

## ***Interactive comment on “Positive Indian Ocean Dipole events prevent anoxia along coast of India” by V. Parvathi et al.***

**Anonymous Referee #1**

Received and published: 26 August 2016

The paper by Parvathi et al. investigates the seasonal and interannual variability of dissolved oxygen off the West Coast of India (WCI) using a coupled physical-biogeochemical regional simulation of the North Indian Ocean. More specifically, the study documents the recent variability in the oxycline along the WCI and explores its potential drivers. It is shown that the seasonal as well as the year-to-year fluctuations in the depth of hypoxia (and hence the likelihood of occurrence of anoxic events in the near-surface coastal waters) are controlled to a large extent by the variability of the depth of the thermocline, and hence are essentially physically driven. Authors show that oxycline like thermocline is shallowest in fall, thus eventually allowing anoxia to develop and reach the coastal region during this period of the year. Finally, it is shown that the year-to-year fluctuations in the oxycline (and hence the potential severity of hypoxia) in the WCI region is partially controlled by the Indian Ocean Dipole (IOD). This

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is because of the sensitivity of the thermocline in this region to coastal Kelvin waves generated by easterly wind anomalies associated with IOD. Yet this effect is only strong during positive IODs where the downwelling Kelvin waves generated by easterlies near the southern tip of India lead to a deepening of the oxycline that prevents anoxia. In contrast, during negative IODs the effect is of smaller amplitude and hence is less important. The authors conclude that these findings have important implications in terms of the predictability of fall anoxia along WCI.

General comment:

The subject of the paper is highly relevant in the general context of quantifying and better understanding the drivers of naturally occurring coastal anoxia and what controls its year-to-year variability. The paper is well written and presents important new findings. The experimental design is appropriate despite some limitations (e.g., model resolution). For these reasons I recommend the publication of this manuscript after improving the presentation and the discussion of certain aspects of the study, namely the potential role of biology in contributing to O<sub>2</sub> variability and the implications of the model low-resolution for the conclusions of the study. Here below I detail my comments:

Specific comments:

page 2, line 16: “The frequent anoxic conditions that occurred. . .”

page 4, line 13: The 1/4 horizontal resolution does not allow a proper representation of coastal upwelling either. The underestimation or potential misrepresentation of coastal (wind-driven) upwelling in the model needs to be mentioned and its potential implications discussed.

page 5, lines 9-20: in addition to the limitations discussed by authors, the CaTS dataset comes from a single coastal site and hence may not be representative of the dynamics of the whole WCI. This needs to be stressed.

page 6, 2nd equation: all parameters (R1o:c, R2o:c, etc., . . .) should be explicitly defined

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in the text.

page 6, Is sediment respiration represented in the model? Please specify. Also, how are denitrification and anamox represented in the model?

Page 9, lines 14-15: I don't think the effect of biology on oxygen is small at the thermocline depth. The respiration fluxes can be very large at 100m. The fact that the oxycline and the thermocline show high correlations may result from the fact that biology itself is constrained by vertical physics and as a consequence that the nutricline and the thermocline are tightly coupled. This statement needs to be reformulated and the role of biology further discussed (authors may consider quantifying the individual contributions of biology and physical transport to oxygen variability).

Page 9, lines 18-22: It is not clear why the correlation between the OCD and the TCD is low in the southern and the southwestern parts of the domain. In the southwestern part authors mention a possible role of lateral advection there, but it is not clear how this might affect the correlation between these two quantities. In the equatorial region authors suggest the low correlation may be due to the definitions used for oxycline and thermocline that may not be meaningful there. Yet, in section 2.4 (page 8, lines 28-31) it is stated that the results and the conclusions of the study are insensitive to the oxygen (50-150 mmol/m<sup>3</sup>) and temperature (20-25C) criteria used to define these two depths. These two statements appear to contradict each other.

Page 9, lines 25-27: during the spring inter-monsoon, the (northwesterly) winds along the WCI are also upwelling-favorable. Yet, the thermocline is relatively deep there (especially in observations). This is an indication that TCD alone cannot be used as a proxy for wind-driven coastal upwelling (probably because of the non-local effects of the coastal Kelvin waves).

Page 10, lines 4-5: in the northern part of the WCI, alongshore (southward) winds seem to be upwelling-favorable. Maybe plotting seasonal time series of upwelling index together with OCD and TCD would help figuring out how these three quantities co-vary

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(can be added to Fig 7).

Page 10, lines 24-32: the overestimation of oxygen in the WCI may not only be due to the coarse representation of the shelf dynamics but also the general tendency of the model to underestimate the intensity of the OMZ in the northern Arabian Sea (see Fig 4 for example). This needs to be discussed.

Page 13, lines 7-8: Identifying the drivers of coastal anoxia requires further investigation. In particular, the role of biology and the effects of O<sub>2</sub> consumption on the shelf need to be quantified and contrasted with the impact of vertical and lateral advection. It is not clear how much of anoxia is driven by large-scale advection of O<sub>2</sub>-depleted water (from the Arabian Sea OMZ) vs. local consumption of O<sub>2</sub> due to respiration in the water column and on the shelf.

Page 13, lines 17-20: Any explanation for the asymmetry of the impacts of positive and negative IODs (and in particular the potential reason for why the wind anomalies are weaker during negative IODs)?

Page 15, lines 11-14: As acknowledged by the authors, the (relatively) coarse resolution of the model (1/4) is probably the main limitation of the study. The underestimated or misrepresented coastal upwelling and mesoscale eddies in the model may have important consequences. I encourage the authors to further discuss the implications this might have and how it may (or may not) affect their conclusions/results.

Page 15, lines 11-16: if the model does not represent sediment processes, the lack of benthic respiration could also limit the model ability to represent the dynamics of coastal hypoxia along the shelf. This needs to be clarified.

Figure 3: the colorbar is missing.

Figure 3: since the study focus is on coastal anoxia that develops during fall, maybe it would be good to show how the model reproduces chlorophyll during this season.

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