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Comment

***Interactive comment on* “Regulation of phytoplankton carbon to chlorophyll ratio by light, nutrients and temperature in the equatorial Pacific Ocean: a basin-scale model” by X. J. Wang et al.**

Anonymous Referee #3

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The authors apply a Phytoplankton-Zooplankton-Detritus model with nitrate and iron as nutrients embedded in a dynamical ocean model. A new model component is documented that describes the relationship between chlorophyll and carbon in phytoplankton. The equations describing the C:Chl ratio in the model are simple and transparent. First, C:Chl is assumed to be maximal for small growth rates and to decrease linearly with increasing (non-light limited) growth rate. Second, C:Chl is assumed to decrease linearly with depth. Results of the model are discussed for the equatorial Pacific and compared to in-situ and satellite-based Chl data.

The topic of this MS is well suited for publication in BG. It is justified to document the

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model approach and to present results for the modeled C:Chl and Chl data. However, significant work is needed to improve the presentations of the findings before this manuscript can be published in Biogeosciences; a major revision of the manuscript is required.

1) The focus of the abstract as well as of the new model component is on C:Chl ratio. However, the bulk of the manuscript deals with Chl concentrations. More space should be devoted to discuss C:Chl ratios.

The comparison between in-situ C:Chl data and model results (brief discussion in the last paragraph on page 7) is not sufficient. A figure comparing modeled and in-situ C:Chl data and a quantification of root mean square deviations between modeled and in-situ C:Chl would be useful. The reader is also wondering why he should believe that modeled C:Chl is realistic as the model predicts lowest C:Chl ratio near the equator in contrast to observations. What is meant with $\&\#8220$;There are some differences in the C:Chl ratio between model and observations. $\&\#8221$;

2) I miss a thorough data-model comparison in particular for C:Chl ratio, but also for Chl concentrations. A Taylor diagram or statistics as presented in table 3 for comparing the results from the sensitivity simulations should be applied to compare modeled C:Chl versus in-situ observations and to compare modeled Chl versus in-situ and SeaWifs data

3) I miss a discussion that relates the C:Chl parameterization used here to other model approaches.

4) The spatio-temporal variability of Chl and Chl:C and its link to ENSO remains unclear. It would be helpful if the MS includes a panel showing modeled and observation-based ENSO index that would allow the reader to link the plotted distributions with ENSO. It would also be helpful to add a new section to discuss the spatio-temporal variations in more detail.

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I am wondering how the Chl and Chl:C distribution looks for typical El Nino and La Nina situation. Does the model capture observed changes between an El Nino and a La Nina? What is the relationship between the Chl distributions shown in Fig 8 and the temperature/density distribution? Is there a link between Fig. 4,5,6 and Fig 9? Is the redundancy between Figure 2b,d,f and Figure 3b,d,f justified?

Further comments 1) It should be made clear in the abstract that the conclusions on the C:Chl ratio are from the model sensitivity simulations (Fig. 11, Table 3). The wording "This study demonstrates that"; used in the abstract is misleading. I first assumed that the findings were derived from the field data.

2) section 2.2: The derivation of eq. 5 from eq 1 requires a constant light attenuation coefficient k_a . Is this a realistic assumption? Please discuss why k_A can be assumed to be independent of depth.

3) Line 19, Eq 2 suggests that $I(Z=0)=I_0$. Thus I_0 appears to equal PAR at the surface and not mixed-layer averaged PAR as stated in the text below eq. 2.

4) Section 2.3 It is not clear how the model calibration has been done.

5) Section 2.3 1, para line 12: could you clarify the meaning of "a reasonable job";. Please be quantitative.

6) Sec 2.3 line 25/26 two typos:

7) Sec 2.3 last para: A figure showing modeled versus observation-based C:Chl values would be useful here. Please expand discussion on C:Chl data-model comparison and provide root mean square deviation or other statistical measures.

8) Sec 2.4 line 20: please clarify what is meant with cold and warm phase of ENSO. I assume the cold phase corresponds to strong upwelling off SA; right?

9) Section 3.1.1, 2.. paragraph: The discussion on the spatio-temporal variability of the Chl field should be improved. It would be helpful to add panels in Fig 5 and 6 that show

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the difference to the results plotted in Figure 4. This would allow the reader to compare the modeled change in Chl from Sep 2005 to Sep 2006 to May 2007.

10) Sec 3.1.2 No explanation is given for the modeled Chl difference between Oct 94 and May 96 as shown in Figure 8. I am confused by Figure 8 (see also comment 8). I would have expected that the Chl isolines would be slopping upwards during the cold phase of the ENSO cycles related to strong upwelling off South America (?) in Apr/May 96. In contrast Fig 8 shows upward slopping for the warm phase/Sep 94. Please explain.

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