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Biogeosciences Discussions

Interactive comment on "Simulating the effects of phosphorus limitation in the Mississippi and Atchafalaya River plumes" *by* A. Laurent et al.

A. Laurent et al.

arnaud.laurent@dal.ca

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Below the reviewer's comments were inserted. Responses are interspersed in italic font. Changes in the revised manuscript responding to these comments are highlighted in yellow.

Comment:

1. If the focus of the model is to simulate P limitation where high productivity and hypoxia occur on the Louisiana-Texas shelf, the selection of boxes to demonstrate potential variability do not fit well with areas of known higher productivity and areas of hypoxia. The Miss Delta box includes areas of $>$ 50 m depth where hypoxia seldom occurs. The Miss Intermediate box is appropriate but extends too far offshore, $>$ 50 m

unlikely to have hypoxia. Also this box is often transposed with language "west of Terrebonne Bay" as an alternate term or a subset of the box. The Atch Delta and the Atch Intermediate are appropriate but could be extended further offshore. The Far Field actually contains area that is sometimes hypoxic, and in 4 of the 7 years that are simulated.

Response:

Reviewer 1 indicated that the subregions Mississippi Delta and Mississippi Intermediate are not well chosen in terms of representing productivity and hypoxia. We would like to emphasize that the subregions were used for spatial averaging to illustrate patterns of surface variability and geographical gradients in surface properties; they are not meant to coincide with the known area of bottom water hypoxia.

Definition of subregions for averaging is always somewhat arbitrary; however, we feel that the regions we chose are sensible because a) they do represent spatial gradients of productivity and surface chlorophyll concentrations well, and b) the Mississippi Delta and Intermediate regions and the Far-field region have been used in several previous studies. The Mississippi Delta and Intermediate regions were used by Lohrenz et al. (1997; see their Fig. 2) when analyzing patterns of primary production. The Mississippi Delta and Intermediate regions and the Far-field region were also used in Fennel et al. (2011). We would argue that it is sensible to continue using these regions in order to allow for comparisons with previously published results. Furthermore, while modifications of the averaging regions would lead to some quantitative changes in the presented results, there would be no qualitative changes to any of our conclusions.

In order to show that the regions represent relatively homogenous areas of surface chlorophyll we show the region outlines with simulated and observed surface chlorophyll in the new Fig. 5 (for May, July and September) and for July in Fig. 1 below. The panels in Fig. 1 below show that the Mississippi Delta region is relatively homogeneous at this time, with high chlorophyll concentrations. The Mississippi Intermediate region

represents a transition zone and the Far-field region encompasses the lowest chlorophyll concentrations on the shelf (i.e. lowest concentrations inside the 100 m isobath).

In response to the reviewer's concern about the expression "west of Terrebonne Bay" it was replaced by "west of 91° W". We use this spatial reference to describe the spatial maps and not the spatially averaged time series.

Comment:

2. Not clear why this is called the Texas-Louisiana shelf, when 3 boxes where hypoxia is most likely to occur are in Louisiana shelf waters, and the Far Field is in Texas waters. Suggest Louisiana-Texas shelf.

Response:

We had used "Texas-Louisiana shelf" to be consistent with previous modeling studies using the same grid domain and because the model grid extends into Texas waters. However, since most of the areas discussed in the manuscript are located in Louisiana shelf waters we replaced all occurrences of "Texas-Louisiana shelf" with "Louisiana shelf".

Comment:

3. A major problem with this paper is the assumption that waters are either limiting by N or P for production of phytoplankton, when it is obvious from multiple studies that dual N+P limitation is often the case, especially across a salinity gradient, and sometimes even silicate. The simplification of either one or the other, along with boxes that cross a wide range of salinity, productivity, depth, and likely occurrence of hypoxia begs the question as to what is being simulated that might be of relevance to productivity or hypoxia.

Response:

Typically, limitation patterns from multi-nutrient ecosystem models are presented as-

suming that only one nutrient is limiting at a time. However, we agree that it is worthwhile to consider co-limitation and revised the manuscript accordingly. We revised the criteria for limitation as follows:

1) N limited: if $L_{\mathrm{N}} < (L_{\mathrm{P}} - 0.1)$ *and* $L_{\mathrm{N}} < 0.75$

2) P limited: if $L_{\mathrm{P}} < (L_{\mathrm{N}} - 0.1)$ *and* $L_{\mathrm{P}} < 0.75$

3) N+P limited: if $|L_{\mathrm{N}} - L_{\mathrm{P}}| < 0.1$ *and* $L_{\mathrm{N}} < 0.75$ and $L_{\mathrm{P}} < 0.75$

4) light-limited otherwise

Fig. 6, 7 and 10 (now Fig. 7, 8 and 11) were updated using the new criteria. The occurrence of N+P co-limitation is limited however and does not change our previous conclusions.

Comment:

4. The available data go from Sylvan et al, Dortch and Whitledge, Smith and Hitchcock and Quigg et al. then slowly drops off all but Sylvan et al., which is a quite limited representation of the shelf. The data for 2002 are dropped because the authors assume methodological differences, but the models by Wang and Justic and Justic and Wang use the 2002 data in a coupled physical (FVCOM) and eutrophication model to accurately simulate an annual cycle of stratification and hypoxia on the shelf.

Response:

We use the data collected by Sylvan et al. (2006, 2007, 2011) and Quigg et al. (2011) because their studies were carried out to look specifically at nutrient limitation in the Mississippi/Atchafalaya river plumes. Their dataset represents 712 observations, which is a significant amount of data, and covers the plume area well (see new Fig. 7). The patterns of nutrient limitation that were calculated from their nutrient dataset (using the limitation factors L_{N} *and* L_{P}*) are consistent with their findings using other methods*

(alkaline phosphatase activity as indicator of P-stress, nutrient addition and APA bioassays, as well as FRR fluorescence (Sylvan et al. 2007) as indicator of nutrient stress and used in conjunction with the other methods), which provides independent lines of evidence in support of our results. We clarified this point in the manuscript (page 7, lines 191-193). We didn't use the data from Dortch and Whitledge (1992) or Smith and Hitchcock (1994) because it doesn't match our simulation period (2001-2007).

Wang and Justic (2009) and Justic and Wang (2009) discuss the development of a FVCOM model of the northern Gulf of Mexico and its application to study the physical processes affecting the development of hypoxia. Their model includes only physical processes. We are not aware of any published work with a coupled FVCOM-eutrophication model using any nutrient dataset.

Comment:

5. The authors too quickly dismiss the use of total P and total N for the N:P ratio in screening for potential N or P limitation. There are several authors, Maestrini et al. (1984a, b) Hecky and Kilham (1988), Klapwijk et al. (1988), Dodds (2003, 2006) and Lewis et al. (2009) concerning the usefulness of the inorganic chemical ratios to define P or N limitation.

Response:

Our analysis relies on DIN and DIP rather than total N and total P for several reasons (some discussed in the introduction) including the very practical reason that the model does not include DON or DOP. Furthermore, the reviewer brought into question the usefulness of inorganic nutrient ratios as a measure of N or P limitation. We are not determining nutrient limitation from direct inorganic nutrient ratios. We use Michaelis-Menten dynamics to calculate the nutrient factors in our model (Eqs. 1-3), which are used to calculate nutrient uptake by phytoplankton (Eq. A1). We use the same calculation with the observation dataset to allow for a direct comparison with model results. As mentioned above, the nutrient limitation factors calculated from the observations of

Sylvan et al. (2006, 2007, 2011) and Quigg et al. (2011) are consistent with other findings using several independent methodologies to assess nutrient limitation. Observational evidence for P-limitation is therefore strong. We don't expect the limitation patterns to change if dissolved organic nutrients were included.

We are aware that some uncertainty is introduced to our results by not accounting for DOP and discuss this issue carefully in the manuscript (page 11, lines 341-347). Despite a consensus on the necessity to explore the sources and utilization of DOP on the Louisiana Shelf, large uncertainties about the use of DOP by phytoplankton remain (e.g. Dagg et al. 2007, Quigg et al. 2011). The nutrient addition bioassays of Quigg et al. (2011) demonstrated uptake of DOP, but phytoplankton responded to only one of the two types of DOP added, and the overall importance of DOP is not clear. In addition, the magnitude of DOP input to the Louisiana Shelf is not well known and most likely not significant enough to alleviate P limitation (Quigg et al. 2011; see also comments above). Nevertheless, the sensitivity of our results to variations in P load (revised Fig. 11) illustrates the variations in spatial and temporal extent of phosphorus limitation that would arise from an additional source of phosphorus.

Comment:

6. Do not agree that the simulated surface chlorophyll are as good in agreement with satellite observations as stated. Simulations are off for the two delta boxes, most likely because of complications in conversion of SeaWiFS data to chlorolphyll in Coastal Type II waters that are turbid. Also, as stated earlier the boxes cover large areas some of which are high in chlorophyll and others that are not.

Response:

Please see response to comments 2 and 4 by reviewer 3.

Comment:

7. There seems to be poor coherence between the simulated P and N limitations in

Fig 6 with the observed. And there are no data for the Atchafalaya intermediate area and the Far Field with which to compare.

Response:

We used a large number of observations (over 700 from 8 different cruises); in fact all the data available from Sylvan et al. (2006, 2007, 2011) and Quigg et al. (2011) to compare with the model (see also response to comment 4). The model successfully simulates the seasonal cycle of nutrient limitation on the Louisiana Shelf (i.e. 2001) and the succession of nutrient limitation away from the river plume. There are some spatial discrepancies with the observations, which is to be expected given that we are dealing with a turbulent river plume. These deviations are discussed in the manuscript (page 8, lines 223-231).

Interactive comment on Biogeosciences Discuss., 9, 5625, 2012.

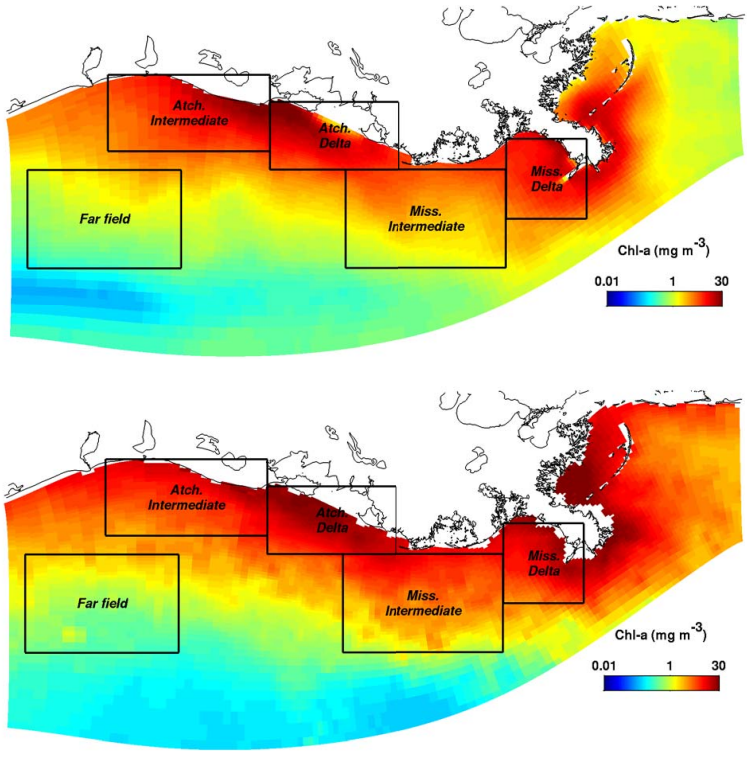

Fig. 1. Simulated (top) and observed (bottom) surface chlorophyll in July. Subregions are indicated.