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

Anonymous Referee #2

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General comments (shared)

Ultimately, I think that there could be a linkage whereby positive IOD occurrence influences OCD conditions along the SW coast of India. The authors have taken a great stab at establishing this link but have fallen a bit short, in my opinion, of establishing the case. Significantly tightening up the analysis, following the suggestions I have made in specific comments below, would go a long way toward achieving a reasoned, well considered analysis that can at least strongly suggest such a linkage is present. It may be that additional data, refinement of this analysis in terms of key locations for exploring causality and higher resolution modeling with nested shelf regions, will be required to fully reveal what is tantalizingly indicated in the current effort.

Specific comments

Introduction, p. 2 & 3. The bottom paragraph of p. 2 and top paragraph of p. 3 have diverted from providing background information of the research to be undertaken to an initial summarizing, drawn from figure 1, of aspects of the study that is being reported on. This leads to an awkward shifting between presenting problem framework and results that interferes with coherent, logically developed reporting. I would recommend that all call outs to Fig. 1 be eliminated from the Introduction section. This could be simply accomplished by shifting the two noted paragraphs out, but note that there is at least one additional callout to Fig. 1 to address. As to the content of these two paragraphs, I do have additional specific comments. In the first of these paragraphs, the authors first introduce the notion that the open ocean OMZ of the Arabian Sea has connectivity to the hypoxia that manifests along the west coast of India. From my knowledge of the literature, and from the literature the authors' have cited, this has so far only been posited and in my view is more anecdotal than proven fact. The 1/4° model solution that the authors use as principal basis of their findings is challenged to demonstrate causality on this point. The authors should acknowledge this explicitly, make a more compelling case based on existing literature, or leverage this in from some of their higher resolution modeling efforts.

Page 5, line 20. In the last sentence, it is interesting to note that these observed instances of full anoxia will be featured/discussed later in the paper but the figure callout should not be included. Could probably accomplish this through merging of that point into the prior sentence and eliminating the remainder.

Page 6, line 8. In this equation describing the biological source / sink terms of DO, it would be interesting to note what is done to ensure negative concentrations of dissolved oxygen are not achieved in the model.

Page 9, lines 1-2. The text here is a bit confusing. At the top of the paragraph it is stated: "... we used the standard Dipole Mode Index ... ". From the subsequent text in this section that indicates DMI based on model SST is calculated, I think what is meant is " ... the standard DEFINITION of Dipole Mode Index ... " is used.

Page 9, line 6. " to the choice of either of them" is awkward. "which is chosen" would work better.

Page 9, line 26 -> top page 10. In the model during MAM, Fig. 6e shows that OCD is not really uniform in eastern AS; and OCD off Mumbai is comparable in value to OCD at bottom of the subcontinent. This directly contrasts what is described in the text. It also dampens the scenario described as a clear-cut influence of coastal KW propagation northward of the shoaled OCD / TCD condition that initiates in the STI region. I believe that the issue here is that care needs to be taken to make clear that description is centered on WCI to STI region and not relevant north of $\sim 10^{\circ}$ where alongshore wind forcing is distinct from what exists farther south.

Page 10, line 8. I think it is stretching what can be elucidated from the model results and presented figures to say that the OCD/TCD pattern is clearly suggestive of faster planetary wave propagation at lower latitudes. Lag associated with coastal KW propagating northward could also factor in.

Page 10, bottom paragraph. The linkage of offshore OMZ to low coastal DO is again a thread in this part of text (see earlier remarks on my reservations).

Page 11, lines 3 - 17, and figure 9. There are a number of interesting features in figure 9. Some comment on the inverse correlation regions appearing in 9a would be interesting to include and may provide some mechanistic insight to IOD-associated biophysical interaction that is the crux of this analysis. Some comment on the several instances where model TCD and observed SLA are out of phase in the 2002-2006 period (9b) would also be potentially illuminating (model issue?, sensitivity to WCI box definition?). Assessing any limitations in either of these is key to explore and characterize for the reader to fully trust results stemming from this analysis.

Page 11, lines 19-20. Details of how anomalies are determined would be useful to document; caption of Fig. 10 is an option if this is not substantial enough to stand alone in methods section. Also, on line 20 panels a-c are noted as regression maps, which

is contradictory to the information in the Fig. 10 caption (and plot labeling) that states panel c is a correlation map. For both of these data reductions details of how they are performed are not documented, making it problematic for the reader to correctly self-determine his/her interpretation. As for grasping why distinct analysis method was applied to zonal wind stress relative to how the other variables (OCD, TCD, SST) were treated, that is even more of a challenge for the reader to intuit.

Page 11, lines 19 - 23 and caption for Fig. 10. The terminology used in referring to the derived fields described here, and the terminology in the captions that accompany the associated distributions in Fig. 10, is inconsistent. Specifically, the narrative notes that all variables are internally varying anomalies but in the caption this is not obvious. In the caption, the data shown in panels a-c are noted to be regressed against "normalized oxycline interannual anomalies averaged over WCI box". My interpretation is that this describes the ts shown in Fig. 11a. If this is indeed the case, noting that as such would be very helpful to the reader as a way to better grasp what is presented in Fig. 10. This may entail modifying figure ordering w/in the ms. I find it interesting that the regression of OCD with OCD(WCI) in the WCI box (panel 10a) is actually relatively low compared to elsewhere in the IO domain. I think some interpretive commentary from the authors would be very interesting to see. Further, as I questioned earlier, does this have implications for the sensitivity / utility / robustness of the WCI box as a foundational component of this analysis? I would like to see the authors critically assess and comment on the choices they have made in setting up and carrying out their analysis.

Page 11, line 24. For clarity and to benefit the reader, please explicitly associate / define how shallower / deeper OCD anomaly relates to + / - OCD anomaly.

Page 11, lines 25-31. The KW propagation patterns noted here are consistent with what is reported in the literature. However, the pathways and timings that are discussed are not identifiable in the Fig. 10. Appropriate referencing of the literature should be given to support what is stated. The remote forcing aspect of thermocline dynamics in the northern IO is quite complex; I strongly encourage the authors to make the effort to clearly articulate and document what is known and how it relates to their study. I believe this would be highly appreciated by the IO readership.

Page 11, line 31. Should specify that the correlation referred to here is negative.

Page 12, lines 4-8. The correlation distribution pattern for taux (Fig. 10c) is different enough that I would hesitate to even characterize it as "reminiscent of IOD signature"; in particular the sign shift in correlation that is apparent in the NE Bay of Bengal and eastward / off equator shift of strongest correlation (i.e., away from Sumatra coast where + IOD signature is most pronounced). Note as well that this is positive IOD signature. And related to that call for clarification, it would be nice to include an example of negative IOD manifestation to accompany the example of positive IOD (Fig. 5d).

Page 12, line 6. Here, the 10 d-f sequence is collectively referred to as regression maps, which is inconsistent with labeling / reporting elsewhere in narrative.

Page 12, lines 8-9. I think it is a stretch to make the statement that these figures, in particular panel 10d which is the key for illustrating the point, demonstrate a "strong link" between OCD in WCI region and IOD. In my view the level of regression in 10d for the WCI region is marginal and certainly much less pronounced than elsewhere (e.g., the Sri Lanka Dome).

Page 12, lines 10-11. The correlation between model-based DMI and OCD(WCI) is interesting and suggestive of what the authors are trying to demonstrate (i.e., causal link between OCD and DMI). I would strongly suggest that this correlation also be performed with standard DMI product, for a couple of reasons. While the authors did report earlier in the ms that the standard DMI and their model-based DMI were largely similar and choice of which is applied did not substantially affect the results, performing the correlation with standard DMI provides grounding to a well-known established index and would mitigate any concerns that may/should arise for the reader vis a vis internal bias inherent in a model-model comparison.

Page 12, lines 14-21, and Fig. 11b. This is an interesting distillation of results, though I still have reservations of robustness similar and related to what has been noted in comments above. I think it would be useful to find a way to split out the DMI values such that those that pass threshold and can be classified as positive / negative IOD are distinct from those that do not (i.e., normal and IOD states are clearly delineated). The regressions that are shown do not add much insight, but if they are retained then they should be performed only on points that are +/- IOD states and not inclusive of all positive of negative values. However, even doing this, I am skeptical that particularly insightful result will be obtained. For the data that are shown that do represent + / - IOD occurrence, there is not really an associated systematic deepening / shoaling of OCD. Certainly the negative IOD case has a number of either OCD result. But there is also a positive IOD case with (slightly) shallowing OCD. It also appears that the years with observed and notable anoxic events tend to be during negative DMI (thought not necessarily negative IOD) but there is also a positive DMI case and a case that is almost uniformly normal (i.e., DMI $\sim$ 0 and OCD $\sim$ 0). Given these results, I do consider that the data are suggestive of positive IOD events influencing appearance of low oxygen in WCI but would be hesitant to make categorical conclusions. Overall, I think the authors need to be more even handed when reporting what is revealed in Fig. 11b.

Page 12, lines 21-27, and Fig. 12. This figure is also quite interesting. It suggests that both IOD phases lead to positive OCD and TCD displacement during SON, although for negative IOD this may not be statistically significant. What I find curious is that taux during SON in STI that has opposite sign between positive and negative IOD. Would this not lead to opposite oceanic response (i.e., upwelling vs. downwelling) between the two IOD phases, which then presumably has contrasting impact on thermocline displacement if this response translates northwards as coastal KW as authors have argued? The authors do note that taux condition for negative IOD is not as "robust" as that for positive IOD, however the relative values of anomaly for October (highest magnitude for SON period) are not strikingly distinct. Which begs the question as to how much OCD / TCD response one can ascribe to STI wind stress condition.

Page 12, slide 30. Instead of “identified fifteen years ago”, reiterating citation of relevant source material would be best.

Page 20, line 8. Title for this reference is incorrect.

Comments on Figures

General comment. There are several figures that include isolines superimposed on a second variable where a color scale is applied. In almost all cases, there is a need for additional labeling of the superimposed contour lines on the plot so these distributions can be grasped. There is also a need to note the contour intervals in the associated captions.

Figure 4. For panels 4b and 4d, the red contour line demarking thermocline is extremely difficult to see. In addition, the isotherms at depth (DO < 80 microM) are also difficult to see. For panels 4a and 4c, the 15° N line is difficult to spot as well.

Figure 9. In panel 9a, white color in the IO domain has non-unique meaning. It can be either masked data or the transition from positive to negative correlation value. A way of uniquely distinguishing these should be used. Additionally, the white land mask is also not ideal but does have benefit of land-sea boundary.

Panels 11a and 12 a-c. The combination of blue and black line colors is ineffective. Very difficult to distinguish between these two.

END OF REVIEW

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