Responses to Referee's comments

The reviewer's comments were inserted. Responses are interspersed in *italic font*.

Referee 2

Comment:

A 3D coupled physical-biological model is developed in order to diagnose biogeochemical rates and to understand their variability. The model is run for a region strongly impacted by river discharges and where strong interactions between pelagic and bottom processes are important. The area is concerned by the occurrence of hypoxic events and thus this study is important on an environmental point of view. After reading the abstract (lines 1-10) and the Introduction section (the whole section), we expect that the model will be used in relation to the occurrence of hypoxic events on the bottom of the Gulf of Mexico (see also the "model description" section page 127, lines 6-20). However, after reading the manuscript, we cannot find any point on hypoxia, even on the oxygen dynamics. Rather, the model is used to provide estimation of some biogeochemical rates (grazing, mortality, primary production) whose usefulness is quite difficult to assess.

Response:

Our objective in this manuscript is not to discuss the distribution of dissolved oxygen, but rather the processes that are thought to lead to hypoxia, i.e. nutrient stimulation of phytoplankton growth and the fate of this organic matter (through transport, respiration, grazing and sinking). We made modifications to the Abstract and the Introduction to make this more explicit and evident. We mention hypoxia, as is typically done in manuscripts describing observations or modeling for this region, but now make more clear that we are not discussing dissolved oxygen here. Model-results for dissolved oxygen will be described in a forthcoming manuscript.

Comment:

Before using a model to diagnose biogeochemical flows, this model has to be thoroughly validated which is not the case here. I agree that the validation of 3d model is a very tricky work that you can always criticize. However, in the case of this paper, the validation is very poor and should be considerably enhanced (see my detailed review). Mainly, the authors made some very rough qualitative comparisons of model results with observations obtained for a different period than the simulation period. Therefore, when the model shows very poor agreement, the argument of the authors is that the periods of data and model results are different. I suggest running the model for a period overlapping the availability of data set.

Response:

We extended the simulation period to 15 year (1990-2004). It now overlaps with the NECOP program (1991-1993), with Sylvan's observations (2001-2004) and the SeaWiFS period (end of 1998 onward). In addition we expanded the model validation (see, in particular, the new figures 4, 5, 8, and 9).

Comment:

Besides, assessing the performances of 3D models require the use of spatial and temporal statistics of errors in order to assess the ability of the model to represent the correct pattern in space and time. The use of these statistics offers a more quantitative estimation of model performances compared to a simple visual comparison. The authors provide a table of statistics but only refer to it with absolutely no analysis on the values of these statistics (for instance, do they consider that a RMS of 6 acceptable or a correlation coefficient lower than 0.6?). There are statistics for which you have threshold values allowing to assess more “objectively “ the model error and judging whether it is acceptable or not (see for instance, Allen and Somerfield, 2007, 2009; Lewis et al., 2006, Holt et al., 2006).

Response:
We substantially revised the model validation section, included new data sets and figures (note specifically Fig.s 4, 5, 8 and 9). We removed the table, but included correlation coefficients and goodness-of-fit in Fig.s 6 and 9, respectively.

Comment:

I also have serious concerns about the spin up time of the model (the model is started from homogeneous conditions), the influence of open sea boundary conditions on the model solutions, the influence of benthic processes which have to be important in a shallow environment. I found the analysis of model results not very relevant. The authors are trying to find correlation between the nutrient loads discharged buy the rivers and as far as I understood the nutrient uptake rates (referred as primary production). Of course there is a relation when nutrients are not limiting since it is in the equation.

Response:
The spin-up time for the water column model is on the order of a few weeks at most and thus not explicitly discussed. We agree that diagenetic models require a long spin-up time, but would like to emphasize that our model does not include a diagenetic model. The benthic parameterization is instantaneous; in other words, there is no benthic memory in the model system.

With respect to the correlation, we show that nutrient load and phytoplankton growth rate are not positively correlated in the vicinity of the delta (as one would expect given the light-limitation), yet nutrient load and primary production are. We discuss the reasons in detail.

Comment:

This paper requires a substantial major revision. To improve it will require several months of work with for instance improving significantly model results which requires new simulations on a different period of time when data are available, to refocus the aim of the manuscript on important key- questions (e.g. relation between nutrients loads and occurrence of hypoxic events, nitrogen budgets of different areas, quantification of the export of nitrogen between the delta and offshore, interaction between the upper and sediment parts) and not to show maps of rates and limitation terms computed by the mode). Some sections need to be rewritten. I suggest to improve the description of the model by adding notably a schematic representation of the state variables and flows between them, a brief description of the processes taken into account.

Response:
The manuscript underwent a major revision. We extended the simulation period, significantly extended the model validation and attempted to better emphasize our key questions. As suggested, we included a schematic of the biological model (new Fig. 2) and a brief description of the biological process represented by the model (in the first paragraph of section 2.2).

Comment:

Since the authors are interested in the simulation of oxygen dynamics, we need to know at least what are the processes governing the dynamics of oxygen, do you have nitrification/denitrifiation in the water column, how the interaction with the sediment is represented? Is anoxic degradation an important pathway in the region (e.g. sulfate reduction), how is represented the air-sea exchange? The physics of the region should also be described in order to understand the transport of the biology. I have provided a detailed analysis of the manuscript.

Response:
As noted above, the objective is not to discuss dissolved oxygen here.

Comment:
General comment:
Please clarify how primary production is defined and phytoplankton growth rate.

Response:
*We have now included the mathematical form of the phytoplankton growth rate (μ) and primary production (μ*Phy).*

Comment:
Specific comments:
Page 125, lines 15-19: The authors mention: "We also investigate the question why primary production rates in the plume region are correlated with nitrogen concentrations and river nitrate loads, even though primary production is light-limited in this region and, hence, should not be sensitive to variations in nutrient concentrations and nutrient load." This sentence is confusing. This is not because light is limiting than nutrient can not be also limiting and influence phytoplankton growth

Response:
We clarified by inserting "nutrient concentrations never drop near limiting concentrations".

Comment:
Page 126, lines 10: It is very surprising that using a spatially uniform winds allows to represent the circulation in the river plume area. River plume areas are regions of intense dynamics and the resolution of the plume patching requires usually the representation of fine scale processes. I suggest that the authors investigate the use of spatially varying winds such as NCEP for instance. Page 126, line 18-23, the authors argue that the physical model has skill in predicting the salinity distributions and refer to Hetland 2010. However, this manuscript is not in the reference list.

Response:
We have run the model with spatially varying winds from the NCEP North American Regional Analysis (NARR) and did not find systematic difference in the simulated variables discussed here. We chose to present the results from the simulation with the spatially constant wind since it corresponds to the physical simulation discussed and validated in Hetland and DiMarco (submitted). We apologize for the missing reference; it is now included and the manuscript is freely available at http://testbed.sura.org/publications).

Comment:
Page 126: In the description of the biogeochemical model the authors forgot to mention that oxygen is a state variable of the mode (at least I expect since we have the impression that the model will be used to investigate hypoxia).

Response:
The aim of this manuscript is to discuss patterns of productivity and variability in phytoplankton. We don't present oxygen here (it is the subject of a forthcoming manuscript), hence don't provide its model equation.

Comment:
Page 127: The authors argue that the amount of nitrate lost through denitrification is estimated based on a relationship between oxygen consumption in the sediment and N2 production. How do you estimate these SCOC?

Response:
We included the following explanation in section 2.2:

> "Using the linear relationship between sediment oxygen consumption and denitrification, the stoichiometries for aerobic remineralization, denitrification and nitrification, and the assumptions that organic matter is remineralized instantaneously and that denitrification occurs through coupled nitrification-denitrification only, the fraction of remineralization that occurs through denitrification can be calculated. Essentially one assumes that sediment oxygen consumption results from aerobic remineralization and nitrification only."

Comment:
Page 127, line 23: We are told that the input of particulate nitrogen to the system is assumed to enter the pool of small detritus in the model? Why is it not partition between the fast and slow pool?

Response:
In the absence of information on whether and how particulate nitrogen should be partitioned between the small and large detritus pools we made this assumption. While the details of nitrogen processing vary between both pools, we don't believe that any of our conclusions are affected by this assumption.

Comment:
Page 127, line 26: What is "Kjeldahl"?

Response:
Kjeldahl nitrogen is the sum of organic nitrogen and ammonium.

Comment:
Equation 1, page 127, explain do the authors consider an extinction coefficient which depends on the salinity to represent attenuation of light in the river plume? Chlorophyll and detritus can be computed from the model. Chlorophyll is included in the formulation of k and to consider an

extinction coefficient varying as a function of salinity in addition to the extinction linked to chlorophyll may overestimate the extinction. Besides, from eq. 1 and the formulation given for Ksalt=max (-0.024+0.89*Sal, 0). we learn that 1) Ksalt is negative, which means that in eq. 1 you have an exponential that is increasing with depth (more light at depth), 2) Ksalt increases when salinity is increasing which is the contrary to what we can expect (we can expect that this formulation is used in order to consider that in the river plume we have an enhanced extinction due to the presence of suspended matter), 3) Ksalt will be zero almost everywhere except for salinity values lower than 0.027, which means at maybe one point corresponding to the boundary condition (I am pretty sure that the model cannot represent a more refined plume). Do the authors have a reference for the use of Ksalt?

Response:
The salinity dependent light attenuation coefficient is included to account for colored dissolved organic matter and suspended sediments in the river plume (we have added this rationale to the text); we don't believe this to be an overestimation.

The coefficient was derived from light attenuation measurements for the coastal ocean by Jay O'Reilly (personal communication). Ksalt is ensured never to be negative by the max(…,0) formulation.

*Unfortunately, there was a typo in the equation that is now corrected to Ksalt=max (-0.024*Sal+0.89, 0). Ksalt increases with decreasing salinity as it should and will be zero at 37 PSU.*

Comment:

Page 128: we are told that the model is run during 8 years starting from homogeneous horizontal profiles, what is the spin up time of the model? Usually, these types of models especially involving sediment dynamics may require a few years of integration (more than 5) in order to reach steady state. So you are not sure whether the interannual fluctuations that you observed in your model outputs are due either to the interannual fluctuations or to the adjustment of the model to the initial conditions. Besides, what is the impact of the open sea boundary conditions on model solution? Are the results sensitive to them? Do you consider the interannual variability of the atmospheric forcing (wind, heat fluxes) or is the forcing repeated each year?

Response:
Model spin-up time is short (a few weeks). Horizontal gradients in salinity and nitrate establish quickly after model initialization due to the freshwater and nitrogen inputs from the Atchafalaya and Mississippi Rivers. We included the following in sections 2.1:

> "We found a horizontally uniform initial condition to work well as most of the region is shallower than 50 m and completely homogenized during winter mixing. On the shelf, horizontal salinity gradients establish quickly (within a few weeks) after model start due to freshwater input from the Atchafalaya and Mississippi Rivers."

and in section 2.2

> "As with temperature and salinity, we found a horizontally uniform nitrate profile to work well for initialization as model spin-up time is short (a few weeks) and horizontal nitrate gradients establish quickly due to nitrogen inputs from the Mississippi and Atchafalaya Rivers."

There is no delay in reaching steady state due to sediment dynamics—the model assumes instant remineralization.

The open boundaries affect the model solution and are chosen to be sensible (climatological values based on observed data as specified in section 2.2). Given that the overwhelmingly dominant driver of this system is the river forcing, we feel that the details of the concentrations specified on the open boundary are of minor importance.

The realistic wind forcing (from measurement buoys in the Gulf as stated in section 2.1) varies from year to year.

Comment:

Page 128, line 7: The authors say that the oxygen and nitrate initial concentrations come from in-situ data. What are these data? Are they different from Sylvan et al., 2006?

Response:
We included: "from the World Ocean Database (Boyer et al. 2006)"
Sylvan's data are only surface and not for winter.

Comment:

Figure 2 showing the year to year variations of the loads of nitrate and PON discharged from the Mississippi and Atchafalaya Rivers: how is computed the seasonal cycle? Is it a typical cycle that is modulated by the annual mean value?

Response:
We modified the Figure to show annual mean discharge and nitrate inputs for the new, longer simulation period (new Fig 3). The new top panel shows the climatological daily nitrate inputs as well as the actual inputs for 1993 and 2000 (the highest and lowest discharge years). As stated in the text (section 2.2) monthly nutrient inputs are taken from the USGS estimates of Aulenbach et al. (2007).

Comment:

Figure 3: From Figure 3, the assessment of model performances is very difficult. I suggest computing a more quantitative indicator like the RMS or correlation coefficient. Besides, for some months (e.g. September), the agreement is very bad. The authors justify the poor agreement by the fact that they are using data from different years than the years of simulations. The symbols used for the different years 2001, 2002 and 2004 are very similar and it is difficult to differentiate these 3 years. I would add the dilution lines. It is not clear why the years 1993 and 1994 have been chosen for the comparison. The authors argue that they choose 1993 and 1994 because these two years represented very particular year of river discharges. If these years are exceptional year, is it really appropriate to compare them with other more "classic" years? Finally, it is surprising to run the model for periods for which you do not have data for the validation. Why the period 1990-1998 has been chosen and not 200-2008?

Response:
We completely revised the former Fig. 3 (now Fig. 5). Since we have now run the model up to

2004 we can compare corresponding years. As the years for which we have surface data from Sylvan (2001, 2003 and 2004) are similar in terms of discharge and nitrogen input, we have decided to only show one year and chose the year with the most data available (2001). Since this figure is meant to illustrate the patterns of nitrogen distribution and not particularly suited for a quantitative comparison, we added another figure with a more quantitative comparison of observed and simulated nitrate (the new Fig. 6).

Comment:

Figure 4: what is the meaning of the color scale on the right going from 0 to 100? The comparison is done by plotting in the log-log. However, it is well known that this type of comparison enhance the correlation. This is not because you have a good correlation in the log space that the correlation will be good as well between the variables.

Response:

An explanation of the color scale is now given in the figure caption:

> "Color indicates the number of simulated and observed chlorophyll pairs per bin (see color scale at the bottom right)."

We agree with the reviewer that correlation in log space is different from correlation in linear space, however, we give model-data comparisons for chlorophyll in linear space in the following figures including a quantitative measure of the agreement (see especially the new Figures 8 and 9). Surface chlorophyll is known to be log-normally distributed which is why it is usually plotted on a log scale (for example, all surface chlorophyll plots produced by NASA Goddard are on a log scale). This is why we chose to produce this Figure. The figure is also consistent with Fig. 1 in Fennel et al. (2008) and thus represents a useful benchmark for comparison.

Comment:

Figure 5 and text pages 130-131 : the authors explain the bad performances of the model in the "intermediate" region for the period 1990-1994 in comparison to the climatological average SeasWiFS cycle by the higher nitrogen load during 1990-1995 compared to the SeaWifs period.? However, from Figure 3, it seems that the high nitrate concentrations are consumed near the delta. It would be nice to have maps showing the spreading of the nitrate discharge by the rivers (horizontal maps) in order to assess the spatial extension of the plume and the area that is influenced by a higher load of nutrients. The authors do not comment the poor agreement in the "far field" region. Once again, this is a pity that the authors have not run the model for the same years of SeaWifs and nutrients availability because the discrepancies of the model with the data is always justified by the different periods of simulations.

Response:

We have now extended the simulation period to include the period form 1998 to 2004 and included a comparison for that period (see new Figures 8 and 9). The new Fig. 8 shows that the model-data agreement is consistently improved in all three regions if the same period is compared. We believe this is strong support for our initial explanation that in years with larger discharge (i.e. in the early 1990ies) chlorophyll is elevated compared to the late 1990is and early 2000s. The coefficient-of-determination or R^2 for this period is 64% (see Fig. 9) which is encouraging especially considering that no systematic parameter tuning was performed.

Comment:
Page 131: lines 13: How do they authors compute primary production in a nitrogen based model? What do they mean by primary production? Nutrient uptake? Once again the comparison is not optimal because it requires converting the nitrogen uptake in a photosynthesis rate and we know that this is not a good estimation. Figure 6 shows this comparison and once again the analysis of the correspondence between the model and data is very poor, we are only told that "The simulated rates agree well with the observations in terms of magnitude and temporal patterns, especially in 1990 and 1992. ". However, looking at this figure, there are also disagreements (e.g. in 91) and these are not commented/discussed.

Response:
Primary Production in N-based models is commonly calculated by assuming a fixed N-to-C ratio (going back to Fasham et al. (1990) and before and ever since). We included the following sentence to make this explicit and clarify:

> "The simulated rates were calculated assuming a constant C-to-N ratio of 106:16 (as is commonly done in nitrogen-based models)."

With respect to the disagreement in early 1991 that was pointed out by the reviewer we added the following explanation:

> "The small observed primary production value for the delta region in early 1991 seems unrealistically low (it is much smaller than primary production in the intermediate region) and is likely not an adequate representation of the mean primary production for the delta region."

Comment:
Page 131, paragraph 3.3: The authors analyzed the annual cycle of the simulated nitrate, phytoplankton and zooplankton content of the mixed layer. This cycle is explained by variability of the export, river inputs.. I would suggest that the authors describe 1) the typical seasonal cycle of the river discharge(peak) and typical interannual variability, 2) the seasonal variability of the circulation (when do you have export, when is influenced the intermediate and far field region by the river discharges? Where are transported these discharged throughout the year, what about the vertical stratification: : :..) showing selected maps of currents because as it is now we have to completely rely on what they say. How is represented the vertical export, I also suggest to revise model description giving at least a brief description of the flows that are represented (we are told that reminaralization and export are important but we have no idea about their representation)

Response:
In section 3.3 we merely describe the annual cycles shown in Fig. 12 (there is no discussion of export, river nutrients or circulation in this section). The typical seasonal cycle of river discharge, its typical interannual variability (i.e. the standard deviation) and the highest and lowest discharge years are given in the new Figure 3. We also included the new Fig. 11 showing the climatological mixed layer depth. As suggested, we expanded the model description section (2.2) by including a brief description of the flows represented in the model (the appropriate references to the model details are included as well).

Comment:

Page 132, lines 1-13: Why do you have constant phytoplankton biomasses of 1 mmolN/m3 (not low) although you have limiting nutrient concentrations? Why do you have higher zooplankton biomasses than phytoplankton in this region? Why do you have similar zoo seasonal cycle in the delta and intermediate region although you have different phytoplankton biomasses?

Response:
Although we did not pose these questions in the same words as the reviewer, they are addressed in the following sections (starting with section 3.4). For example, net phytoplankton transport occurs into the intermediate and far-field regions, but out of the delta region. Limiting concentrations don't imply there is no growth, simply that it is not occurring at its highest possible rate. Zooplankton biomass can be higher than phytoplankton biomass if there is a steady supply of phytoplankton (in steady state situations the rates matter more than the standing stocks), and zooplankton standing stocks don't have to be proportional to phytoplankton standing stocks.

Comment:
Page 132, line 16: symbols are given for describing the phytoplankton growth rate but their signification is not specified

Response:
We included the detailed parameterizations for the phytoplankton growth rate and limitation terms in the expanded model description section.

Comment:
Figure 8: I do not understand how the nutrient limitation function can be 0.6 with no nitrate in the mixed layer as shown in Figure 7a. Lines 15-27, the authors compared their "growth rates" with observations. However, what they have in the model is more nitrate uptake rates and besides this is conditioned by the parameters that you choose for the models. Of course, they choose values for _max in agreement with typical values of the literature and this is not surprising that it is similar to observations. This is not a proof of agreement of the model with observations this is just a proof that the parameters used in the model are in the good orange of observations.

Response:
Nutrient concentrations in the far-field are small but not zero. A nutrient limitation of 0.6 can easily arise, for example, given a half-saturation concentrations of 0.5 mmol N /m3 (see section 2.2) and a nutrient concentration of 0.75 mmol N /m3 (see new Fig. 12) the resulting Michaelis-Menten limitation term is N/(kN + N) = 0.75/1.25 = 0.6.

We are comparing rates because it is often pointed out that model-data agreement of standing stocks is not enough (for example, in a steady state situation the same standing stocks could result from different combinations of rates).

Comment:
Page 133, paragraph 3.5: I do not understand what the authors are doing. They have put a mortality term for the phytoplankton which is I assume proportional to the biomass*mortality rate. Then, they analyze the seasonal and spatial variability of this flux which is of course similar with phytoplankton seasonal and spatial variability! Besides, the authors do not mention how they

convert the nitrogen biomass in carbon. The value of this flux is of course very sensitive to this ratio as well as the comparison with observations.

Response:
We expanded the model description section by explicitly including the parameterizations for the mortality and aggregation processes and give the formulas for the corresponding fluxes discussed here. We used the Redfield ratio to convert between N and C units, which is standard. This is now stated explicitly in the caption of the new figure 14.

Comment:
Page 134, line 1: the author refer to a mean aggregation rates, once again it is not possible to understand what they mean with the very reduced description of the model that is provided. They compare an aggregation rate with observation but once again the good agreement is just the proof that the parameters are in the range of the literature but not that the model give a good export. Since the main aim of this study is to represent the occurrence of hypoxic event on the bottom, the authors need to validate the export flux they have in the model with data. This is not done and so the conclusions reached can be questionable.

Response:
We refer to the previous two responses and would like to emphasize that the main objective of this study is to discuss phytoplankton patterns and variability not hypoxia in bottom waters. Of course phytoplankton growth fuels hypoxia and the simulation of hypoxic conditions is one of our goals, but it is not the subject of this manuscript.

Comment:
Page 134: Conclusions: I do not agree with the conclusion of the authors that the model I abale to reproduce the spatial and temporal evolution of phytoplankton, zooplankton, nitrate, and also rates. See my previous comments.

Response:
Given the additional evidence we produced for the revision (new Figures 5 to 9) we hope the reviewer is now agreeable to our statement.

Comment:
Fig. 9: I do not understand why the flows are always given in carbon units although the model is nitrogen based. This requires a conversion and the use of a NC ratio which is not obvious especially for phytoplankton in which the CN ratio may vary from a factor of 3*4 during the course of the bloom.

Response:
We inserted a sentence in the figure caption stating that we are using the Redfield ratio for conversion. Using a constant C-to-N ratio is necessary and standard for N-based models.

Comment:
Fig. 10: How is the growth rate defined ? From figure 10, (it seems that it is different from the growth rate introduce page 132, line 16 since it can be negative. It seems that it is a specific

growth rate since it is expressed /day) . How is primary production defined?

Response:
The growth rate is explicitly defined in the model description and can never be negative, however, anomalies of the growth rate can be negative (whenever the growth rate is smaller than the climatological mean). All negative y-axis values in this figure (now Fig. 15) are anomalies (deviations from the mean).

Comment:
Page 134, lines 25-27 and after: If I understood correctly, the authors are plotting the nutrients uptake and they call it primary production. They say that "it is surprising that there is a correlation with nitrate load", this is not surprising since this correlation appears from June to September, when the limitation function is not 1. This is strange to look after a correlation between a nitrate uptake rate and the nitrate concentration, because it is clear that from the equation used in the model you will have an influence of the nitrate load on the nitrate uptake rate when the nitrate concentration is comparable to the half saturation constant used in the model. This influence is in the equation used in the model.

Response:
The point is that nutrient concentrations are always well above the half-saturation constant and thus aren't limiting. As we stated on line 27 and show in some detail in section 4.1, phytoplankton growth is not limited by nitrate in the delta region, thus one would not expect a correlation between the two. In fact, the model shows that there is no positive correlation between growth rates and N load (or nitrate concentration).