Anonymous Referee #2

General comments

This is a well executed investigation into the contrasts of IOD and ENSO on biological variability in the Indian Ocean and was a pleasure to drill into. The biggest shortcoming relates to the authors' not being more accommodating to a reader that does not take up their manuscript with an already developed understanding of the topic and familiarity with the analytical tools that are applied. The specific comments provided below cover much of the needs related to this issue. The one additional suggestion I would make is that the authors review their text very carefully to ensure that precision in terminology is adopted, so that their message is clear. In particular, the nomenclature that is employed when discussing the partial regression and residual results needs to be be uniformly applied.

Similarly, for the chlorophyll acronyms (IChl, SChl and CHL), it should be clear whether or not these are anomaly fields. The reader should not be left to work this out over the course of the narrative.

We thank the reviewer for his/her positive comments. We will undertake to carefully review the text and figure captions with a focus to make it more digestible to non-experts of the topic and analytical tools. In addition, we will focus on the precision of terminology and ensure consistent use thereof throughout the manuscript. Except for Figure 1, all analyses make use of, and refer to, chlorophyll anomalies. We will clarify this in the text.

Specific comments

Pg. 5844, Lines 21-24. This sentence is too complex as is, please split to make meaning simpler to follow.

We will break this passage into shorter sentences as follows: "This mode is commonly referred to as the Indian Ocean Dipole (IOD) mode, even though contention exists over whether it should be referred to as a "dipole" (Baquero-Bernal et al., 2002; Hastenrath, 2002). A positive event is associated with anomalous easterly winds in the central Indian Ocean and cold SST anomalies off the south and west coasts of Java and Sumatra."

Pg. 5847, Line 1. The correct term here is first optical depth (rather than first attenuation depth). This is approximately 37% of the surface irradiance [e.g., Loisel and Stramski, 2000].

We will change the term to 'optical depth'.

Pg. 5847, Line 3. Rather than "filling" suggest to use " capable of revealing". This would read more smoothly and convey the intended meaning, I believe.

We agree and will make the change.

Pg. 5851, Lines 15-21. Suggest tabulating these domain boundaries rather than stringing them out in the text. A table would be much simpler to process for the reader, and easier to find and refer to. This table could also include the boundaries used for the subregions used later in the manuscript, which are shown on Figure 6. The acronyms used for region definitions are all straightforward, except for TRIO. The bounds given for this indicate that it encompasses the western half of the tropical Indian Ocean or maybe the western portion of the SCTR (Seychelles-Chagos Thermocline Ridge). In any case, how TRIO was obtained is not at all clear.

*We will tabulate the boundaries in the revised manuscript. The TRIO acronym referred to the '**t**hermocline **r**idge in the southwestern **I**ndian **O**cean' and is a legacy from a previous paper that some of the co-authors were involved in (Jayakumar et al., 2011). We are happy to rename it the western Seychelles-Chagos Thermocline Ridge (wSCTR) region in the revised submission.*

Pg. 5851, Lines 23-25. This sentence is difficult to comprehend and took some time to grasp. My understanding is that this is referring to the climate indices (DMI and Nino3.4) used in this analysis. The phrase “standardized (adimensional) indices” is not clear, in part because adimensional is not a word in English. I believe the intention is to say non-dimensional; further standardized is probably better stated as normalized. My suggestion to rephrase this sentence would be (save fomatting the chlorophyll units):

Since nomalized (non-dimensional) climate indices were used (i.e., DMI and Nino3.4), the regressions provide values (e.g., mg m-3 for SChl) that correspond to the “typical” anomalies associated with IOD and ENSO.

One last remark, please clarify what is meant by “typical” here. Is this intended to reflect that values obtained from the regression techniques will be in a similar value range (i.e., ∼ -2 to 2)?

To clarify this sentence, we propose to reword it in the revised manuscript as follows: “As climate indices (i.e., DMI and Niño3.4) were transformed so as to have zero mean and unit variance, regression coefficients are in units of the response variable (e.g., mg m^{-3} for SChl), and their value corresponds to the change in the response variable that would be expected from a climate anomaly of magnitude 1.”

Pg. 5852, Lines 2-3. Should enfold CHL, DMI and Nino in parentheses. For the latter, is this the Nino3.4 index that was mentioned on the previous page? Presumably so, but would be good to definitively state that here. Also, the choice of reference to the ENSO index (i.e., Nino vs. Niño (vs. Niño3.4)) should be unified throughout the manuscript.

We will make use of parentheses as suggested. The only ENSO index reported in results is indeed the Niño3.4 index. We will make this clear in the text and refer to it in a consistent manner throughout.

Reviewer #1 also requested some clarification on this part of the manuscript and we have undertaken to improve it. Please see the response to reviewer #1 (pg 2 of that response) for details of how we plan to address this, beyond the specific responses here and below.

Pg. 5852, Lines 1-6. Split this text into two sentences. This description is challenging enough to follow without the reader also having to process a long, complex sentence.

We will split up the long sentence and simplify as far as possible.

Pg. 5852, Eq. 1-3. The form of these equations is inconsistent with their description in the text. Based on the text, which makes better logical sense to me, they should be of the form:

r.CHL = CHL - a.Niño

In response to this and the next comment, we suggest to re-structure and expand on the equations, as well as adjust the surrounding text as follows (pg 5852, lines 1-11):

"For example, to compute the partial regression between a time series of chlorophyll anomalies (CHL) and the IOD index (DMI), independently of the ENSO index (Niño3.4), one first subtracts signals that are linearly related to Niño3.4 from CHL and DMI (equations 1 and 2 below), thereafter regressing the residual chlorophyll time series ($r.CHL_{-E}$) on the residual DMI time series ($r.DMI_{-E}$), to provide an estimate of chlorophyll variability (CHL_{noENSO}), that is related to IOD without the effect of ENSO:

$$r.CHL_{-E} = CHL - a * Niño3.4 \quad (1)$$

$$r.DMI_{-E} = DMI - b * Niño3.4 \quad (2)$$

$$CHL_{noENSO} = r.CHL_{-E} - c * r.DMI_{-E} \quad (3)$$

The reciprocal partial regression was also performed, removing the DMI signal from Niño3.4 and (in this case) CHL, before regressing their residuals to obtain an estimate of chlorophyll variability (CHL_{noIOD}) that is related to ENSO without the effect of IOD:

$$r.CHL_{-I} = CHL - a * DMI \quad (4)$$

$$r.Niño3.4_{-I} = Niño3.4 - b * DMI \quad (5)$$

$$CHL_{noIOD} = r.CHL_{-I} - c * r.Niño3.4_{-I} \quad (6)$$

The letters a, b and c represent regression coefficients and the chlorophyll anomalies (CHL) were *substituted with D20 and SST anomalies to compute the respective partial regressions of those variables.*

To follow up on the equations, beyond their definition on p. 5852 they do not seem to be referred to subsequently. As a mechanism to improve clarity, I would suggest that where these residuals appear later in the graphics or tables that callouts to the equation numbers be included so the reader can track back. This would serve to reinforce the application of the methods developed for the analysis. Of course, to develop this properly would necessitate that the reciprocal cases also be explicitly documented in the set of equations (i.e., removal of IOD signal from ENSO and CHL (page 5852, lines 11-12). The advantage to such a fully developed treatment would be a much clearer explanation for the reader; plus this comprehensive documentation would be available in the literature for subsequent analyses to refer to.

As shown above, we will insert the reciprocal cases of the equations and include callouts to the equation numbers where the partial regression coefficients are reported.

Pg. 5853, Line 26. Need to be precise in terminology. Elevated biomass (high chlorophyll) is not synonymous with high productivity.

We will replace the word 'productivity' with 'chlorophyll concentration'.

Pg. 5854, Lines 1-2. Some comment on the +Chl anomaly in the Norther AS in DJF in the model that is inconsistent with SeaWiFS distribution would be of interest. Is this time/space of the model solution subject to the same issues as already noted or does it suggest additional considerations?

We will add a sentence acknowledging this inconsistency with observations in the revised version. We have no clear explanation for it at this time. We will investigate how the results (in Tables 1 and 2, Figs. 9 and 10) differ for the western Arabian Sea (WAS) box if we adjust the box boundaries, so as to exclude this feature. If these results are materially different, we will update the manuscript accordingly.

Possibly related to this point and others on the model-SeaWiFS comparison. Why is the temporal frame of model Chl anomaly (1990-2000) not made consistent with that of SeaWiFS (1998-2009) (as stated in the caption of Figure 2)? This could actually lead to vastly different results given that three + IODs were active in the 1990s time frame, including the prominent 97/98 event that is partially avoided based on the temporal bounds noted for determining the SeaWiFS climatology.

A similar point was raised by reviewer #1 and we have responded to it in detail there (including how we will update the manuscript). In summary: Due to the temporal limitation of the ERA40 forcing fields, the simulation could only extend until 2001, thereby overlapping the SeaWiFS period by just over four years. We preferred to use a 10-yr period to estimate the average climatological *season. Using the common 1998-2001 period to estimate the climatology provided very similar results, as shown in our response to reviewer #1.*

Pg. 5854, Lines 7-11. The Behrenfeld et al. [2009] analysis is also relevant in this context, and reinforces that iron limitation in the tropical Indian Ocean is broadly relevant to the open basin.

We will add the suggested reference.

Pg. 5856, Lines 9-11. The phrasing at the end of this sentence is awkward and needs revision.

The sentence will be split and re-written as follows: "Our results indicate a less-extensive and weaker ENSO influence on tropical thermocline variations than that of IOD. This finding is consistent with Rao et al. (2002) who suggest that interannual thermocline variability in the tropical Indian Ocean is governed by the IOD."

Pg. 5860, Lines 1-2. The content of Figure 10 requires explanation, which could be accomplished here. This presentation is not so straight forward to interpret. The authors should take time for a few sentences to ensure the reader can navigate and ingest this information. In the process, the acronym NS should be formally defined.

We will insert a short explanation of Figure 10 and define the NS acronym (= non-significant).

Pg. 5861, Lines 12-15. It seems to me that this text is discussing SBoB (not "central part of the Bay"). The central BoB shows a negative D20 anomaly (fig. 4f) and neutral IChl (fig. 7e). I gather that the text here is referring to the feature in the SE BoB. Further, the question of whether an upwelling RW is responsible, needs further support that could be provided by the model results. And a contrast to the interpretation in Wiggert et al. (2009) for this feature's appearance during the 97/98 IOD would be interesting to include (i.e., typical downwelling RW that suppresses thermocline in SE BoB in DJF was disrupted by IOD dynamics).

As correctly assumed, the central part of the SBoB was implied and not the central part of the bay, we will correct this mistake. The D20 anomaly that induces the increased IChl content is quite convincingly associated to an upwelling Rossby wave, visible on Fig. 4f. Our interpretation of this feature agrees with that of Wiggert et al. (2009) and we will add a reference to their study in this passage.

Related to the IOD impact on BoB thermocline, the negative anomaly that intensifies and appears to propagate CCW around the Bay from SON through DJF is intriguing, though not featured as part of the author's analysis. Would be very interesting to see the authors consider and interpret this aspect of their results (i.e., Figs. 4b, 4d, 4f and 4h).

The negative D20 anomaly that propagates counter-clockwise around the rim of the Bay is the *signature of a coastal trapped Kelvin wave in response to IOD wind forcing. Rao et al. (2010) and Nidheesh et al. (2012) have previously referred to this signal in their studies. The resultant upwelling feature is associated with an increase in chlorophyll concentration in the northern part of the Bay (Fig. 7c and d). Brief mention and discussion of this interesting feature will be added to the updated manuscript. We thank the reviewer for pointing it out.*

Pg. 5862, Lines 15-16. Unsure what is meant by "weaker than normal SChl and IChl anomalies". This is same sign but lower magnitude or sign reversal?

This should read "...negative SChl and IChl anomalies..." and will be updated in the revised version.

Pg. 5869, Lines 17-19. The details for this bibliographic entry are incorrect.

We had used details from an early online publication version, but have updated the correct details.

Page 5887, Figure 9 caption. Rather than referring to line type as plain, solid would be more appropriate.

We will change accordingly in all relevant figure captions.

Technical comments

None.

Suggested References

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Loisel, H., and D. Stramski (2000), Estimation of the inherent optical properties of natural waters from the irradiance attenuation coefficient and reflectance in the presence of Raman scattering, Appl. Opt., 39, 10.1364/ao.39.003001, 3001-3011.

END OF REVIEW

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Rao, S.A., Behera, S.K., Masumoto, Y., Yamagata, T., 2002. Interannual subsurface variability in the tropical Indian Ocean with a special emphasis on the Indian Ocean Dipole. Deep Sea Research Part II: Topical Studies in Oceanography 49, 1549–1572.

Wiggert, J.D., Vialard, J., Behrenfeld, M.J., 2009. Basinwide modification of dynamical and biogeochemical processes by the positive phase of the Indian Ocean Dipole during the SeaWiFS era, in: Wiggert, J.D., Hood, R.R., Wajih, S., Naqvi, A., Brink, K.H., Smith, S.L. (Eds.), Indian Ocean Biogeochemical Processes and Ecological Variability, Geophysical Monograph Series. p. 350.