Biogeosciences Discuss., 10, C3113–C3118, 2013
www.biogeosciences-discuss.net/10/C3113/2013/

Biogeosciences Discussions
Open Access

Interactive comment on "Indian Ocean Dipole and El Niño/Southern Oscillation impacts on regional chlorophyll anomalies in the Indian Ocean" *by* J. C. Currie et al.

Anonymous Referee #2

Received and published: 29 June 2013

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 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.

Specific comments

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

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].

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.

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.

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):

BGD

10, C3113–C3118, 2013

Interactive Comment

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., $\sim$ -2 to 2)?

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.

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.

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

And so on.

To followup 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

BGD

10, C3113–C3118, 2013

in the literature for subsequent analyses to refer to.

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

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?

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.

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.

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

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.

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

BGD

10, C3113–C3118, 2013

Interactive Comment

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).

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).

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?

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

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

Technical comments

None.

Suggested References

Behrenfeld, M. J., T. K. Westberry, E. S. Boss, R. T. O'Malley, D. A. Siegel, J. D. Wiggert, B. A. Franz, C. R. McClain, G. C. Feldman, S. C. Doney, J. K. Moore, G. Dall'Olmo, A. J. Milligan, I. Lima, and N. Mahowald (2009), Satellite-detected fluorescence reveals global physiology of ocean phytoplankton, Biogeosci., 6, 779-794.

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

BGD

10, C3113–C3118, 2013

Interactive comment on Biogeosciences Discuss., 10, 5841, 2013.

BGD

10, C3113–C3118, 2013

Interactive
Comment