

## ***Interactive comment on “A numerical analysis of biogeochemical controls with physical modulation on hypoxia during summer in the Pearl River Estuary” by Bin Wang et al.***

**Anonymous Referee #2**

Received and published: 27 January 2017

This study by Wang et al. present a numerical investigation of the hypoxia condition in one of the largest river estuary along the Western Pacific - the Pearl River Delta. According to the author, the Pearl River has a similar nutrient load as that of the Mississippi and the Pearl river runs through a highly populated region. The importance of this study is beyond question. And the coupling between 1D river model and 3D coastal model as well as the author's efforts of improving the ECOM model is plausible. However, I found this manuscript is not well written and the author failed to explain his/her points throughout the manuscript. The so-called “physical modulation method” is merely to isolate lateral DO from in-situ biological source and sinks. In addition, throughout the manuscript the definition of ambient biogeochemical process is very

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vague and sometimes contradictive. The available observation data does not support the high frequency zone (HFZ) proposed by the author. I do agree with the other reviewer that this manuscript needs a throughout edits of scientific writing. I have a lot of problems to understand what the author wants to convey. My detailed comments are as following.

First of all, the abstract contradicts with itself. The author first indicated DO is affected by both local biological processes and DO fluxes from the ambient water (I would treat this as boundary condition). And the author claims the later (ambient water) is unclear. Then in next paragraph, the author indicated that the re-aeration (air-sea, i.e. surface) and sediment oxygen demand (bottom) are most important.

Again, the author indicates the importance of the re-aeration, followed by a statement saying “turning off the re-aeration leads to hypoxia. ... to the west lower estuary”. However, the author immediately states that hypoxia was observed in west lower estuary. So my question is re-aeration good or bad? Important or not important at all?

Then, in the last part of the abstract, the author states photosynthesis is less important. Yet right following this, the author indicates that in HFZ, photosynthesis cause an increase of hypoxia area to 591 km<sup>2</sup>— it confusing me how photosynthesis would worsen hypoxia (should be respiration, right? But respiration is not directly linked with photosynthesis).

Page 2, line 29-34. The author first indicate DO from ambient water body, followed by “Take the re-aeration as an example”. How would these two processes be connected (one is lateral boundary and the other is upper boundary)? The author then mentioned, “given the mechanism remains unclear”, which one mechanism? Why it is not clear? Also, I do not think DO flux from ambient water bodies belong to the biogeochemical processes only, it indeed a combination of both bio and physical.

Page 3, line 14-15, the author compared PRE with Northern Gulf of Mexico. I think one of the core mechanisms here is the tidal range difference (M2 in PRE vs S1 in NGOM).

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Page 3, line 17. I do not agree that the mechanism remains clear, since just two lines above the author indicated that previous studies suggest sediment oxygen demand and stratification are two main reasons. The author should give a more persuasive justification for this study—I believe there are many indeed.

Page 6. Line 3, what is RDOC, LDOC, ReDOC, ExDOC stands for?

Page 6. Line 15. The authors used a constant boundary condition to study the impact from ambient DO transport—which worries me a lot!

Page 7 Line 7. Should not re-aeration of DO a physical process instead of a biological one?

Page 7. I am confused why the author needs to give repeated information in equation (6)-(12). They simply divide DO into  $DO_{bc}$  and  $DO_{bio}$ , and they can just use equation (13), that is enough.

Page 8. Model validation, I think it is necessary to present more validation for both physics and water quality, specifically some time series comparison. The Taylor diagram could represent the correlation yet we are not sure how the model could resolve temporal variability.

Page 9. Section 3.2. The author presents the difference between RCA and the Physical Modulation method. How about the difference between Physical modulation and the in-situ data? Is there any improvement compared with the difference between RCA and in-situ data?

Page 9. Line 30. “The agreement indicates . . .” which agreement?

Page 10. Line 16. The HFZ refers to high frequency zone of areas with  $DO < 3$  mg/L. But why the author states “indicating HFZ is most possible to form hypoxia”?

Page 11. Line 6. What does the author mean by “it is encompassed by the isoline of 10%”?

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Page 11. Line 7-16. Should not these abbreviations go to the figure caption?

Page 13. Line 4. “Although . . . However . . .” very awkward wording

Page 13, Line 10. “Both of which contribute 79% and 25% . . .” I do not understand what the author wants to convey here.

Page 13. Line 19-20. From Figure 8e I could tell how horizontal and vertical advection balance each other.

Page 14. Line 10-11. The author mentioned “two distinct areas characterized by the high level of sediment oxygen demands”. However I could not find it on figure 10a referred here (I only see one area delineated by the white line).

Page 14. Line 21-22. I do not understand what does the author mean here “depth to water column respiration and photosynthesis”. Or the author means depth of water column respiration/photosynthesis. Also why this estimation is underestimated?

Page 14. Line 27-28. Why such decrease cannot be explained only by biogeochemical processes?

Page 14. Line 34-36. I could not tell how “simulated DO distribution in the bottom is in reasonable agreement with DO distribution”. If we look at the available DO observations in Fig. 4. There are only 2 points in Figure 4(c) that show low oxygen in the so-called HFZ zone.

Page 15. Line 2-3. I could not tell that the lowest  $DO_{sed}$  is observed in the HFZ zone from Fig. 4. Although the author mentioned Fig. 11 here, but it is model result rather than in-situ observation.

Page 15. Line 25. Figure 9921a. I do not know what the author wants to convey here.

Page 16. Why “phosphorous limitation can not be convincing enough?” The author does not mention much about phosphorous budget before this.

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Page 16. Line 20-21. Again, I am not convinced that Fig.10c (model) shows the realistic condition (Fig. 4).

Page 16. Line 28. "is comparable more important". Awkward wording.

Page 17. Line 17. The author used a constant boundary condition, how can he/she justify that "DO originating from the boundaries to the bottom of the PRE and HFZ"?

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Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-454, 2016.