarding grammar and spelling.

Title: there is a comma missing between present and past. Ok
The abstract will be rewritten

l. 2 'is' should be changed to 'are'; 'it favors' should be changed to 'they favor'. I also do not quite understand the use of the (admittedly modern) expression 'ecosystem services'
l. 4: change 'which' to 'and its'
l. 8/10 past tense is used- is this because it refers to the geological past?

Introduction
l. 25: 600 mio years is a bit short. Canfield, Lenton and Lyons give different ranges, but they are about 2.3-3.2 billion of years for the rise of oxygen.
The sentence refers to the rise of oxygen to the nearly present day level and the occurrence of algae and planktonic cyanobacteria. This will be clarified.

27 ff I am not sure what this means? Are you suggesting those are the only habitats of anaerobic organisms? Because they are quite abundant throughout the marine water column on particles see e.g. (Ganesh et al., 2015;Ganesh et al., 2014). In addition, nitrate reduction to N2 can happen via anammox- in this case one could more or less claim those are anaerobic microbes. Denitrifiers are not anaerobic microbes, they are facultative and respire oxygen when possible. This part will be deleted.

l. 39 'of' is missing before 'oxygen' Ok
l. 49 'expense' Ok
l. 62 Here, a definition of hypoxic and anoxic would be helpful. Also, this way to abbreviate looks very awkward. Change to 'inhibit', 'prevent' Ok – regarding the definition, please see the authors comment above.

l. 66 'of' before 'anaerobic' is missing Ok
l. 70 a reference to work by Schmidtko et al. (2017) and Keeling et al. (2010)is missing
Schmidtko et al. (2017) and Keeling et al. 2009 will be added (we assumed that 2009 instead of 2010 was meant)

l. 75'margins' Ok
l. 77 Again, this needs a definition of hypoxia. see above

l. 85 ff a reference to Naqvi et al. (2010) is missing.
Line 845 ff describes sedimentary denitrification rates. Naqvi et al. 2010 is about iron-limitation and only mentioned that decreasing nitrate and nitrite concentrations indicated an intense denitrification.
However, this aspect will be mentioned in the revised ms.

l. 166 How do the different primary producer communities look? How is the food web- wouldn't this also be important to make claims about export fluxes?
The impact of ballast minerals on the export is well-studied whereas the influences of primary producers and food-web structures on the carbon export is not well-constrained. We are also not aware of any work describing the impact of primary producers on the carbon export in the Indian Ocean. On the other hand, there are papers quantifying the impact of ballast minerals on carbon export and we summarized their results (see the authors comment above).

Also, if we have a faster export, would the a more anoxic sediment or deeper water layer be expected?
In principle we agree but this depends on many more factors such as bottom water oxygen concentrations and the composition of sediments. The Bay of Bengal is a deep-sea fan where in addition to vertical, a lateral supply via deep-sea channels also matters (e.g. Galy et al.2007). However, organic carbon accumulation in the Bengal fan is high which supports the assumption that the ballast effect increases the export of organic matter and its preservation in sediments.

Galy, V., France-Lanord, C., Beyssac, O., Faure, P., Kudrass, H., Palhol, F., 2007. Efficient organic carbon burial in the Bengal fan sustained by the Himalayan erosional system. *Nature*, 450, 407-410.

This statement is also somewhat contradictory to the claim made based on Bristow et al, that a microbial feed back stabilizes the trace oxygen concentrations.
To our understanding Bristow et al. (2017) suggested first of all a microbial feedback that operates at low oxygen concentrations and prevents nitrite reduction from becoming significant. However, this will be clarified.

I would suggest mentioning the reasons for the difference in OMZ intensity in a way that is less exclusive and so that they can complement each other. The way it is, it is confusing.
We will add an additional paragraph on steady states in a previous chapter, which includes the various processes controlling oxygen concentrations in the water column.

l. 174 this needs a reference ok
l. 174 ff The statement is unclear, I think you are talking about a sulfidic event when saying anoxia,
Ivanenkov , V.N., Rozanov, A.G., 1961 report the occurrence of H2S in the NE Arabian Sea within the secondary nitrite maximum. This implies that the H2S originated in the water column and was not produced in the underlying sediments. Since the occurrence of H2S indicates anoxia, we would say it was an anoxic event at which H2S was formed.

l. 176 if this is an 'only report' why do you have three references?
Because they all agree that it is the only report.

l. 178 'don't seem to evolve every year' Ok
l. 179 Who dies during those mass mortalities? Please replace ' in between' with 'occasionally'. How

confident are we that those mass mortalities do not result from trace metal contaminations from the land?
We went back to the original papers and found out that fish mass mortalities occurred in back waters only. Therefore we deleted the term 'mass mortalities'.

Nandan, S.B., Azis, P.K.A., 1995. Fish Mortality from Anoxia and Sulphide Pollutions. *Journal of Human Ecology*, 6, 97-104.

L 183 'also' could be removed, sounds awkward. Ok
l. 184 change 'were' to 'was' Ok
l. 187 Actually, Bristow shows microaerobic processes to occur Ok
l. 190 ff awkward sentence, please rephrase Ok
l. 192 Please add an explanation what excess N_2 measurements are good for. I don't think a non-N cycle expert can possibly know that. This part will be deleted.
l. 199 'outcompetes' Ok
l. 202 remove 'the', change 'rate implies' to 'rates may explain' This part will be deleted.
l. 204 ff, l. 206ff Please rephrase- awkward sentences. Ok
l. 208 'Follow-up studies also reported' Ok
l. 212 ff Schunck et al didn't report on periodic outbreaks but on a one-time event, another report from the same region would be Callbeck et al. (2018), both references combined may give some hint for a regular occurrence. The term 'periodic' will be deleted.
l. 215 this may also just be a result of the monitoring program. If no one went there to measure one wouldn't find it either. We agree!

l. 220 remove 'the' before 'biological' Ok
l. 221 'This approach is based on' Ok
l. 223 what does 'regarded' mean here? The water masses of interest? Ok
l. 232 'approximately' Ok
l. 238/ 239 I don't understand this statement. This statement will be deleted.

l. 258 'isotope ratio' Ok
l. 261 ff it would be helpful to explain which values are typical for denitrification and other processes
We will add an additional figure to show this.

l. 262 change 'indicates to 'is located in' Ok
l. 268 ff this statement doesn't make sense to me, the reference is also maybe not ideal.
This will be changed.

l. 270 'SNM' Ok
l 272 'suggests' Ok
l. 273 ff What is the purpose of this statement?
The statement will be deleted.

l. 279 'within' Ok
l. 280 'key factor' Ok

l. 315 This could benefit from a reference
Generally, I was missing references to work on eddies in Atlantic OMZ waters and their relevance for oxygen budgets and biogeochemistry (Fiedler et al., 2016;Karstensen et al., 2017;Schütte et al., 2016), especially in lines 366 ff.
These references will be included. We will also add the following two references to support the statement

in line #315 (McWilliams, J.C., 2008, *Ocean modeling in an eddying regime*, and 2) McGillicuddy Jr, D.J., 2016. *Mechanisms of physical-biological-biogeochemical interaction at the oceanic mesoscale*.)

l. 391 remove 'the' Ok
l. 392 what is meant by 'nitrogen'? N_2, organic or inorganic nitrogen species?
Organic nitrogen in sediments is meant.

l. 392-405 this section would benefit from an explanation of what those values mean. This will be included
l. 397 remove 'the' Ok
l. 407 the core has the lowest oxygen concentrations?
It is not the core but the water column above the site at which the core was taken. This will be clarified.

l. 419 this sentence seems to be missing something. This will be clarified.
l. 428 'an onset' Ok
l. 422 ff, this part would benefit from observations by Orsi et al. (2017) This reference will be included.
l. 429 ff I don't understand the purpose of this statement This will be clarified.
l. 430, the abbreviation ICW is only explained in l. 439. ICW was already defined in the chapter 'background'.
l. 433 what does BP stad for? (before present – will be clarified)
l. 436'surfac- derived oxygen-rich water' Ok
l. 448 Kiel Climate Model, introduce the abbreviation as you use it later on, also this needs a reference. This will be added.
l. 454 explain what PISCES stands for
PISCES stands for 'Pelagic Interactions Scheme for Carbon and Ecosystem Studies) and is a biogeochemical model which simulates the lower trophic levels of marine and the biogeochemical cycles of carbon and of the main nutrients

Aumont, O., Ethé, C., Tagliabue, A., Bopp, L., Gehlen, M., 2015. PISCES-v2: an ocean biogeochemical model for carbon and ecosystem studies. *Geosci. Model Dev.*, 8, 2465-2513.

l 485 'the' before 'late' Ok
l. 513 what does that mean that it is backward? Replace 'oxygen values' with 'oxygen concentrations or saturations' whatever is appropriate
We rewrote this sentence to make it clearer:
"In most ESMs the east – west contrast between the Arabian Sea and Bay of Bengal is opposing to what observations show, with most global models producing lower oxygen concentrations in the Bay of Bengal than in the Arabian Sea."

l. 516 there are high resolution options including mesoscale dynamics in CMIP6
Analyzing the performance of the CMIP6 models in the northern Indian Ocean would exceed the scope of this paper. And as far as we know there hasn't been a publication on that so far that we could refer to. As we do not want to speculate here, we only could include the outlook that the future generation of ESMs is targeting that problem.

l. 519 Isn't it rather a general problem that there is no circulation model available?
Here we are not quite sure of what is meant: Do you mean that the large-scale circulation in the Indian Ocean is not well represented in the ESMs or that the parameterization of the mesoscale processes in the Indian Ocean is insufficient?

L 579 ff this section is lengthy and could lead better to the point
L 612 'Arabian Sea' ok
L 721 what is and 'edge effect'?

The “edge effect” concept is described previously, on L 702-705.

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