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Biogeosciences Discussions

Interactive comment on "Impact of physical processes on the phytoplankton blooms in the South China Sea: an eddy-resolving physical-biological model study" *by* Y. Sasai et al.

Anonymous Referee #2

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General comments:

This paper investigated how the phytoplankton bloom was influenced by the monsoon driven upwelling, anticyclonic eddies and the intrusion of the Kuroshio in the South China Sea (SCS). The author used an eddy-resolving coupled model, which was an interesting method for studying the role of physics on biogeochemical processes in the SCS region. Unfortunately, however, the authors did not adequately validate the model they used in this study. They need to fully discuss the advantages and disadvantages of the model and let the reader know what the limitations of the model are, and why it

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is suitable for this study despite these limitations. In addition, many facts appeared in the model that are not well described and appropriately interpreted. The mechanism associating the potential physical influence on biology needs further analysis before the conclusions being reached. I feel the paper need a major revision before considering being published on BG.

Specific comments:

1. The comparison of model and observations needs to be improved before we can trust that the model is a valid tool to study the issue addressed by this paper.

a. One main conclusion reached by the paper is that the phytoplankton bloom in the SCS is caused by the upwelled nutrients driven by the monsoon, eddies or Kuroshio intrusion. The authors had an extensive description of the simulated variability of the vertical structure of nutrients, chlorophyll, and thermocline. However, the model validation work was limited to the surface chlorophyll using SeaWiFS only. To give the readers more confidence that the model is suitable for this study, the authors should provide some evidence that the nitrate field from the simulation is comparable to observations. In addition, does the model reproduce the vertical structure of chlorophyll? I encourage the author to use shipboard measurements in this region, or at least provide some numbers or figures from the literature to expand the model-observation comparison in these respects.

b. Even for the surface chlorophyll comparison, the OFES simulation (Figure 1b) is unlike the SeaWiFS observations (Figure 1a). There is a high chlorophyll region along the southwest China, Vietnam Coast, and Philippine island in the SeaWiFS data, but not in the OFES. The author explained this was because the model didn't include the river. Is the river-delivered nutrient an important nutrient source for the open SCS in addition to the upwelling (The conclusion on p.1590 line 23-24 says that the river-discharged nutrients initiate the phytoplankton plume. It seems as if this is important?) Why not first include the river for the model-observation comparison, and then perform

BGD
10, C398–C404, 2013

a scenario experiment by turning the river discharge off to examine the upwelled nitrate influence?

c. Instead of just saying that the model shows a general pattern comparable with the observations, the comparison results need to be quantified. It is best if the author can show the model-observation comparison results using some type of quantitative analysis, for example by using Taylor or target diagrams (Jolliff et al. 2009) or a Willmott skill score (Willmott, 1981). At least the author should provide some simple skill numbers such as the bias, correlation coefficient, or RMSD.

d. Since the paper has two sections to discuss how the biological field altered by the eddy and declared this is an "eddy-resolving" physical-biological model. It is better to include one figure to show the eddy field from the model resembles the observation.

2. Many interesting outputs from the model need better descriptions and some model results need further analysis before substantial conclusions can be reached.

a. On Fig. 3a (Box-L), the model shows that the highest chlorophyll concentration was in January. The author said "this is consistent with the shallow thermocline depth, the high nitrate concentration in the upper 73m, and the strong vertical nitrate flux". However, the shallowest thermocline depth and highest nitrate concentration in the upper 73m occurred in February instead of January and the highest nitrate flux was actually in December. The similar phenomena could also be observed in Fig. 3b (Box-V), the highest chlorophyll concentration was in August, but the shallowest thermocline depth and highest nitrate concentration was in October. Why they are not synchronous? Shouldn't the chlorophyll peak lag the nitrate peak?

Another similar example exists in Fig. 7 and Fig. 8. When the eddy passes by, we see low nitrate concentrations within the anticyclone eddy and high nitrate concentrations south of the eddy at 73m depth (Fig.7c and Fig. 8b), but the surface high chlorophyll region does not match the high nitrate region all the places (Fig. 7b). The surface high chlorophyll was restricted to the edge of the eddy. Also, the chlorophyll maximum

BGD

10, C398–C404, 2013

always stays subsurface instead of sometimes being at the surface (Fig. 8a). The unmatched nitrate and chlorophyll fields can also be observed in Fig.9 and Fig.10 (the Vietnam coast). Can we attribute the variability in the chlorophyll field all to the upwelled nutrients? Is the chlorophyll being transport by Ekman and eddy movements? Is chlorophyll concentration changed as the thermocline depth fluctuated? What about the top down control here? Does zooplankton play a role? The authors need to exclude these other influential factors to conclude that the phytoplankton bloom was caused by the upwelled nitrate.

b. On Fig. 4c, the author found that 2004 was the year that chlorophyll from OFES didn't peak and explained this was caused by a modest reduction in the strength of the summer monsoon. However, on Fig 4d, the plot of wind didn't clearly show a reduction in wind stress in 2004. The author needs to quantify how much the wind was reduced in 2004 compared to the other years, and calculate how much the upwelling velocity and nitrate flux was reduced because of the reduction in wind stress. This would be a useful addition to the text.

c. Page 1584, Line 19 – Line 26: The author found surface nutrient and chlorophyll was reduced despite the elevated depth-integrated nitrate, and the model failed to capture the extremely high chlorophyll events at the end of 2005 and 2007; yet, the author didn't provide any analysis or discussion as to why.

d. Page1585 line 6 and Figure 4: Why does OFES overestimate the chlorophyll peak compared to the observation?

e. Page 1585 line 12: Why does the subsurface maximum in chlorophyll stay for a few months after the surface bloom?

f. Page 1586 line 1-2: "The peak in the subsurface maximum occurs slightly later in the year (around September/October) than at the surface". Why is the subsurface later? It looks to me as if the timing of the subsurface maximum was more synchronous with the maximum nitrate concentration and thermocline depth in Fig. 3 (maximum in October).

BGD

10, C398–C404, 2013

Why?

g. Page 1588 line 6 – Line 9: The author mentions the intrusion of the Kuroshio impact on the nutrients and chlorophyll field. Although the manner similar to the anticyclone eddy, it still needs to expand the discussion and include 1 or 2 figures to show it.

3. I believe the model has the ability to reproduce the seasonality of the chlorophyll field due to upwelling effects, but it seems to me that the model lacks the ability to reproduce the interannual chlorophyll variability (Fig. 4a and 4c). The discussion of the interannual variability in section 3.2 was not particularly illuminating. The author probably should consider removing this section. However, there are a couple potential interesting points in this section that the author should consider keeping and expanding:

a. The potential influence of reduced wind stress on nutrient upwelling. Maybe an experiment reducing the wind stress by 20% could be performed?

b. The scenario in which an eddy passes by before, in the middle of, and after the phytoplankton bloom. (This was actually discussed in 3.3 P1587 Line 26 – P1588 Line 5)

4. The methodology part needs more description to allow the readers to repeat the experiments:

a. P1581 Line 22- 23: “the biological model is incorporated into the physical model after the 50-yr spin-up with climatology and for a further 5-yr period under the climatological monthly mean forcing”. What is meant by “50-yr spin up with climatology” vs. “climatological monthly mean forcing”? What climatology is used for the 50-yr spin up, if it is not monthly means?

b. Although an ecosystem model has been published in Sasai et al. (2006, 2010), please consider including a simple description on the biological model in the text or in an appendix, especially the source and sink terms. The model doesn’t have river

BGD

10, C398–C404, 2013

included. Is atmosphere nitrogen deposition important? Is there any kind of denitrification process included in the model as a nitrate sink?

Technical corrections:

1. P1570, Line 17 – Line 27: This paragraph talked about the potential ENSO influence on the SCS; however, the ENSO influence is only discussed at the end of the conclusions and is distractive. Please consider removing it.

2. P1581, Line 18-19: In the sentence "The source and sink terms represent the biological activity", please be specific about which biological activities are represented and what source and sink terms are included (see specific comments 4b).

3. P1582, Line 10: In the statement "Ocean color satellite images reveal strong seasonality of . . .", the word "strong seasonality" was not quite appropriate to use here, because the results didn't show the surface chlorophyll distribution in all four seasons or representative months of the four seasons. Only August and December are compared. Please consider revising.

4. P1582, Line 15, 16, 17: The statement "the surface physical fields (Ekman pumping . . .) support the peak conditions of model surface chlorophyll" doesn't explain how they are related in the following sentence. It seems that was explained from Page 1582 line 25 to Page 1583 line 4. Please consider moving them together.

5. P1582, Line 22-23: "The simulated chlorophyll distribution represents same pattern . . ., but has a relatively low concentration along the coast of southwestern China". The chlorophyll is not just low along the coast of southwestern China but also along the Vietnam coast.

6. P1583, Line 19-20: In the statement "The strong Kuroshio inflow also effects on the spreading of surface chlorophyll distribution" the words "effects on" should be "affects". Also there is no explanation about how the Kuroshio influences the chlorophyll distribution. Please either expand the sentence with some explanation or remove it.

BGD

10, C398–C404, 2013

7. P1585, Line 5: The word “year boundary” should be changed to “the beginning and the end of the year”.

8. Figure 5 and Figure 6: Please plot the thermocline and nutricline depths on top of the chlorophyll concentration to better show that they co-vary.

9. P1586 Line 7: “During the northwestly winds . . .” should it be “northeasterly wind”? Also on the same page Line 13: “during the northwest monsoon . . .” should it be “northeast”? P1587 Line 4: “because of the strong northwesterly wind”, shout it be “northeasterly”?

10. P1586 Line 10: “The OFES reproduces the number of eddies in the northwestern Luzon during the northeast monsoon (Fig 2)”. Figure 2 doesn’t support this statement. Please consider revising this.

11. P1587 Line 9-Line 10: ”There are large upward and downward motions associated with the eddy”. This sentence states a fact that most people know and has loose association with the previous sentence. Please consider removing it.

Some useful references:

Jolliff, J. K., Kindle, J. C., Shulman, I., Penta, B., Friedrichs, M. A., Helber, R., and Arnone, R. A. (2009). Summary diagrams for coupled hydrodynamic-ecosystem model skill assessment. Journal of Marine Systems, 76(1), 64-82.

Willmott, C. J. (1981), On the validation of models, Phys. Geogr., 2, 184 – 194.

Interactive comment on Biogeosciences Discuss., 10, 1577, 2013.