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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 #2

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General comments:

The paper by Masotti et al has the potential to contribute valuable information on the influence of ENSO on marine ecosystem variability. Especially, there are many unknowns related to the 1997-1998 ENSO, although given for a short period, analyses of the 8 months of OCTS data combined with the SeaWiFS gives valuable information on phytoplankton composition response to the ecosystem evolution under ENSO conditions. However, because the manuscript is not very well written and the results are not well discussed within the context of previous research done, the main results remain unclear and thus unconvincing. Paper focuses on NO3 control of phytoplankton groups but neglects iron, while there is consensus that iron is the main limiting nutrient in the Cold Tongue region. Why iron results of PISCES are not shown, is it simply because

they are correlated with NO3 (or are they decoupled)?

There is an extensive list of publications that focus on Equatorial Pacific under different states of ENSO (Murray et al., 1995; Radenac and Rodier, 1996; Ishizaka et al., 1997; Mackey et al., 1997; Dunne et al., 2000; Aufdenkampe et al., 2001; Aufdenkampe and Murray, 2002; Le Borgne et al., 2002, Salihoglu, 2009) and on EqPac phytoplankton composition (Chavez, 1989; Iriarte and Fryxell, 1995; Lindley et al., 1995; Bidigare and Ondrusek, 1996; Chavez et al., 1996; Coale et al., 1996b; Landry et al., 1996, 2000; Latasa et al., 1997; Higgins and Mackey, 2000) that the authors seem to be unaware of.

Specific comments:

*I am not sure I follow what is suggested in the last sentence of the abstract. If the authors are suggesting that functional group modelling is important, this has been discussed extensively in various publications and this sentence is superficial as is.

*Introduction needs to better give the justification of using the model.

*Sections 3.1 and 3.2: To my knowledge La Nina was strongest during 1998 (McPhaden, 1999). Highest SeaWiFS chl concentrations are also observed during June-August 1998 (Murtugudde, 1999) , and towards the end of 1998 chl concentrations went down to pre ENSO conditions, it would be really interesting if the authors present results from 1998 La Nina and not only 1999.

*page 2532 line 5: references should be given

*Section 3.3: poorly written, it is not possible to follow the last sentence of page 2533 and the following part.

*pg 2535: authors should check the paper by Landry et al 1996 and the JGOFS data, there are some picoplankton data from 140W during the 1992 ENSO

*It is not possible to read the legends of Figs 3 and 6.

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