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Interactive comment on "A numerical model study of the main factors contributing to hypoxia and its sub-seasonal to interannual variability off the Changjiang Estuary" *by* Haiyan Zhang et al.

Anonymous Referee #2

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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.

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.

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).

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.

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 exampleÂă: 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.

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.

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.

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.

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