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Biogeosciences
Discussions

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.

X. J. Wang et al.

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Preface: We have made a major revision in response to three reviewers' constructive comments. As a result, some elements have been dropped off from the revised manuscript. Therefore, some of the following comments are not applicable (N/A) anymore.

Referee #1

General Comments:

My main concern is the separation of the signals of spatial, seasonal and interannual variability in the model-data comparison, which is not done appropriately in the present form of the manuscript. The equatorial Pacific is characterized by strong seasonal and interannual variability and distinct spatial separation into the Western Pacific Warm Pool and the Equatorial East Pacific (EEP) upwelling zone. The results therefore have to be analyzed and interpreted more in the context of these general characteristics. For example, the time periods chosen in Figures 4-6 are determined by data availability. It has to be mentioned to what phase of the ENSO cycle (warm/cold) the periods belong and the results have to be explained against this background. A more consequent spatial separation between results from the Western Pacific Warm Pool (WPWP) and the Eastern Equatorial Pacific (EEP) upwelling regime would also help for clarification.

$>>$This is a truly good point. The issue of spatial and temporal variability is extremely important for the equatorial pacific. Therefore, we have specifically addressed this issue in a manuscript entitled 'Spatial and temporal variability of the phytoplankton carbon to chlorophyll ratio in the equatorial Pacific: a basin scale model study' (JGR MS). We have included this reference. However, the author is correct that we should link to the ENSO cycle when we present the results. We have done so accordingly during the revision.

At several points the authors speak about model skill and model performance, however, no such analysis is presented. The evaluation of model skill should yield some statistical value that enables to rate the model among alternative solutions. This is not the case in the present study. More information on the model tuning, which is mentioned in the text but by no means explained, and the criteria applied could probably serve as evaluation of model skill.

$>>$We appreciate the referee's constructive comments. We have done a great effort on model skill assessments (i.e., statistical analyses and a new figure of Taylor diagram as suggested).

The study gives a brief and well formulated introduction into the usefulness of C:CHL ratios of phytoplankton for the determination of marine productivity and car-

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bon turnover. However, a few more words about photo acclimation and its relevance for the ecosystem and carbon cycling would be desirable, particularly in the context of anticipated future climate change and ocean stratification.

>>This is a good suggestion. We have added 'These environmental conditions are most likely to change in association with anticipated future climate change and ocean stratification, suggesting implications for the marine ecosystem dynamics and the carbon cycle' in the Introduction.

When comparing the model output with CHL-data from remote sensing it has to be made clear that these are not direct measurements either. Satellite CHL is derived from ocean colour, which involves a further model application.

>>We have added 'The in situ Chl concentrations are considerably higher than the SeaWiFS Chl on the equator, suggesting further calibration is needed to derive Chl from the ocean color'.

Specific comments:

p. 3875, l. 6-13: Is the cited linear relationship between C:CHL and growth rate positive or negative or is this unclear? Please clarify your statement in the text.

>>We have reworded as 'Le Bouteiller et al.(2003) shows that the surface C:Chl ratio linearly decreases with increasing the growth rate under non-light limitation conditions'

p. 3876, l. 20-24: maybe you should also mention sub grid scale (spatial) variability in the field data, that is probably not resolved by the model

>>We have added 'Field data may have fine spatial and temporal resolutions that are probably not resolved by the model'

p. 3876, l. 25: typo: ’field’

>>Corrected.

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p. 3877, l. 1: replace ’coherent with’ by ’represented by’

>>Done.

p. 3877, l. 2-4: I cannot see a significant difference between the results for those two transects, please see also comments below for Figures 2 and 3.

>>We have removed Figure 2 as suggested.

p. 3878, l. 13: there is no evaluation of ’model skill’ in the present study. The study is a model-data comparison, while the evaluation of skill should include a further statistical measure (skill-score), please see general comments above.

>>We have added one section about the model skill.

p. 3879, l. 10: model performance (see comments above)

>>See response above.

p. 3880, l. 13-14: ’annual mean longitude of the front between the HNLC and the warm pool’: please explain the meaning of east-west shifts of this front.

>>We have reworded this paragraph and also provided an explanation as requested.

p. 3880, l. 26: ’under-estimate’: compared to what?

>>We have added one panel showing both in situ and SeaWiFS Chl.

p. 3881, l. 3-5: where can those results be seen?

>>N/A.

p. 3882: the results explained here are difficult to be found in Figure 11, please see comments for Figure 11, below.

>>We have addressed this issue (see response below).

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p. 3883 (p3884?): l. 3: I do not agree that the ratio of standard deviations for iron is so much better than the one for nitrate in the frontal zone.

>>We have reworded this paragraph.

Figure 2: On the left shown are model results for temperature, salinity and nitrate, which are not compared to observations. I would suggest to either include observations and to derive a model skill score for ocean circulation and nutrient distributions from this comparison or to remove Figure 2 entirely. The sub panels (b), (d), and (f) are not very much different from what is shown in Figure 3 and therefore not needed.

>>We have removed Figure 2.

Figure 3: It is a good idea to display zonal and meridional transects of model output. However, to get a better idea of the 3-D distributions and the distinction between the WPWP and the EEP it would be good to have a second meridional transect for the eastern part of the Pacific (e.g. 120W). Furthermore, Figure 2 shows that the high sub surface CHL concentrations close to the equator are restricted to a very small area (Figure 3b). They are averaged out in the zonal section (Figure 2a) when a too large region (2N-2S) is included. I suggest displaying Figure 2a, c, e from 1N-1S.

>>We have modified this figure (now, numbered as Figure 2) as suggested (e.g., 1N-1S). We have addressed the issue of WPWP vs. EEP in the JGR MS.

Figure 4: To my eye the differences between the two meridional transects (125W, 140W) are not obvious. I’d suggest to either remove one of them or to chose two transects that clearly separate between the Western Pacific Warm Pool (WPWP) and the Eastern Equatorial Pacific (EEP) upwelling zone. Anyways, the more western transect should be displayed in the left column, the more eastern transect on the right.

>>We prefer to present both transects for two reasons: (1) the in situ data have never been published, and (2) readers may want to know if there is any difference between 125W and 140W. We have modified Figure 4 as suggested.

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Figure 9: I suggest separating the Hovmoeller Diagrams into seasonal and interannual variability by showing first the average seasonal cycle of CHL and then the temporal evolution as anomalies to the seasonal CHL. This allows for a clear distinction between seasonal and interannual variability, once as ’observed’ by the satellite and once as obtained from the model.

>>We have specifically addressed the issue of seasonal and interannual variability in the JGR MS. The focus of this manuscript is model validation and relative role of light, nutrients and temperature regulating the C:Chl ratio.

Figure 10: Please move sub panels (e) and (f) to Figure 11 (see comments below).

>>We have made considerable modification during the revision so this is not applicable any more.

Figure 11: It is difficult to distinguish the actual differences between the sensitivity experiments and the control experiment. I’d suggest to show the absolute values for C:CHL ratios from the control in the top line of Figure 11 and then display the results from the sensitivity experiments as anomalies (differences) to the control. Here again two meridional transects, as exemplary for the WPWP and for the EEP, might give a clearer picture of the spatial patterns (see comments for Figure 3, above).

>>As suggested, we have made two new figures showing the differences.

Referee #3

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 referee is correct about the space issue. This manuscript has a focus on model validation. There is limited C:Chl data so we have to use chl data for model validation. However, we have removed three chl figures (i.e., Figures 5, 6 and 8), and added one section discussing the effect of a variable C:chl ratio.

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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 “There are some differences in the C:Chl ratio between model and observations.”

>>We have added one Taylor diagram for model-data comparisons.

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.

>>See response above.

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

>>We agree that such a discussion is useful. However, we think that it should involve with model comparisons, which can form another manuscript. Nevertheless, there are some discussions related to this issue in this manuscript (e.g., in the Introduction and Methodology) and in the JGR MS.

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.

>>Done (see Figure 6c). The spatio-temporal variations are addressed in the JGR

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MS.

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?

>>The JGR MS presents the Chl and C:Chl changes and discusses links to ENSO. However, we have added one panel showing Chl with SOI.

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.

>>We have added the word 'modeling'

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

>>We have added 'There is evidence of uniform distribution in particulate organic nitrogen in the euphotic zone of the equatorial pacific (Wang et al., 2008), suggesting probably uniform distribution of detritus. Observation also shows relatively uniform distribution of chlorophyll concentration in the upper 100 m at 0° and 3°S along 180° during the EBENE cruise (see Figure 6 in Le Bouteiller et al., 2003). Hence, we assume that kA is constant in the euphotic zone'.

3) Line 19, Eq 2 suggests that I(Z=0)=I0. Thus I0 appears to equal PAR at the surface and not mixed-layer averaged PAR as stated in the text below eq. 2.

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>>We have made clear by adding '(Z>0)' above eq. 2.

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

>>There was no real calibration because we did not have the data until recently. We have made correction.

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

>>We have reworded, and also provided statistical analyses.

6) Sec 2.3 line 25/26 two typos:

>>Corrected.

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.

>>Done.

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?

>>We have added '(i.e., strong upwelling in the equatorial Pacific)'

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 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.

>>We have added one panel (i.e., Figure 6c) showing Chl concentrations with SOI.

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

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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.

>>N/A. To answer this question, strong upwelling during the cold phase of ENSO pushes the HNLC front to the west of the dateline, leading to high nutrients in some parts of the western equatorial Pacific (e.g., at 165°E in May 96). So the entire sampled area between 165°E and 150°W experienced HNLC conditions in May 96, revealing almost same DCM depth. The HNLC front was placed to the east of the dateline (˜170°W) in Oct. 94. So the sampled area displayed two regimes, the oligotrophic to the west with deeper DCM (˜100 m) and mesotrophic to the east with shallower DCM (˜50 m). More information can be found in Le Borgne et al. (DSR II, 49: 2471-2512, 2002).

C. Voelker (Referee)

Firstly I think that the authors do not give enough justice to other works that attempt to describe variations in phytoplankton C:Chl. Their parameterization is really quite similar to the parameterization of Cloern et al. (1996, An empirical model of the phytoplankton chlorophyll:carbon ratio - the conversion factor between productivity and growth rate, Limnology and Oceanography 40, 1313-1321), but neither is this work cited nor are the differences and similarities between the two parameterizations discussed. Also the authors state referring to previous parameterizations (citing explicitly Geider et al., 1998) "Most of these approaches ... prescribe the relationship between the C:Chl ratio and temperature dependence of growth rate because of a lack of observations to parameterize the nutrient dependence. Thus, the combined eﬀects of light, nutrients and temperature on the phytoplankton C:Chl ratio are not well known, and not yet quantiﬁed". While I agree that the combined effects are not well known, at least the model by Geider et al. (1998) shows some reaction in equilibrium C:Chl to nutrient levels. The papers by Pahlow (2005, Linking chlorophyll-nutrient dynamics to the Redfield N:C ratio with a model of optimal phytoplankton growth, Marine

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Ecology Progress Series 287, 33-43) and Smith and Yamanaka (2007, Optimization-based model of multinutrient uptake kinetics, Limnology and Oceanography 52, 1545-1558) are also interesting in that respect.

>>We appreciate these additional references. We have included them, and reworded a few sentences to address this issue.

Secondly it is unclear to me, whether the variable C:Chl ratios from the parameterization enter the prognostic equations for phytoplankton growth by changing the slope of the photosynthesis-irradiance curve or whether it is a pure diagnostic relationship that only affects the conversion from nitrogen units to chlorophyll after the model run. Maybe this information is contained in the cited paper by Wang et al. (2008), but it would be helpful to have that information here in this paper, since it affects the interpretation of the results.

>>We have added 'Then we compute chlorophyll concentration using a diagnostic conversion, i.e., the phytoplankton C:Chl ratio'

Thirdly, while the modeled chlorophyll distributions look quite good, it would be helpful to see what effect the variable C:Chl ratio has on the distribution of the chlorophyll field, e.g. by comparing it to a chlorophyll field generated with a constant C:Chl ratio. The question is whether a variable C:Chl ratio gives a better fit to Chl observations than a fixed one. In principle this could be simply done by showing modeled phytoplankton N, converted with a constant average N:Chl ratio, i.e. without a new model run. However, if the model uses the variable C:Chl in the prognostic equation for phytoplankton, the authors might also consider re-running the model with a fixed C:Chl ratio, if that can be done without too much additional work.

>>We have added one small section to address this issue.

Finally, the authors use a constant (Redfield) carbon to nitrogen ratio to convert from their nitrogen-based model to carbon units and then finally to chlorophyll. This is a perfectly justified approach, but since the modeled region encompasses quite different nutrient regimes, it is maybe worth a short discussion whether C:N might also vary here.

>>This is a good point. We have added 'Our approach of applying a constant C:N ratio may have implications for chlorophyll estimation. However, the uncertainties or potential errors would be small because of relatively small range of C:N ratio in phytoplankton uptake (Raimbault et al., 1999)'.

I have no other minor comments than the two other referees, except: Figure 1: The linear fit of C:Chl ratio to depth is probably o.k. for the purposes of tis study, but looking at the data one gets a hint of a systematic curvature in the data that is not present in the fit: C:Chl are slightly underestimated by the fit near the surface and at depth, while they are overestimated in mid-depth.

>>This is an interesting point. The referee is correct that the linear relationship may not hold at depth, e.g., below the euphotic zone. More field data are needed to better understand the C:Chl variations.

Interactive comment on Biogeosciences Discuss., 5, 3869, 2008.