**Responses to Anonymous Referee #2**

Below the review is reproduced in black font and our responses interspersed in blue.

**Comments:**

This publication presents a numerical model study of the hypoxia events off the Changjiang Estuary.

The combination of the different modeling components is a priori convincing and appropriate: 3D oceanic circulation model, biogeochemical model, sedimentary oxygen consumption module, river discharge (nutrient and freshwater load), and atmospheric forcing from reanalysis.

My main concern is about the model validation or skill evaluation before any use.

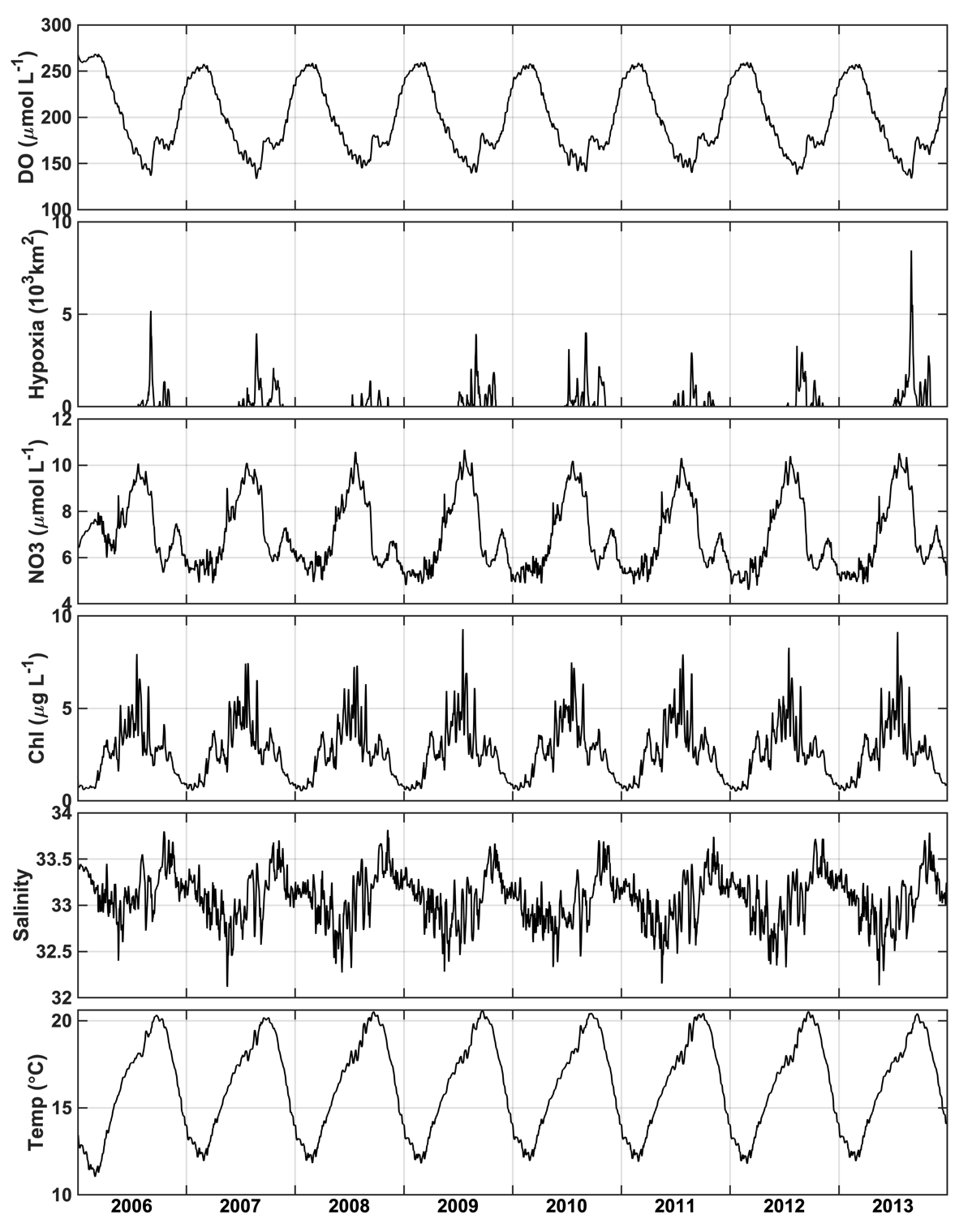
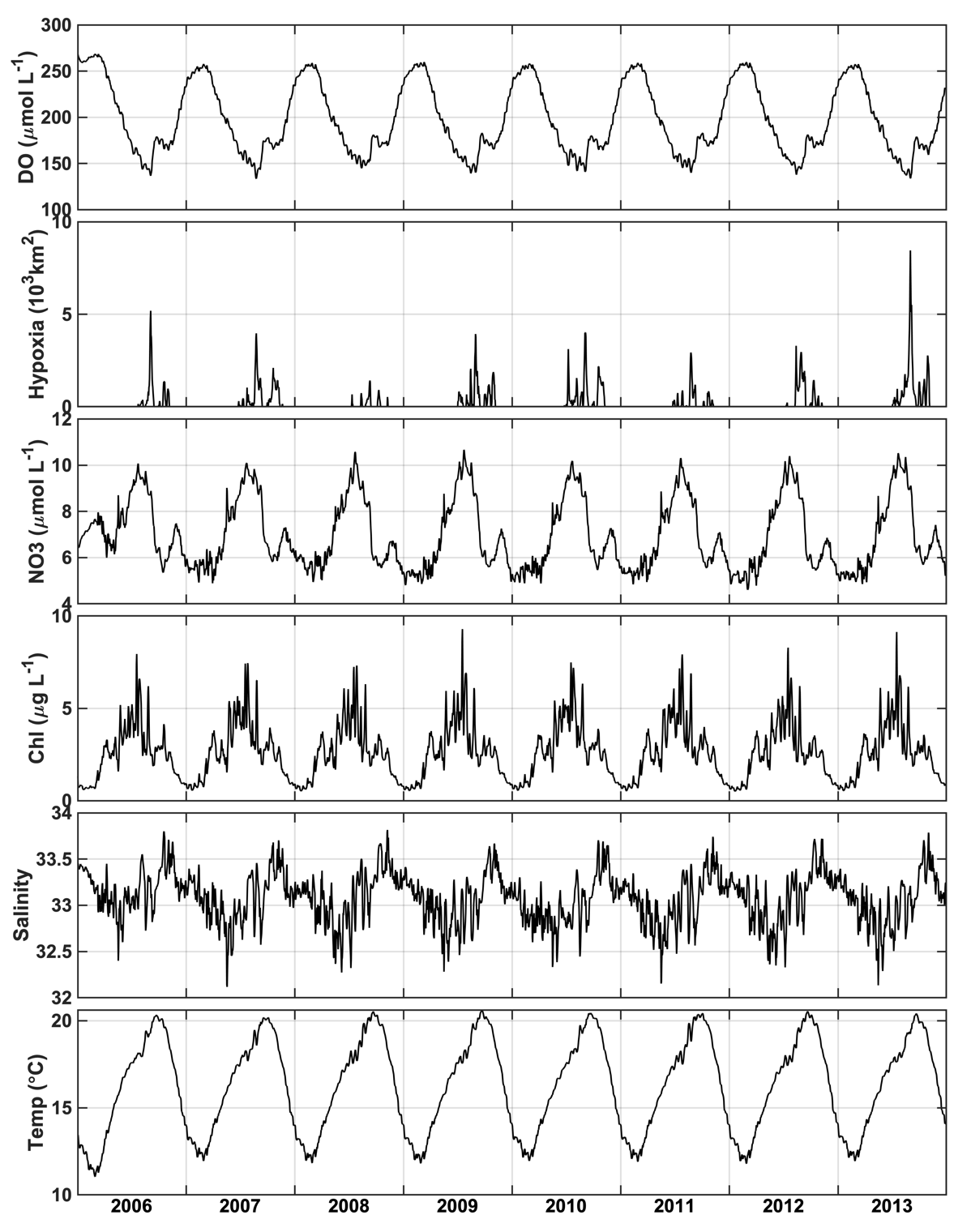
**Reply:** We appreciate this overall positive assessment and believe we have addressed the Reviewer’s concerns regarding model validation as described in more detail below.

The model-data comparison is presented in section 3.1, Figure 2 only, and some other in the Supplement. The display of Figure 2 is problematic: color points (data) having the same color (same value) as the background (the model) do not appear. It is really difficult to see the observational structure and to evaluate the agreement with the model. (same for Figures S2, S3, S4). It could be separated figures (data distribution and model). Figure S6, including the bottom line, is much more speaking.

**Reply:** We have added a dedicated model-data comparison section (2.1) and a comparison for nitrate to illustrate that the model reproduces nutrient distributions well.Also, we agree with the Reviewer that the data points were blending in the background and have replotted the comparison figures for the in-situ observation comparisons in better quality and in colourblind-friendly scales. We are happy that the Reviewer finds the 2D histograms in Figure S6 (see also Figure S1) informative. These graphs only make sense when a large number of data points is available (usually only the case for satellite data). For the comparisons with in-situ observations we included correlation coefficients.

The authors aim at reproducing the observations from 9 cruises from march 2011 to september 2013. Therefore, the simulation starts in 2006, uses climatological observations from this period to force the model, 2006-2007 are used as spin-up, and the model is run in 2008-2013 for analysis. Regional models may be very dependant on the boundary conditions. Nothing is demonstrated about the robustness of the inner region : is there any drift in the total budgets (nutrients, oxygen, intensity of the Primary Productivity) ? The model is set and used. I would be more confident with the results if any sensitivity test would be performed. By example, it would be possible to run the model for the same duration (8 years) but with repeating the same annual forcing (e.g. 2006), in order to control that the inner structure of the PP and hypoxia are repeated or if any trend exist. It would also evaluate the model-internal-variability, not to be confused with the variability induced by the varying forcing (winds, river discharge).

**Reply:** We are confident that there is no drift in the domain. Following the Reviewer’s suggestion, we have conducted an 8-year climatological simulation where the 2006 forcing was repeated year after year. Shown below is the mean bottom oxygen concentration and surface chlorophyll, nitrate, temperature and salinity in the region affected by hypoxia. We hope it is obvious that there is no drift and that the system is in dynamic steady state. Also, we appreciate the Reviewer’s suggestion to contrast the original simulation with realistic forcing with the climatological simulation that repeats the same forcing every year; however, given the plot below, which shows that the results are essentially identical from year to year (except for some random fluctuations) we believe this is not necessary or instructive.



The model is used in its "optimal" configuration, but the evaluation to reach to this configuration is not presented. The model here is not used to make any sensitivity experiment. Part of this is explaned late in the paper (line 384, just before the conclusion): there is a companion paper (Grosse et al.) that presents modeling experiments to quantify the relative importance of the processes responsible for hypoxia. This is important since the authors just infer the importance of processes (lines 325-334), without proceeding to the sensitivity test to their hypotheses. In this case, I would indeed recommand to proceed to a simulation while removing the nutrient load (which seems to be done in the companion paper). It should be presented from the beginning that part of the modeling analysis is done somewhere else.

**Reply:** We now refer to the companion paper by Grosse et al. also in the Introduction. We are not presenting nutrient load reduction experiments in this manuscript. Some are presented in the companion paper by Grosse et al. A more extensive analysis of nutrient load reduction experiments is the subject of a forthcoming manuscript led by Arnaud Laurent.

Concerning the main conclusions of the publication, the analysis of the main contributors to hypoxia, in the whole water column and in the bottom layers, is relevant. It is important to be able to evaluate the relative importance of Water Respiration versus Sedimentary oxygen consumption. But once again, data are missing, or at least a more rigourous model-data evaluation. As an example: Figure 3 focuses on the patterns of the hypoxia events from 2008 to 2013, and different behaviors or chronology could be distinguished (that is very interesting in itself, and the modeling tool is really appropriate for this kind of studies). Unfortunately, it is unsufficiently documented, how does this relate to observations ? Same for the discussion about the influence of wind events (4 typhoons) on the hypoxia extent.

**Reply:** We appreciate the Reviewer’s assessment that one of our main conclusions about the contributions of water column versus sediment respiration is relevant and important. We also acknowledge that a rigorous model-data comparison is desirable but not the year-to-year comparisons are hampered, to some degree, by the relatively limited availability of observational data. Observed rates of SOC are reported in the discussion. Also, we recently became aware of a nutrient data set for the region and have added the resulting comparisons to the new model validation section. Furthermore, we present model-data comparisons of satellite-derived SST and Chlorophyll, and model comparisons against in-situ data of temperature, salinity and oxygen. We believe that these comparisons provide the currently best attainable level of confidence in the model’s ability for us to present model results. However, we fully agree that more would be much better. If the Reviewer is aware of any additional in-situ data that are available, we’d appreciate hearing about these and would happily include them.

With regard to the interannual variations shown in Figure 3: We have substantially expanded the analysis of these differences in the revised manuscript and sincerely hope the Reviewer finds the useful.

I would recommand to improve the model-data evaluation in order to convince that the modeling of hypoxia events are (1) not biased by model-depending behaviors (2) close to observations.

**Reply:** We believe we have satisfactorily addressed both of these points. See responses to the comments about model drift and validation above.

Specific comments:

The model includes a light-attenuation term dependent on water depth and salinity (lines 177-181). Could you confirm that places where the light attenuation is applied ( f(z,S) ) are indeed places where particles (RDOM, Detritus , phytoplankton, . . .) are present and induce this shadowing effect ? Some other parameterisations exist that compute the shading directly in situ from the biogeochemical species. Using depth and salinity has the convinience to put this effect where it has been observed, but has the inconvenience to decouple the modeled biogeochemistry from its shading effect.

**Reply:** Thank you for raising this point. We have clarified that light is attenuated everywhere in the model domain by seawater constituents (specifically chlorophyll and detritus) and seawater itself. Inaddition to this, light is also attenuated by suspended sediment according to the parametrization referred to above. Observations show relatively higher suspended sediment concentrations, and thus light attenuation, in shallow areas (Bian et al., 2013; Chen et al., 2014). To account for this additional contribution to light attenuation by suspended sediment, which are not explicitly modeled, a simple parametrization depending on bathymetric depth and salinity is implemented. We is now clarified.

Minor comments

line 60. ref. Fennel & Testa : missing comma

line 187. "based on"

Figure 3 : labels a, b and c are missing on the figure itself.

**Reply:** We addressed these comments. Thank you for pointing them out.