Reply to the comments by Reviewer#2

We thank the reviewer for reading through the manuscript and offering several comments and suggestions. In this reply, comments by the reviewer are given in black font and replies in blue.

Reviews and syntheses: Physical and biogeochemical processes associated with upwelling in the Indian Ocean

Vinayachandran et al. described twelve upwelling regions in the Indian Ocean. The description starts with upwelling systems associated with coastal currents in the southwestern part of the Indian Ocean, followed by those largely driven by the Asian Monsoon and ends with those associated with coastal currents off Australia in the southeastern part of the Indian Ocean. Additionally, the authors discussed the Seychelles-Chagos Thermocline Ridge (SCTR), which is an upwelling system that develops in the open ocean between 5 and 15°S in the western part of the Indian Ocean.

Writing a review and synthesis paper on upwelling systems in the Indian Ocean is a great idea and I am very pleased by the efforts of the authors. However, to my understanding, a review and synthesis paper should provide a comprehensive overview without overloading the reader with details and aspects, which are interesting but not directly related to the topic. Considering this demand, the individual chapters of the paper reveal pronounced disparities. For instance, there are chapters such as the Introduction and chapter 2.6 which do not reflect the state of the art whereas e.g. chapters 2.1, 2.9, and 2.12 include so much additional information that they could most be stand alone papers on the Agulhas Current, the Bay of Bengal and the oceanography along the Australian west coast. Hence, to my understanding, the current version of the manuscript needs a major revision.

We thank the reviewer for reading the manuscript carefully and offering several constructive comments. We have revised the paper in the light of the reviewer's comments and all the issues raised by the reviewer have been addressed. Reply to each of the reviewer's comments are given below.

A revised version of the paper should include an Introduction clarifying terms, which thereafter are used coherently throughout the manuscript. Furthermore, chapters on the individual upwelling regions should focus on upwelling and exclude regional characteristics, which are not directly related to upwelling. It would also be helpful to summarize discussions on upwelling mechanisms since after reading about it from two different perspectives (modeling vs. observations), the general conclusion often remains unclear. In sum, I believe that uniform subdivision of all chapters into three sections such as Background, Mechanisms, and Productivity and Ecosystem, as seen e.g. in chapter 2.4, would help to shorten the manuscript, which is with 128 pages quite long. Simultaneously, it would increase the readability of the manuscript.

We agree with the reviewer on including definition of all clarifying terms; in the revised version of the paper, all such terms have been defined appropriately. We have also limited the descriptions to those regional features that are relevant to the upwelling. In the revised version, uniform subdivision of chapters into the general framework suggested by the reviewer based on Background, Mechanisms and Productivity and Ecosystem has been adopted for describing the upwelling systems.

In the following I will provide arguments and suggestions whereas I will start with the Instruction and continue, thereafter, with chapter 2.

Introduction

The authors subdivided the Introduction into three sections describing upwelling (i), biogeochemical implication of upwelling (ii) and the study area (iii). This is a convincing structure but the section on upwelling contains only two references; one from 1905 and the other one from 1937. This is insufficient to introduce the reader into our current understanding of upwelling, which includes the formation of coastal parallel jets, meso-scale eddies and offshore-directed filaments, as well as interactions of these features with remotely and locally trigged waves. After reading an introduction to upwelling, I would expect that the reader exactly knows what the authors mean with 'classical upwelling dynamics' (see lines 745 – 746). Furthermore, it is crucial to define the following terms in order to better understand the description of the individual upwelling regions and, in particular, the differences between these regions:

Ekman upwelling, wind-driven coastal upwelling, core upwelling, shelf-edge upwelling, slope upwelling, divergent upwelling, dynamic upwelling, topographic-driven upwelling, dynamic boundary upwelling, current-driven upwelling, upwelling wedge, current-induced upwelling, upwelling Kelvin waves and upwelling node.

We respect the reviewer's suggestion that the introduction could be updated to introduce the current understanding of the topic. The introduction has been revised to include coastal parallel jets, mesoscale eddies and offshore-directed filaments and the interaction of them with wave propagations. The terms listed by the reviewer has also been defined in the paper.

To emphasize the role of upwelling on marine ecosystems and ecosystem services such as fisheries, the authors refer to Eastern Boundary Upwelling Systems (EBUS) in the second section of the Introduction. This is convincing since it emphasizes the relevance of upwelling in general, as well as the uniqueness of the Indian Ocean where such an EBUS is not established. However, this approach suffers from the poor discussion of EBUS. In the following, a couple of references are listed which provide to my understanding nice overviews on EBUSs:

We thank the reviewer for pointing out the relevant reference on EBUS. The description on the EBUS has been expanded into a separate paragraph to include the features of upwelling systems mentioned in the review. References about production in the EBUS has also been included in the revision.

Kämpf, J. & Chapman, P. in Upwelling Systems of the World (Springer International Publishing, 2016).

Messié, M., Chavez, F.P., 2015. Seasonal regulation of primary production in eastern boundary upwelling systems. Progress In Oceanography, 134, 1-18.

Chavez, F.P., Messie, M., Pennington, J.T., 2011. Marine Primary Production in Relation to Climate Variability and Change. Annual Review of Marine Science, 3, 227-260

Messié, M., Ledesma, J., Kolber, D.D., Michisaki, R.P., Foley, D.G., Chavez, F.P., 2009. Potential new production estimates in four eastern boundary upwelling ecosystems. Progress In Oceanography, 83, 151-158.

Carr, M.-E., 2001. Estimation of potential productivity in Eastern Boundary Currents using remote sensing. Deep Sea Research Part II: Topical Studies in Oceanography, 49, 59-80.

Carr, M.-E., Kearns, E.J., 2003. Production regimes in four Eastern Boundary Current systems. Deep Sea Research Part II: Topical Studies in Oceanography, 50, 3199-3221.

Bakun, A., 2017. Climate change and ocean deoxygenation within intensified surface-driven upwelling circulations. Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences, 375, 20160327.

Brady, R.X., Lovenduski, N.S., Alexander, M.A., Jacox, M., Gruber, N., 2019. On the role of climate modes in modulating the air–sea CO2 fluxes in eastern boundary upwelling systems. Biogeosciences, 16, 329-346.

Zuidema, P., Chang, P., Mechoso, C.R., Terray, L., 2011. Coupled ocean-atmosphere-land processes in the tropical Atlantic. Joint edition of the newsletter of the Climate Variability and Predictability Project (CLIVAR) exchanges and the CLIVAR variability of the American Monsoon System Project (VAMOS), 55, 12 - 14.

Chavez, F.P., Toggweiler, J.R., 1995. Physical estimates of global new production: The upwelling contribution. In C.P. Summerhayes, K.-C. Emeis, M.V. Angel, R.L. Smith, B. Zeitschel (Eds.), Upwelling in the ocean, modern processes and ancient records (pp. 313-320). Chichester: Wiley & Sons.

Regarding the study area, I would suggest to go beyond mentioning papers and describe climate, ocean currents, and climate anomalies such as Madden-Julian Oscillation, El Nino Southern Oscillation (ENSO) and the Indian Ocean Dipole.

This indeed is a very good suggestion. However, we feel this this would make the paper much too long. Moreover, there have been reviews and synthesis of the above topics published earlier (Xie et al., 2009), the most recent one by Hood et al., (2015).

Furthermore, it would be extremely helpful to include maps showing the general circulation and primary production as well as the location of the upwelling region, which the authors discuss in chapter 2. Also, in this context it is crucial to define and standardize the used terms. For instance, select only one term for: northeast monsoon; NE monsoon; northeast Monsoon; NEM; North-East Monsoon which should also indicate whether it refers to boreal or austral regime. This is just one of many examples making it difficult to follow the description of the upwelling systems.

A new figure (Figure 1B) has been included which shows a schematic of the circulation during summer and winter. The background of this Figure is satellite derived chlorophyll data which presents the major upwelling regions along the coast. We have also adhered to the terms of northeast monsoon (NEM) and southwest monsoon (SWM) throughout the paper.

Chapter 2 Coastal Upwelling Systems

General remarks:

Since the Seychelles-Chagos Thermocline Ridge (SCTR) is an upwelling system that develops in the open ocean, I would suggest adding an additional chapter on upwelling systems in the open ocean. In this chapter the SCTR could be discussed, as well as the equatorial upwelling in the Indian Ocean. So far this upwelling region has not sufficiently been considered by the authors, even though same papers on this upwelling system have been cited.

We have moved SCTR to section 3 under open ocean upwelling. We could not find sufficient material to warrant a separate section for the equatorial Indian Ocean. The equatorial upwelling is restricted to the eastern boundary and this has been included in Section 2.11

Instead of describing the remaining elven upwelling regions in dependence of their geographical location, it would also be helpful for the reader if the author could subdivide chapter 2 into sections, which relate to e.g. driving forces. Such overarching sections could be: (i) monsoon-driven upwelling systems, (ii) upwelling systems associate with coastal currents and (iii) upwelling systems strongly influenced by freshwater fluxes such as those in the Bay of Bengal and off Sumatra/Java. This is just a suggestion, but I am convinced that defining overarching types of upwelling will help the reader to better understand upwelling in the Indian Ocean.

We were unable to find a better organization in terms of driving forces. Therefore, we have opted to retain the present organization.

Within chapters 2.5 to 2.9 contents are mixed up. For instance, the authors discussed iron-limitation in chapter 2.5 line 697 – 699 and cited Naqvi et al. 2010. According to the reference list, this is a paper entitled ‘Marine hypoxia/anoxia as a source of CH4 and N2O’ but I guess the authors mean:

Naqvi, S.W.A., Moffett, J.W., Gauns, M.U., Narvekar, P.V., Pratihary, A.K., Naik, H., Shenoy, D.M., Jayakumar, D.A., Goepfert, T.J., Patra, P.K., Al-Azri, A., Ahmed, S.I., 2010. The Arabian Sea as a high-nutrient, low-chlorophyll region during the late Southwest Monsoon. Biogeosciences, 7, 2091-2100.

Thanks for pointing out this mistake. This has been rectified in the revised version. We have also organized these sections and repetitions have been eliminated.

This paper discusses data measured off the Arabian Peninsula and along a transect across the central Arabian Sea. Hence, if the authors want do discuss iron limitation, it belongs to chapter 2.6. Furthermore 'iron limitation' is a controversially discussed issue. Similar to results derived from numerical models, also studies based on observations are coming to opposing conclusions regarding iron limitation. To my understanding, such opposing conclusion should have also been considered by the authors. See e.g. Rixen, T., Goyet, C., Ittekkot, V., 2006. Diatoms and their influence on the biologically mediated uptake of atmospheric CO2 in the Arabian Sea upwelling system. Biogeosciences, 3, 1 - 13.

The reviewer has rightly pointed out that the conclusion of Naqvi et al. (2010) was based on observations made along a transect from the coast of Arabia to the central Arabian Sea. Hence, including this statement in the Section for the "Somali upwelling system" may cause confusion. This statement was made to elaborate on possible mechanisms that limit the phytoplankton bloom in this region. We noted that most of the observation-based hypotheses on productivity limitation were a spin-off from the JGOFS observation and therefore, limited to the coast of Arabia and in the central Arabian Sea (see, for example, Rixen et al., 2006; Naqvi et al., 2010). Whereas, model studies suggest that the iron limitation is not only limited to the coast of Arabia but also along the coast of Somalia (Wiggert et al., 2006; Wiggert and Murtugudde, 2007). Hence, suggesting that the conclusion based on observation collected off the coast of Arabia may also be true for the coast of Somali. The citation of Naqvi et al. (2010) was, in fact, in this sense mentioned in the Somali region.
However, we agree that the statement was misleading a bit, primarily due to lack of a link between the discussion on model studies and the observation-based studies. Since, in the revised version, we intend to re-organize the structure of the sections by combining the observation and model-based studies, all the limiting processes discussed in the literature (the original version discussed quite extensively about other limiting processes as well) will be arranged coherently for a better flow and more definite conclusions. Nevertheless, we will include Rixen et al. (2006) and its conclusions in the revised texts.

In chapter 2.6 (line 806) the authors wrote: 'a detailed discussion on biophysical interactions along this coast is discussed in Section 2.7.5.' It makes really no sense to discuss biophysical interactions along this Arabian Coast in a chapter on upwelling along the Indian coast! Please spilt chapter 2.7.4 and 2.75 and discuss biogeochemical implications in association with the respective upwelling regions. In chapter 2.9.2 the authors discuss general biogeochemical characteristics of the Bay of Bengal, but it remains open to which extent this relates to upwelling! To my understanding this is also a main problem of chapters 2.1 and 2.11 as mentioned already before.

We agree with this suggestion by the reviewer and the contents of sections 2.6 and 2.7 have been re-organized to be self-contained. Chapters 2.1, 2.9 and 2.11 also will be revised to eliminate topics that are not relevant to upwelling.

In the following, I will focus on chapter 2.6 and 2.8 because these chapters suffer from missing contents and describe upwelling system, which are comparable to EBUS. Upwelling in the western Arabian Sea is, perhaps in addition to the one off Somalia, the only major upwelling system in the Indian Ocean with an impact on marine productivity, which is comparable with those of EBUS. The upwelling region off Indonesia, in turn, is the only large upwelling system associated with an eastern boundary current in the Indian Ocean.

The authors introduce chapter 2.6 with a hint to the enormous work, which was carried out in the framework of JGOFS with respect to upwelling in the western Arabian Sea. This included modelling and fieldwork whereas the fieldwork had also a strong focus on biogeochemical implications of upwelling. I guess that > 6 'Arabian Sea special issues' have been published. Sharon Smith edited many of these special issues and worked a lot on zooplankton dynamics in upwelling system. Other groups studied phytoplankton successes including their link to nutrient dynamics in surface waters, primary production and export production, as also recently summarized by Rixen et al. 2019a,b and references given therein. Hence, moving this topic to chapter 2.7.5 is incomprehensible and weakens the discussion on ecosystem shifts. To my opinion, ecosystem shifts as well as overfishing, are among the largest threads to upwelling systems and aspects which should be considered in the discussion of each upwelling system in case the availability of data allows it. Goes and Gomes published a number of papers on ecosystem shifts, and the one belonging to chapter 2.6 have been cited in chapter 2.7. Please see also their latest work: Goes et al. 2020.

We thank the reviewer for these thoughtful comments. Yes, we agree that the separation of biogeochemical response to the Section 2.7.5 distracted the discussion. In the revised version, we propose to scrap section 2.7.5 and bring the relevant discussions in Section 2.5-2.7.
Regarding a comprehensive review of the studies based on US JGOFS observations, the review paper by Schott and McCreary (2001) and Hood et al. (2017) provide great detail on these findings. Hence, in this manuscript, we tried to focus on some of the aspects that were not quite highlighted in the earlier reviews and keep repetition as minimum as possible. The same was stated upfront in the introductory paragraph of this section in the original submission.
We agree that the topics like "ecosystem shifts" and "overfishing" were not fairly discussed in the original submission. Considering that these are the pressing challenges in the fast-changing world, we also believe that a better discussion is necessary. We will try to address this in the revised version and would like to thank the reviewer for providing some of the important references in this

Goes, J.I., Tian, H., Gomes, H.d.R., Anderson, O.R., Al-Hashmi, K., deRada, S., Luo, H., Al-Kharusi, L., Al-Azri, A., Martinson, D.G., 2020. Ecosystem state change in the Arabian Sea fuelled by the recent loss of snow over the Himalayan-Tibetan Plateau region. Scientific Reports, 10, 7422.

Rixen, T., Gaye, B., Emeis, K.-C., 2019. The monsoon, carbon fluxes, and the organic carbon pump in the northern Indian Ocean. Progress In Oceanography, 175, 24-39.#

Rixen, T., Gaye, B., Emeis, K.C., Ramaswamy, V., 2019. The ballast effect of lithogenic matter and its influences on the carbon fluxes in the Indian Ocean. Biogeosciences, 16, 485-503.

Chapter 2.10. According to authors 'The upwelling off the southern coasts of Sumatra and Java Islands is a remarkable eastern boundary upwelling system (EBUS) in the Indian Ocean'. To my understanding this is not correct. Even though upwelling associated with an eastern boundary current develops off Indonesia, it reveals fundamental differences to other EBUS going beyond aspects discussed by the authors. The development of EBUSs is associated with the development of deserts on land. In contrast to this coevolution, Indonesia is characterized by extremely high river discharges. Freshwater fluxes establish e.g. capping effects similar to those in the Bay of Bengal and an ENSO-driven seesaw linking upwelling off Indonesia in the Indian Ocean to those in the equatorial Pacific Ocean. These are important aspects, which, to my understanding, should be considered in a review paper.

We thank the reviewer for these suggestions, the distinguishing characteristics of upwelling along the Sumatra and Java coasts have been described in Chapter 2.10, by revising this section.