Biogeosciences Discuss., 7, C1533–C1535, 2010
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Biogeosciences Discussions

Interactive comment on "Large-scale shifts in phytoplankton groups in the Equatorial Pacific during ENSO cycles" *by* I. Masotti et al.

Anonymous Referee #1

Received and published: 24 June 2010

General comments

The contribution by Masotti et al. touches on an important topic in marine sciences: understanding the relation between climate-driven changes in ocean dynamics and phytoplankton community structure. However, before the paper is published in Biogeosciences the authors first need to carry out a more detailed review of the rich history of equatorial Pacific phytoplankton composition and abundance (papers by Bidigare, Chavez, Chisolm group, Landry – JGOFS data available on line) and the results from the PHYSAT model placed in this context. For example, Prochlorococcus is the dominant group along the equator in these studies yet that does not seem to be the case for PHYSAT estimates. Synechococcus appears to be overestimated by PHYSAT as well. There is significant smaller scale spatial variability in abundances from the equator

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proper to a few degrees north and south that is not considered. Is this not picked up by the satellite algorithm or is the resolution of the analysis such that this effect is missed? In general there is a mismatch between the presentation of global results and the regional focus of the paper. The use of the coupled numerical model seems superfluous in the present version. To keep the coupled numerical model as part of this paper the authors should consider model results beyond estimates of nitrate concentration which surely can be predicted at least as accurately from satellite temperature. The reader is left wondering why the phytoplankton predictions from the model are not shown. The paper cries for a more detailed analysis of the processes responsible for the proposed changes in nutrients, abundance and composition. What are the temporal and spatial physical, chemical and biological dynamics in the model and the satellite time series?

Specific comments:

Several of the figures (3, 5, 6, 7) are very difficult to read given their size and the labels

The authors mix in results from other geographical regions when the focus is on the equatorial Pacific. Stick to the topic.

SOI values / identification of strong/moderate El Niños (p. 2528 l. 4-8 ie. data&methods) should be in the results when first using the SOI; the OCTS/SeaWiFS comparison (p. 2529 l.22-27 results) should be in data and methods when introducing the satellites.

Fig. 3 is referred to as climatology when it really isn't – OCTS is based on 1996-97 and SeaWiFS on 1999 ie El Niño vs. La Niña - which the authors acknowledge, but still use it as "basic comparison" between the two satellites. If it's just to say that OCTS has more missing data, a simple number (average percentage of missing values per month for each) should be enough. If they do want to compare, then use all SeaWiFS years rather than just the 1999 La Niña. Same should be done for fig. 4. Authors should complete Fig. 3 discussion before moving on to Fig. 4 (refer to Fig. 3 very briefly p.2529, then Fig. 4, and come back to Fig. 3 p. 2531).

BGD

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Why is there no impact on phyto composition in 2002-03 when nitrate concentration decreases rather significantly?

The arguments about Syn linked with NO3 do not seem valid – they show that Syn concentration/numbers increase with NO3 but NOT that their dominance increases – ie. that they increase more than others. The argument seems based on half-saturation constants (p. 2536 l.12) but this reviewer could not find the values in the Veldhuis 2005 paper.

Why not look at N:P (p. 2537) with PISCES?

Technical comments:

- p. 2525 l.20-25: there are 2 different considerations that are treated together: phytoplankton have different C-fixation efficiency as we as different nutrient requirement. Not clear, re-write.

- p. 2534 l.14: a space missing (NO3is)

- fig. 4: Synetochococcus or I don't know what in the caption...

- fig. 5: I'd change the scales, especially for Pros/Dia. Maybe add the SOI somewhere in Fig. 5e/g.

Interactive comment on Biogeosciences Discuss., 7, 2523, 2010.