1. Does the paper address relevant scientific questions within the scope of BG?

This paper addresses the scientific questions related to the interannaul variability of some of the components and processes in the study of Biogeochemical Cycles in the Indian Ocean. This is within the scope of BG.

2. Does the paper present novel concepts, ideas, tools, or data?

This paper addresses the biological consequences of IOD and ENSO events in the Indian Ocean using a biophysical model. Earlier studies on this topic have not been carried out in detail to understand the relative impacts of IOD and ENSO on Chlorophyll.

Statistical tools have been used to isolate the effects of IOD and ENSO on Chlorophyll in different regions of the Indian Ocean.

3. Are substantial conclusions reached?

Satellite data is available only for a short-period to differentiate the effect of IOD and ENSO events. Results of the model simulations for 41 years have been used to understand and differentiate the effect of IOD and ENSO events on the physical (thermocline depth, wind stress) and biological processes (chlorophyll concentration), using statistical methods. Substantial conclusions have been reached on the relative impact of IOD and ENSO events on Chlorophyll (surface and integrated over euphotic zone) in six different regions during a few seasons.

Though some conclusions have been reached in different regions of the Indian Ocean during a few seasons, more understanding of the response of physical, biological and chemical processes to IOD and ENSO events are required at the surface and subsurface levels in the ocean.

We have attempted to address the reviewer's concerns in this regard; the proposed changes to the manuscript are detailed below.

4. Are the scientific methods and assumptions valid and clearly outlined?

Details of the model, forcing data and observational data sets are given in Section 2. Statistical tools and the results based on the statistical analysis are discussed. What these statistical tools do and what is the exact meaning of the results can be described in more detail to understand the interpretation of the results. Eqns. 1-4 need to be explained properly for the reader’s benefit.

Similar concerns were raised by both reviewers over the description of statistical tools in Sect. 2. We propose to clarify and make this section more easily understood in the following way:

1) Adding a figure that displays the time series of IOD, ENSO, IOD with ENSO signal removed, *surface chlorophyll in the EEIO box and surface chlorophyll in the EEIO box with ENSO signal removed. This figure will illustrate the principle of the partial regression method, as well as the temporal evolution of IOD and ENSO indices.*

2) Adding a non-technical sentence, which provides a simple explanation of what the statistical methods do, at the beginning of each paragraph that deals with the partial regression methods in Sect. 2.

3) Changing the notation related to the partial regression as detailed in the response to reviewer #2.

There are a few assumptions that are contentious:

a) As the model is an ocean only model, we wonder if a detailed statistical analysis of SST is warranted as it is determined mostly by atmospheric forcing.

We agree that the model SST is relatively strongly constrained by the forcing datasets. However SST is not restored to observational records of SST. The modelled SST results from a complex interaction between a number of processes, including insolation, air-sea energy transfer and water-column mixing due to wind forcing. As such, the modelled SST is a non-trivial result. The SST analyses and figures are not provided as model validation, but rather to describe the typical SST patterns associated with the IOD and ENSO, which may not be familiar to some readers. We will point this out more clearly in the text.

b) A more problematic assertion has been made on page 5854, second para. ("Attributiong causes to model-data differences .....study). The paper focuses exclusively on IOD and Nino3 and completely ignores the influence of ecosystem dynamics on the evolution of the chlorophyll field. As the study quickly moves to using model results as a proxy for reality, not using all the terms in the evolution of biogeochemical fields when they are available, is a serious problem. While we agree that it would be impossible to analyse all the biogeochemical compartments, we fear that the conclusions reached only on the basis of physics could be potentially very misleading.

The aim of this paper was to investigate the interannual changes in chlorophyll caused by IOD and ENSO-induced changes to the physical ocean. We will adjust the introduction (Sect. 1) to state this more clearly. Certainly, factors beyond the climate modes may dominate interannual variations in some parts of the Indian Ocean, which could include ecosystem dynamics. We agree that it is important to acknowledge the potential role of ecosystem effects and will address the gap identified by the reviewer in the following way: In the section identified by the reviewer, we will add a sentence acknowledging that upper trophic levels are not included in the model and that lacking *trophic cascades and ecosystem dynamics could account for some of the model-SeaWiFS differences. As our investigations show, a large proportion of interannual chlorophyll variability can be linked to climate modes (Table 2), suggesting that in many areas, the influence of ecosystem interactions on interannual anomalies may be of second order. However, there are equally regions where climate modes do not explain a large proportion of the interannual chlorophyll fluctuations (e.g. western Arabian Sea). In reference to these regions, we will mention the potential role of the ecosystem dynamics in controlling the interannual chlorophyll fluctuations. Lastly, we will add a dedicated paragraph in the discussion (Sect. 5) to expand on the influence of biogeochemical and ecosystem processes (such as grazing or nutrient recycling) that could affect interannual chlorophyll variability.*

I am not sure if IOD independent of ENSO ? Could the authors elaborate?

There is ample evidence in the literature that IOD events are partially independent from ENSO events (Annamalai et al., 2003; Yamagata et al., 2004; Fischer et al., 2005; Behera et al., 2006; Luo et al., 2008, 2010; Izumo et al., 2013). As stated in the introduction (page 5845, line 8-9), there is a tendency for IOD events to co-occur with ENSO events, but they can also occur independently. We will expand this introductory paragraph to make it clearer. As a result of the frequent ENSO/IOD co-occurrence, the IOD and ENSO indices are statistically correlated (Yamagata et al., 2004; Izumo et al., 2013), and hence we use the partial regression method to separate the signals associated with IOD only or ENSO only (as mentioned on page 5851, lines 25-27).

5. Are the results sufficient to support the interpretations and conclusions?

Results discussed may be sufficient for the interpretations and conclusions arrived in this paper.

Some more studies may be required on the subsurface properties and also on the other biogeochemical components and processes which influence the chlorophyll in the Ocean. For example, regenerated production, multinutrient limitation, grazing etc.

Please see comment 4b.

The analysis of thermocline depth variations already allows discussing changes in oceanic physical subsurface properties. As mentioned in the manuscript (pg. 5849, lines 8-14), the biogeochemical model used in this study simulates regenerated production, the limitation of five different nutrients (NO3, NH4, PO4, SiO4, and Fe), and zooplankton grazing on phytoplankton (though not the impact of higher trophic levels). Analyses of the impact of these processes on chlorophyll would indeed be interesting, but in our opinion beyond the scope of this manuscript. As stated above, we will add a paragraph on the potential role of biogeochemical and ecosystem dynamics in *controlling interannual chlorophyll fluctuations in the revised manuscript discussion.*

Responded to comment 4b above.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

Description of methodology, especially statistical methods and the meaning of results obtained after statistical analysis are not sufficient to be used by fellow scientists.

As detailed in the specific comments section below, we propose to revise the description and discussion of the statistical methods to better illustrate their application and interpretation.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Authors indicate their new contribution in the Abstract.

8. Does the title clearly reflect the contents of the paper?

Title reflects the contents of the paper.

9. Does the abstract provide a concise and complete summary?

Abstract provides the summary of the paper.

10. Is the overall presentation well structured and clear?

Overall presentation is well structured

11. Is the language fluent and precise?

Language is fluent

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Some of the formulae, symbols and abbreviations are not explained in detail.

We will clarify the symbols and terminology in greater detail in the revised manuscript, as part of the improvement to the descriptions of statistical methods (detailed below).

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

Parts of the paper providing the description of the model and evaluation of the model from the earlier papers are not very clear. Discussion of results in some of the figures can be more precise.

We will revise the model description and evaluation discussion (Section 2.2), in order to clarify and make easier reading of this section as best we can.

14. Are the number and quality of references appropriate?

Looks reasonable

15. Is the amount and quality of supplementary material appropriate?

I could not access the Supplementary material

There is no supplementary material for this manuscript.

General Comments

This paper addresses the scientific questions related to the interannaul variability of some of the components and processes of Biogeochemical Cycles in the Indian Ocean. The main focus is to understand the biological consequences of IOD and ENSO events in the Indian Ocean using a biophysical model. Study of chlorophyll anomalies driven by these two climate modes is essential since they are responsible for the significant interannual variabilities in different regions of the Indian ocean. Results of the model simulations for 41 years have been used to understand and differentiate the effect of IOD and ENSO events on the physical (thermocline depth, wind stress) and biological processes (chlorophyll concentration), using statistical methods. Earlier studies have not been carried out in this detail to understand the relative impacts of IOD and ENSO on Chlorophyll in different regions of the Indian Ocean. Statistical tools have been used to isolate the effects of IOD and ENSO on Chlorophyll. Description of the model, forcing data and observational data sets are given in the paper. Statistical tools and the results based on the statistical analysis are discussed. Most of the figures in the paper are based on the statistical analysis. However, what these statistical tools do when applied to the model results and observational data sets, and what is the exact meaning of the results are not described in detail to understand the concepts. Conclusions have been reached on the relative impact of IOD and ENSO events on Chlorophyll (surface and integrated over euphotic zone) in six different regions during a few seasons based on physical processes.

Results discussed may be sufficient for the interpretations and conclusions arrived in this paper. Some more studies may be required on the subsurface properties and also on the other biogeochemical components and processes which influence the chlorophyll in the Ocean.

This kind of model study helps in improving the understanding of the interannual variability of productivity and carbon fluxes in the Indian Ocean.

The parts of the General Comments that require responses are repeated from the previous section and have been responded to there (point 5 and 6).

Specific Comments

Abstract:

P.5843, Line 13: A previously unreported --- I believe Wiggert (2009) has already made this observation??

We will correct this erroneous statement, thank you for pointing it out.

p.5843, line 18: ENSO and IOD cause significant and predictable --- Productivity is very specific term that refers to the rate of primary production and we take offence when it is loosely used to describe standing crop via chlorophyll abundance

We will re-word and use "chlorophyll concentration" throughout the manuscript.

1. Introduction

p.5844, lines 15-16: Yoder and Kennelly (2003) – Interannual modes of variability ascribed to ENSO Control, not IOD ??? – Please check the correctness of this statement.

We have re-checked the study by Yoder and Kennelly (2003) and are confident of our statement.

p.5845, line 3: Subsurface temperature anomalies ---:

➔ ENSO and IOD influence both surface and subsurface properties - Is it true?

There is convincing evidence that both IOD and ENSO induce Ekman pumping south of the equator in the Central Indian Ocean. This pumping forces Rossby waves that induce thermocline variations and hence subsurface temperature anomalies in the Seychelles-Chagos thermocline ridge region (Masumoto and Meyers, 1998; Tozuka et al., 2010). IOD events seem to drive sea level variations north of 10°S, while those south of 10°S are mainly forced by ENSO-related wind forcing (Rao and Behera, 2005; Yu et al., 2005). To avoid confusion, we will refer to "thermocline depth" in place of "subsurface anomalies" in the revised manuscript.

2. Data and Methods

Section 2.2

p.5848, line 24: some details on the extensive validation of OGCM required

Greater detail on the model validation will be provided by adjusting the paragraph on pg 5848 (lines 24-28) and 5849 (lines 1-2) as follows: "The OGCM has been used extensively in an uncoupled mode (Lengaigne et al., 2002; Cravatte et al., 2007) and coupled with an atmospheric model (Lengaigne et al., 2006; Lengaigne and Vecchi, 2010), using various forcing strategies. It has been shown to accurately simulate the vertical structure of equatorial temperature and currents (Vialard et al., 2001), as well as the interannual variations of heat content in the Pacific Ocean (Lengaigne et al., 2012). The model also compares favourably with sea level from satellite altimetry and tide gauges at interannual timescales in the tropical Indian and Pacific Oceans (Nidheesh et al., 2012). Keerthi et al. (2013) demonstrate that the model reproduces interannual variability of the mixed layer depth relatively well in the Indian Ocean, with similar spatial patterns and reasonable phase agreement compared to estimates from in-situ data."

p.5850, line 9: Seven-year spin-up – Is it enough?

A seven year spin-up is sufficient because the initial conditions used at the beginning of the seven-year spin-up are taken as the last time step of the simulation described by Aumont and Bopp (2006). The model can therefore be considered to be equilibrated before performing the seven-year spin-up. The following sentence will be added in the revised manuscript to clarify this point (pg 5850, lines 9-10): "The physical-biogeochemical coupled model was initialized from outputs of the simulation described by Aumont and Bopp (2006). The model was then run for a seven-year spin-up period before performing the simulation over the period of 1958 to 2001, providing outputs with a temporal resolution of five days."

Section 2.3 Methodology

Most of the figures are containing the results of statistical analysis and conclusions of the paper are based on the interpretation of results of statistical tools applied to data and model outputs. Therefore, more details on the statistical tools are required to understand the concepts.

While we feel that we provide an accurate technical description of the methods and tools employed in the present study, we agree that this description may be too technical for researchers not accustomed with these techniques. We have detailed our proposed improvements to this section under point 4 above.

p.5852: A better description of the equations required

We have detailed our proposed improvements to this section under point 4 above, which address this comment. Suggested changes to the notation of equations are detailed in the response to reviewer #2.

Section 3

p.5853, line 16: Kone et al (2009) --- emergent biogeographic provinces

Not clear; need a more specific mention of what exactly was compared; It would also be helpful to the reader who is more biologically-inclined (rather than physically) to get a birds- eye view of the performance of the model's biogeochemical aspects

We will reword this sentence and add more detail about the comparisons performed by Koné et al. (2009) as follows (pg 5853, lines 16-18): "A more detailed comparison of the seasonal evolution of the modelled and observed surface chlorophyll in the Indian Ocean are provided in Koné et al. (2009). Using the same methods as Lévy et al. (2007), their study demonstrates that the model identified similar biogeographic provinces to SeaWiFS data, specifically in most of the Arabian Sea, Bay of Bengal and in the convergenze zone regions south of the equator. These biogeochemical provinces were based on the cumulated increase in chlorophyll of summer or winter phytoplankton blooms, as well as the timing of these bloom onsets."

p.5853, 2nd Paragraph: Anomalies are calculated for different climatology periods for model and data. Need a short explanation why this is so and how it would influence the results

A similar point was raised by reviewer #2. 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. This short period was not adequate to estimate a robust seasonal cycle in our opinion. As such, using a different 10-year climatology period (but with maximum overlap) was the best we could do with available model outputs. We will clarify this in the 'Data and methods' (Sect. 2) of the reviewed manuscript by adding the following: "The model simulation and SeaWiFS overlap during a period of only four years (and four months), however we preferred to use a longer 10-year period to provide a robust estimate of the seasonal cycle. The climatology periods were chosen so as to maximise the overlap between the model and observations, i.e. 1998-2009 for SeaWiFS and 1990-2001 for the model. Using the common 1998-2001 period to estimate the climatology provided very similar results (Fig. A1 below)."

p.5854, lines3-5: Model – data differences - See comment 4b:

See response to comment 4b above.

Discrepancies in Fig.2 is both in Coastal and Open ocean regions.

Some open ocean regions do show discrepancies with observations, especially in the central Arabian Sea during fall and winter. These biases are of relatively small magnitude, however, *compared to those near the coastline. In order to address the reviewer's comment, we suggest to add to the following sentences as follows (pg 5853, lines 11-16): "During the summer/southwest monsoon and fall intermonsoon (Fig. 1e-h), the model correctly simulates phytoplankton blooms along the coasts of Somalia and the Arabian Peninsula, at the southern tip of India, around Sri Lanka, along the Seychelles Chagos thermocline ridge between 5° S and 15° S and in the southeastern Indian Ocean. The amplitude of these blooms are frequently overestimated in oceanic regions compared to SeaWiFS, whereas the chlorophyll values are notably underestimated in the central Arabian Sea, resulting in an exaggerated gradient from the western continental margin to the interior of the basin (Fig. 1e-h).*

Section 4:

4.1: Physical Response

Discussion on relationship between SST variations and Thermocline depth → Some more physical processes may have to be studied.

The basic mechanism relating the thermocline depth anomalies and the SST variations is provided in the first paragraph of Sect. 4.1 (pg 5854, lines 20-24). Similarly, the end of the second paragraph provides a discussion of the physical relationship between D20 and SST during IOD events (pg 5855, lines 26-29; pg 5856, lines 1-7) while the fourth paragraph discusses the same for ENSO (pg 5856, lines 27-29; pg 5857, lines 1-8). It is not clear to us what additional investigation of the SST/D20 relationship the reviewer is requesting here.

p.5856 and p. 5857: Interpretation of ENSO and IOD impacts on Surface and subsurface variabilities, results in Figures 4 and 5 are not clear.

We will undertake to improve Figures 4 and 5 to make them as simple and clear as possible: We will improve the sizes of the wind vectors and simplify the figure captions. The figures do contain a lot of information, however we feel that the overlaid information is useful for interpretation and would prefer to keep the wind vectors overlaid on the thermocline anomaly plots, rather than separate them into additional panels.

4.2 Biological response

p.5857, lines 17-24; p.5858, lines 14-18: Similar behaviour is not observed in Schl

We agree that quite different patterns are seen in IChl and SChl relationships with D20, which we have already explained in the text. In response to the reviewer's comment, we could emphasize this more, by adding the following sentence at the end of the paragraph (pg 5858, line 17): "The *regions that display a significant IChl-D20 relationship, but no SChl-D20 relationship, are areas where changes in a relatively deep thermocline and a deep chlorophyll maximum have minimal bearing on the overlying SChl."*

p.5857, line 28: lack of significant relationship – Is it so?

While there is a widespread relationship between IChl and D20 over most of the tropical Indian Ocean, this is not true for the region offshore of the horn of Africa. Interannual chlorophyll variability is likely dominated by offshore advection of nutrient-rich water and eddy variability in this region. We assume the reviewer is pointing out that the area referred to is centred in the western half, rather than the central part of the Arabian Sea. We will therefore rephrase this sentence as follows (pg 5857, lines25-28): "Similarly, the central Arabian Sea is dominated by Ekman convergence in boreal summer (Schott et al., 2009), which together with strong coastal upwelling and offshore advection of nutrient-rich waters from the Somali and Oman coasts, might explain the lack of a significant relationship in the western half of the Sea."

p.5859, line 9: Difference between Figures 7e and f – why?

The most striking difference between these panels is the horseshoe-shaped area of negative anomalies in the western Indian Ocean seen for IChl (Fig. 7e), which is absent in SChl (Fig. 7f). Comparing Figs. 6c and 6d reveals an explanation for this difference. Unlike IChl, SChl is not related to D20 in those western (off-equatorial) Indian Ocean regions. Due to the lack of relationship between D20 and SChl in these areas, positive D20 anomalies related with IOD events in the western Indian Ocean (Fig. 4c, e) do not translate into changes for SChl, as they do for IChl. To clarify this point, we propose to add the following sentence in the revised manuscript (pg 5859, lines 9):"This difference is related to the lack of relationship between D20 and SChl (Fig. 6d) as opposed to IChl (Fig. 6c) in this region. The climatological thermocline and subsurface chlorophyll maximum are relatively deep is these regions, so that variations in D20 and subsurface chlorophyll content do not seem to reach surface chlorophyll."

p.5859, lines 15-23: Are these results consistent with earlier studies?

This selection of text refers to our results from the western Arabian Sea and the novel finding that the chlorophyll (and physical) anomalies in this region seem to be more closely related to ENSO than to IOD. The only two studies that we are aware of, which focus on the impact of climate modes on interannual chlorophyll anomalies in the Arabian Sea, are Sarma (2006) and Wiggert et al. (2009). We discuss both of their results in comparison to our findings in this region in the Summary and Discussion section (pg 5865, lines 11-28). In the same paragraph we also cite studies that have shown a link between ENSO and wind anomalies in this region, providing support to our *conclusions that ENSO affects interannual variability in this region.*

Figures 9 (a-j), Tables 1 & 2 – Relative impacts of ENSO and IOD on Chlorophyll in six different regions are discussed.

IOD and ENSO do not show similar anomalies for SST, D20, IChl, SChl

We agree, a statement to this effect will be inserted at the beginning of Section 4.3.

Impact of SST and D20 on IChl and Schl – not very clear in different regions

SST is not expected to be a significant direct driver of chlorophyll. If the reviewer and editor require it, we could remove SST from Fig. 9 and restructure the figure. However we prefer to retain SST in the figure, as together with D20, it provides a useful diagnostic to interpret the physical effects of the climate modes. Some regions exhibit a clear relationship between D20 and chlorophyll, while other regions do not (Figs. 6c and d). The regions investigated in Fig. 9 were not customised to demonstrate the most convincing or clearest impact between D20 and chlorophyll, but rather to capture areas characterised by a high interannual variability of chlorophyll. In the context of Fig. 9 and the tables, a negative result (i.e. lack of impact/relationship) is frequently still an interesting result and worth reporting, as it points to alternate forcing playing a considerable role in interannual anomalies.

Some results on IChl and SChl may be related to Subsurface properties of biological components

We agree that interannual chlorophyll variability might be impacted by grazing pressures and nutrient recycling by biological components. This is especially pertinent for regions where the climate modes seem to play a relatively small role in controlling chlorophyll variability (e.g. the western Arabian Sea; Table 2). As explained in our response to point 4b above, we will emphasize the potential role of ecosystem dynamics in controlling chlorophyll fluctuations when discussing the regions where climate modes have seemingly little control over chlorophyll. This comment will equally be addressed in the dedicated paragraph that we plan to add to the discussion (Sect. 5), which will expand on the potential influence of biogeochemical and ecosystem processes that could affect interannual chlorophyll variability.

Tables 1 & 2 : Partial regression coefficients are given for different seasons for Ichl and Schl

Based on Figure 9, it is clear that the peak season for IChl and SChl variations in response to the climate modes can be different in some regions (e.g. TRIO and TIO regions). We therefore decided to show these regression coefficients for the season where a multiple regression including both ENSO and IOD explained the greatest proportion of IChl or SChl variance. A clearer justification *of the season's selection in Table 1 and 2 will be provided in the corresponding captions in the revised manuscript.*

Table 1: Coefficients corresponding to peak impacts of ENSO and IOD seasons are not provided (sometimes).

We assume the reviewer is referring to the fact that the seasons shown in Table 1 (and 2) are kept the same for each of ENSO and IOD (within either of IChl or SChl), rather than reporting the coefficients of the peak season for each. The latter option was considered, however the added complexity that this would introduce to the table deterred us from doing so. If the reviewer and editor require it, we can change the tables to allow different seasons for the IOD and ENSO coefficients; it will not alter the conclusions of the paper.

To describe the results on impacts of ENSO and IOD on Chlrophyll, many physical processes in the Ocean and Atmosphere are discussed. Sometimes these discussions look confusing.

We acknowledge that some of the detail and discussions on the physical ocean response to the atmospheric forcing are not vital to the goals of this paper. As such, we will review and shorten the discussions related to the physical processes in the revised manuscript.

Summary and Discussion

p.5864, lines20-24: IOD forced decrease in IChl around the southern tip of India during October-December – What is the reason?

The STI box shows negative chlorophyll anomalies during ~July-December, but not significant D20 anomalies in response to IOD forcing (Figs. 9g and h). Yet the northern lobe of the off-equatorial Rossby Wave response caused by IOD, does result in significant positive D20 anomalies around the southern tip of India and Sri Lanka (Fig. 4b and d). The STI box likely does not isolate this positive D20 anomaly signal effectively (and likely captures some of the negative anomaly signal equatorwards of those positive anomalies), hence it is not significant in Fig. 9g. The negative chlorophyll anomalies in this region during October-December are likely due to, at least partially, the deeper-than-normal thermoclines caused by IOD. We do provide a tentative explanation in Sect. 4.2 of why there is not stronger coupling between D20 and chlorophyll in this region (pg 5862, lines 6-10): "The lack of obvious coupling between thermocline depths and surface temperature or chlorophyll around the southern tip of India is likely due to intense horizontal circulation between the Bay of Bengal and the Arabian Sea (e.g. Vinayachandran et al., 1999), as well as the added complexity of seasonal barrier layers in these regions (Sprintall and Tomczak, 1992)."

p.5864, lines 29-30; p.5865, lines 1-3: Chl and PP depend on several nutrients, not only Nitrate, Regenerated production is usually higher than the new production in the north Indian Ocean – some clarifications

We do not mention nitrate in this passage of text, nor do we imply that it is solely responsible for documented IChl changes. We assume perhaps the reviewer may have confused 'nutricline', which we do mention, with 'nitracline'. We agree that regenerated production may dominate new production in many regions of the Indian Ocean. Our paper focuses, however, on the anomalies to the normal/background production. These anomalies are (to a substantial degree) brought about by changes in the dynamics and thermodynamics in the surface waters of the ocean. We argue that a large proportion of the IChl anomaly signals that we see, are caused by changes in the thermocline depth that bring nutrients closer or further from the sunbathed surface. In the passage referred to by the reviewer, we provide multiple references of papers that provide similar conclusions. Another such reference, which we do not include there is Cermeño et al. (2008).

Novel contribution in this study is the separation of impacts of ENSO and IOD in six regions in the Indian Ocean

We agree and undertake to emphasize this point in the Discussion if we have not done so adequately.

References

Annamalai, H., Murtugudde, R., Potemra, J., Xie, S.P., Liu, P., Wang, B., 2003. Coupled dynamics over the Indian Ocean: spring initiation of the Zonal Mode. Deep Sea Research Part II: Topical Studies in Oceanography 50, 2305–2330.

Aumont, O., Bopp, L., 2006. Globalizing results from ocean in situ iron fertilization studies. Global Biogeochemical Cycles 20, GB2017.

Behera, S.K., Luo, J.J., Masson, S., Rao, S.A., Sakuma, H., Yamagata, T., 2006. A CGCM Study on the Interaction between IOD and ENSO. Journal of Climate 19, 1688–1705.

Cermeño, P., Dutkiewicz, S., Harris, R.P., Follows, M., Schofield, O., Falkowski, P.G., 2008. The role of nutricline depth in regulating the ocean carbon cycle. Proceedings of the National Academy of Sciences 105, 20344–20349.

Cravatte, S., Madec, G., Izumo, T., Menkes, C., Bozec, A., 2007. Progress in the 3-D circulation of the eastern equatorial Pacific in a climate ocean model. Ocean Modelling 17, 28–48.

Fischer, A.S., Terray, P., Guilyardi, E., Gualdi, S., Delecluse, P., 2005. Two Independent Triggers for the Indian Ocean Dipole/Zonal Mode in a Coupled GCM. Journal of Climate 18, 3428–3449.

Izumo, T., Lengaigne, M., Vialard, J., Luo, J.-J., Yamagata, T., Madec, G., 2013. Influence of Indian Ocean Dipole and Pacific recharge on following year's El Niño: interdecadal robustness. Climate Dynamics early online release.

Keerthi, M.G., Lengaigne, M., Vialard, J., De Boyer Montégut, C., Muraleedharan, P.M., 2013. Interannual variability of the Tropical Indian Ocean mixed layer depth. Climate Dynamics 40, 743–759.

Koné, V., Aumont, O., Levy, C., Resplandy, L., 2009. Physical and biogeochemical controls of the *phytoplankton seasonal cycle in the Indian Ocean: A modeling study, 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.*

Lengaigne, M., Boulanger, J.-P., Menkes, C., Masson, S., Madec, G., Delecluse, P., 2002. Ocean response to the March 1997 Westerly Wind Event. Journal of Geophysical Research-Oceans 107, 8015.

Lengaigne, M., Boulanger, J.-P., Menkes, C., Spencer, H., 2006. Influence of the seasonal cycle on the termination of El Niño events in a coupled general circulation model. Journal of Climate 19, 1850–1868.

Lengaigne, M., Hausmann, U., Madec, G., Menkes, C., Vialard, J., Molines, J., 2012. Mechanisms controlling warm water volume interannual variations in the equatorial Pacific: diabatic versus adiabatic processes. Climate Dynamics 38, 1031–1046.

Lengaigne, M., Vecchi, G., 2010. Contrasting the termination of moderate and extreme El Niño events in coupled general circulation models. Climate Dynamics 35, 299–313.

Lévy, M., Shankar, D., André, J.-M., Shenoi, S.S.C., Durand, F., De Boyer Montégut, C., 2007. Basin-wide seasonal evolution of the Indian Ocean's phytoplankton blooms. Journal of Geophysical Research-Oceans 112, 12014.

Luo, J.-J., Behera, S., Masumoto, Y., Sakuma, H., Yamagata, T., 2008. Successful prediction of the consecutive IOD in 2006 and 2007. Geophysical Research Letters 35, L14S02.

Luo, J.-J., Zhang, R., Behera, S.K., Masumoto, Y., Jin, F.-F., Lukas, R., Yamagata, T., 2010. Interaction between El Niño and Extreme Indian Ocean Dipole. Journal of Climate 23, 726–742.

Masumoto, Y., Meyers, G., 1998. Forced Rossby waves in the southern tropical Indian Ocean. Journal of Geophysical Research-Oceans 103, 27589–27602.

Nidheesh, A.G., Lengaigne, M., Vialard, J., Unnikrishnan, A.S., Dayan, H., 2012. Decadal and long-term sea level variability in the tropical Indo-Pacific Ocean. Climate Dynamics In press.

Rao, S.A., Behera, S.K., 2005. Subsurface influence on SST in the tropical Indian Ocean: structure and interannual variability. Dynamics of Atmospheres and Oceans 39, 103–135.

Sarma, V.V.S.S., 2006. The influence of Indian Ocean Dipole (IOD) on biogeochemistry of carbon in the Arabian Sea during 1997—1998. Journal of Earth System Science 115, 433–450.

Schott, F.A., Xie, S.-P., McCreary, J.P., 2009. Indian Ocean circulation and climate variability. Reviews of Geophysics 47, RG1002.

Sprintall, J., Tomczak, M., 1992. Evidence of the barrier layer in the surface layer of the tropics. Journal of Geophysical Research-Oceans 97, 7305–7316.

Tozuka, T., Yokoi, T., Yamagata, T., 2010. A modeling study of interannual variations of the Seychelles Dome. Journal of Geophysical Research-Oceans 115, 04005.

Vialard, J., Menkes, C., Boulanger, J.-P., Delecluse, P., Guilyardi, E., McPhaden, M.J., Madec, G., 2001. A model study of oceanic mechanisms affecting equatorial Pacific sea surface temperature during the 1997–98 El Niño. Journal of Physical Oceanography 31, 1649–1675.

Vinayachandran, P.N., Masumoto, Y., Mikawa, T., Yamagata, T., 1999. Intrusion of the Southwest Monsoon Current into the Bay of Bengal. Journal of Geophysical Research-Oceans 104, 11077–11,085.

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.

Yamagata, T., Behera, S.K., Luo, J.J., Masson, S., Jury, M.R., Rao, S.A., 2004. Coupled ocean-atmosphere variability in the tropical Indian Ocean. Geophysical Monograph Series 147, *189—211.*

Yoder, J.A., Kennelly, M.A., 2003. Seasonal and ENSO variability in global ocean phytoplankton chlorophyll derived from 4 years of SeaWiFS measurements. Global Biogeochemical Cycles 17, 1112.

Yu, W., Xiang, B., Liu, L., Liu, N., 2005. Understanding the origins of interannual thermocline variations in the tropical Indian Ocean. Geophysical Research Letters 32, 24706.

Appendix A

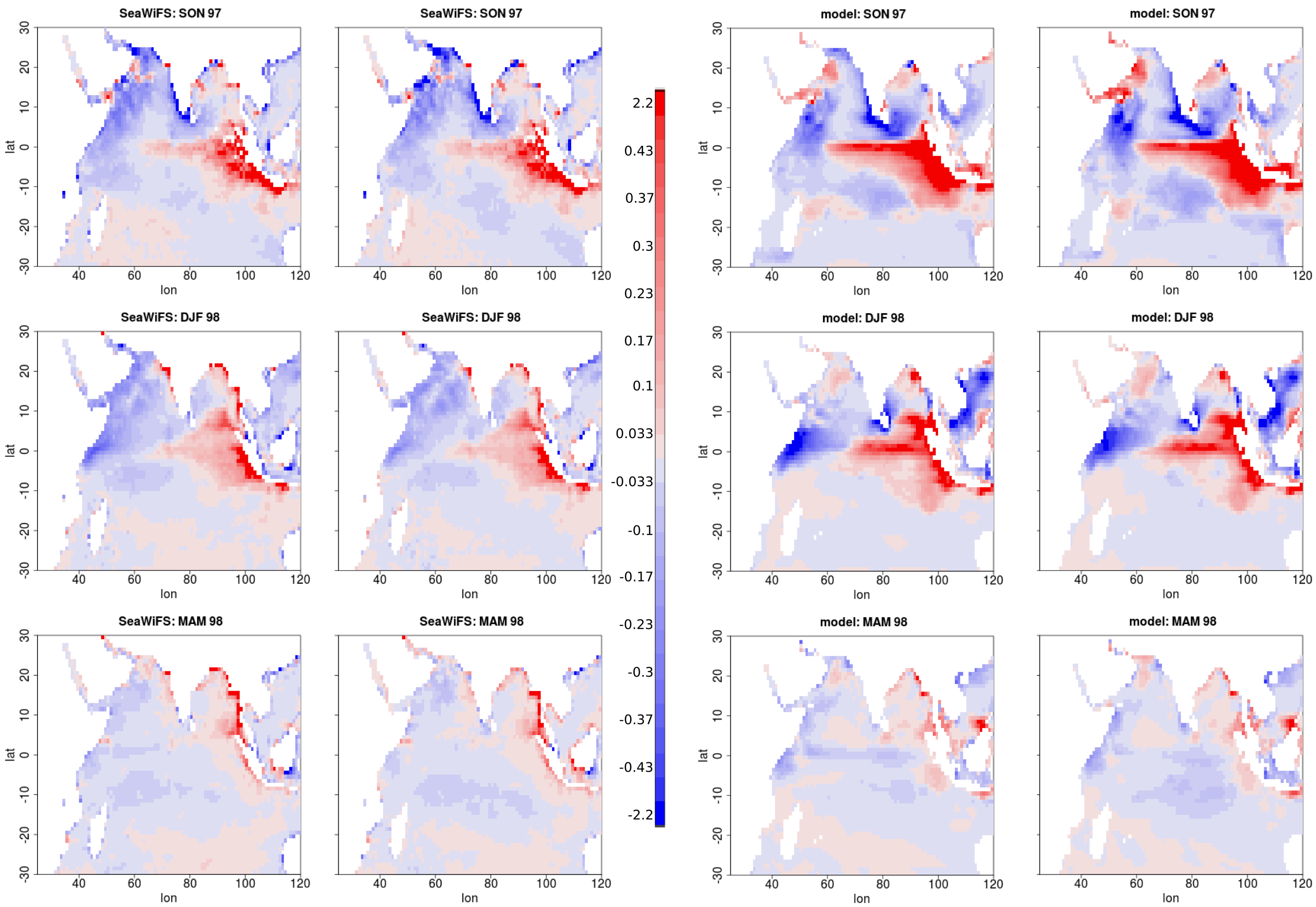

Fig. A1. *De-seasoned chlorophyll anomalies from the 1997/1998 event, comparing side-by-side the 10-year climatology periods used in the manuscript (1st and 3rd column) and the overlapping (identical) 4-year climatology period (2nd and 4th columns). The surface chlorophyll anomalies were calculated for SeaWiFS data (on the left) and from model outputs (on the right).*