**Anonymous Referee #2**

We thank the reviewer for the constructive comments and suggestions that are very helpful to the revision of our manuscript.

Detailed response to all comments are given below (responses are shown in blue)

**Summary:**

In this manuscript the authors use a physical-biogeochemical model to examine a hypoxic event in the Pearl River Estuary (PRE) in July and August 2006. They conduct several numerical experiments in order to determine the relative impact of riverine inputs of oxygen, nutrients and organic matter on hypoxia in the PRE. They specifically examine three processes that affect oxygen dynamics: re-aeration due to air-sea oxygen flux, sediment oxygen demand, and all remaining processes which together is referred to as WCP (water column production). This is a well-written manuscript with some very interesting results, but some clarifications, some more discussion, and a few additional experiments should be performed before publication. The comments below are lengthy, but if addressed fully the resulting paper would be a very valuable contribution to Biogeosciences.

**Major comments:**

1. As I understand it, all results shown in the manuscript are for July and August 2006. This should be made clearer in the abstract, which is written more like this is the "general" case for the PRE. I understand that the model has only been evaluated for July and August 2006, so we don't really know whether the oxygen concentrations at other times of the year are correct or not; however, as a reader I was very interested to see results for the whole summer (May to September), or even for the whole year, rather than just for two months of one year. How does the temporal variability of hypoxia change in the numerical experiments? This analysis does not seem complete without this addition.

Response:

We will make it clearer in the revised manuscript that the results are based on July and August 2006 only. As suggested, we will show results of hypoxia for the whole summer (May to September) in our revised manuscript. Plus, we will also provide more justifications on why we focused on July-August only and add discussions on how representative or comparable is the hypoxia in this time of the year to that of other months or years.

In terms of validations, in this manuscript we have only presented the model-data comparison results in July and August 2006. However, the coupled physical-biogeochemical model has been thoroughly validated against not only physical variables (i.e. water levels, salinity, and temperature), DO concentrations, but also the historical observations of some important biological variables (e.g. chlorophyll and particulate organic carbon) and processes (i.e. re-aeration, sediment oxygen demand, and primary productivity) (see Wang et al., 2017). Our model is able to reproduce the observed hypoxia near the Modaomen sub-estuary and the main processes associated with DO. Previous studies have also reported the hypoxia near the Modaomen sub-estuary with the similar spatial extents and characteristics as the model simulated (Cai et al., 2013; Lin et al., 2001; Zhang and Li, 2010).

With regard to the temporal variability of model simulated hypoxia, Figure r1 shows that the simulated hypoxia in 2006 starts to develop in April, peaks in August, and disappears in October. However, oxygen observations are only available in July and August 2006 when hypoxia are observed, while for other months no observations are available for validating the model simulated hypoxia. This is one of the main reasons why in the manuscript we only focused on July and August in 2006 to study the impacts of riverine inputs on hypoxia and oxygen dynamics in the Pearl River Estuary. Additionally, previous reported hypoxia also mainly occurred in July and August (Cai et al., 2013). Plus, from the aspect of the river discharges from the Pearl River network, these two months are typical wet seasons with the monthly-averaged total river discharges over 20,000 $m^3$ $s^{-1}$ and the year 2006 is a wet year with the annual averaged total river discharges exceeding10,000 $m^3$ $s^{-1}$ (Figure r2).

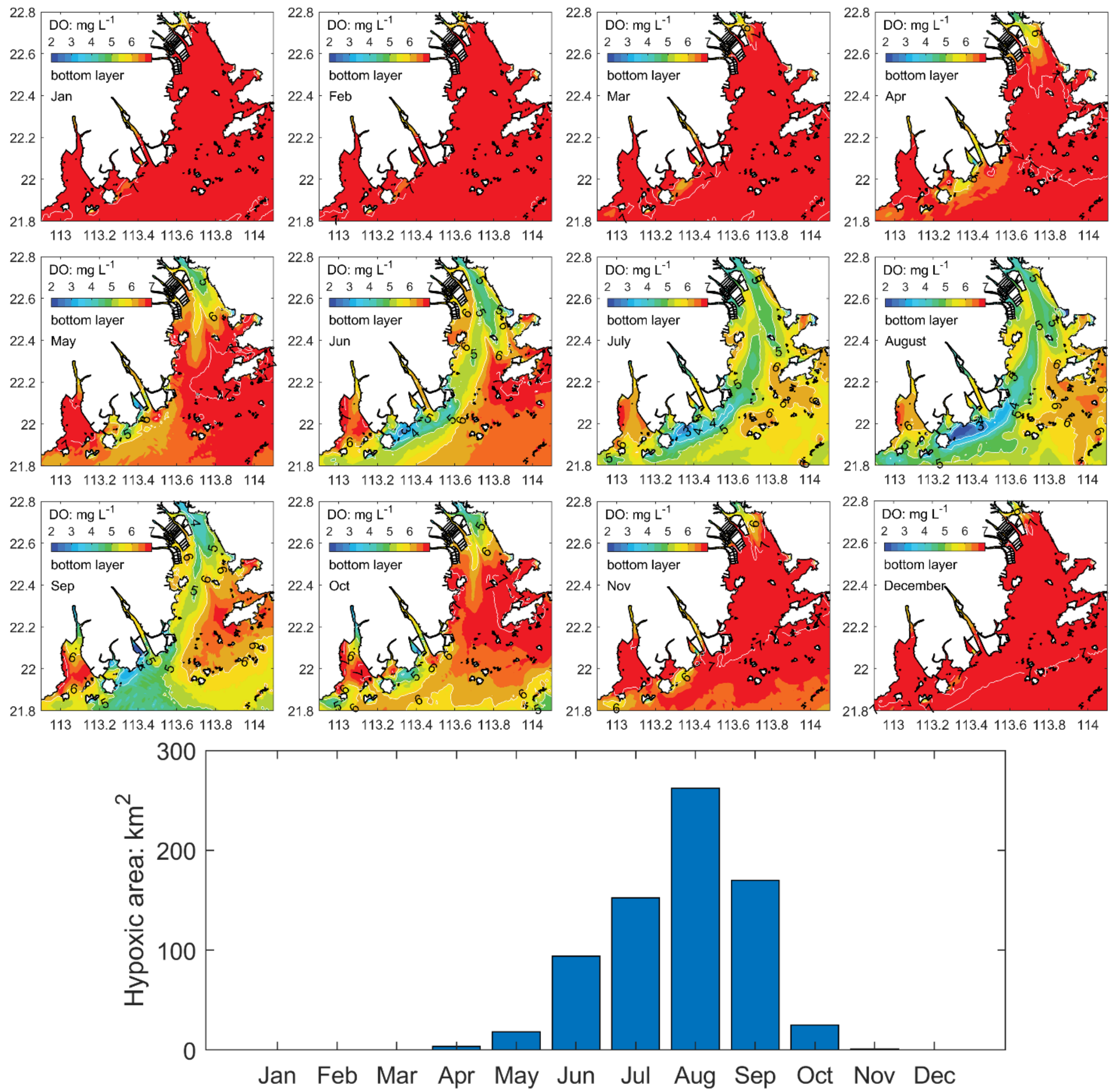


Figure 1 Distributions of the monthly averaged bottom DO and annual cycle of the hypoxic area in the Pearl River Estuary

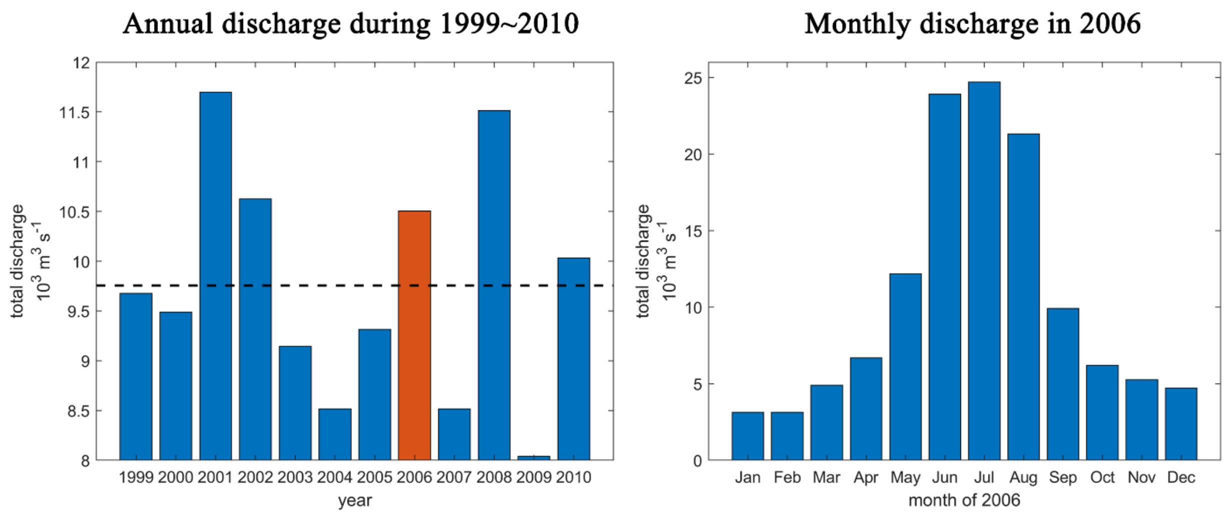


Figure r2. Inter-annual total river discharges of the Pearl River Network during 1999~ 2010 (left panel) and the annual cycle of monthly-averaged total river discharges in 2006 (right panel)

2. As a reader, I was also wondering whether July and August 2006 was a typical year. Was 2006 a particularly dry July/August? Or wet time period? Are the results of the sensitivity experiments conducted here likely to hold in other years?

Response:

As shown in Figure r2, the year 2006 is a wet year with the total river discharge over 10,000 $m^3 s^{-1}$ and the monthly river discharges during July-August over 20,000 $m^3 s^{-1}$. More justifications on why choosing July and August 2006 please see our response to the comment 1.

We think that our conclusions drawn from year 2006 would be applicable to other years because previous studies on hypoxia in other years have also reported some similar features as we observed in year 2006. These include the strong re-aeration (Zhang and Li, 2010), the dominance of the sediment oxygen demand (Yin et al., 2004; Zhang and Li, 2010), and the important contributions of the terrestrial particulate organic carbon (will be renamed as the allochthonous POC as suggested) (Hu et al., 2006; Yu et al., 2010).

We agree that it would be an interesting topic to study the annual cycle and multi-years variations of hypoxia in the Pearl River Estuary. However, it will be quite difficult to study the annual cycle and multi-years variations of hypoxia due to the insufficiency of observational data. And to our knowledge, there are currently few studies on these two topics. Nevertheless, we believe that our study can provide some scientific basis and guidance for further modelling or observational studies on the hypoxia in the Pearl River Estuary. We have also conducted a cruise observation in the Modaomen sub-estuary in January this year. The next cruise will be carried out in August this year.

As suggested, we will provide more discussions on how representative or comparable is the hypoxia in 2006 to that of other years in our revised manuscript.

3. One of the main results of this manuscript was that hypoxia in the PRE is not sensitive to nutrient concentrations of the river water entering the region (unlike the Chesapeake Bay and the Gulf of Mexico, for example). This result, however, has to be at least slightly dependent on what value is used for the nutrient concentrations in the eight rivers. What concentrations are used and are they realistic? Where do these concentrations come from? A terrestrial-biogeochemical or watershed

model? More detail is needed here. Also, it sounds as if only the nutrient concentrations were changed in the largest river, not all eight rivers. The authors need to show results of changing the concentrations in all eight rivers, not just the largest, since the smaller ones closest to the hypoxic zone might impact the hypoxia zone more than the large river, which is farther from the region of hypoxia. (The same is true for the oxygen and POC experiments.)

**Response**:

In the Cont case (will be renamed as Base case as suggested by reviewer), the river boundary conditions are prescribed based on the monthly observations in 2006 collected by the State Oceanic Administration (including DO, NH4, NO2+NO3, and PO4) and Liu et al. (2016) (including dissolved organic carbon, particulate organic carbon, dissolved organic nitrogen, particulate organic nitrogen, dissolved organic phosphorus).

The riverine inputs (including oxygen, nutrients, and particulate organic carbon) are changed in all of the eight outlets. We will include more details of the riverine inputs and better explain the numerical experiments setting clearer in the revised manuscript.

4. This analysis compares the impact of sediment oxygen demand, re-aeration and WCP on hypoxia. However, this is misleading since WCP is the sum of multiple positive and negative terms. Thus this term is likely smaller than its components. For a more complete analysis, the authors need to separate out the various components of WCP, including respiration, nitrification, water column remineralization etc. . . This is particularly important because in the discussion they state that in the PRE water column respiration/remineralization is not as important as it is in places such as the Chesapeake Bay. But we cannot see this (truly interesting!) result unless the authors isolate these terms.

**Response**:

We understand the reviewer's concerns about the application of water column production (WCP). In our previous study (Wang et al. 2017), we conducted the DO budget analysis and found that the magnitude of nitrification and oxidation are much smaller than the respiration. For the convenience of discussions, we used the water column respiration (WCR, the sum of respiration, nitrification, and remineralization/oxidation) to represent the gross rate of DO consumptions in the water column, a term that has been widely used in the field (Murrell and Lehrter, 2011) and modeling studies (Li et al., 2015; Yu et al., 2015a). According to our budget analysis in Wang et al. (2017), the sediment oxygen demand dominated the DO depletion both for the Pearl River Estuary and the high frequency zone (pls. see Figure 11a and 12a in Wang et al. 2017), which has been reported by previous studies (Yin et al., 2004; Zhang and Li, 2010) in other years. In the contrast, the hypoxia in the Chesapeake Bay is dominated by the water column respiration (Li et al., 2015). Differences in the relative importance of water column respiration vs. sediment oxygen demand in the two systems (the Pearl River Estuary and the Chesapeake Bay) has been widely accepted by other studies (Rabouille et al., 2008; Hong and Shen, 2013).

In this study, we used the water column production (WCP, the sum of water column respiration and photosynthesis), the re-aeration, and the sediment oxygen demand to represent the net effects of water column, the air-sea interface, and the water-sediment interface, respectively. According to our DO budget analysis in Wang et al. (2017), the photosynthesis and respiration were two major oxygen source or sink term in the water column. Considering that photosynthesis and respiration are both closely and directly correlated to phytoplankton growth, they have the similar distributions and responses to changes in riverine inputs. For example, increasing the nutrient loading will facilitate the growth of phytoplankton and hence both photosynthesis and respiration. Based on these reasons, we did not consider each component separately in our current manuscript

As suggested, we will include more detailed explanations of using the WCP in our revised manuscript. We will also include the equations for each component of WCP in the revised manuscript.

5. It is not completely clear why the "physical modulation" method is needed. If this is a fully coupled physical-biogeochemical model (as is stated), then why can't the authors simply save each of the oxygen flux terms in the oxygen budget? Presenting results in units of DO per unit time (as is done in Figure 7) would be much more helpful for the reader. The idea of different "species" of oxygen seems a bit convoluted. Clearly REA, WCP and SOD have units of oxygen per unit time (see equation 1). Showing figures of these quantities, rather than DO_REA, DO_WCP and DO_SOD would make the manuscript more clearly understandable to readers.

**Response**:

The physical modulation method was firstly introduced and implemented in our pervious study (Wang et al. 2017) to explain the underlying processes and mechanisms of hypoxia in the Pearl River Estuary. We also conducted DO budget analysis in our previous study and found the advantages of the physical modulation method in explaining the occurrence, the spatial extent, and the duration of hypoxia in the Pearl River Estuary. Comparing with the budget analysis, the physical modulation method can demonstrate the spatial connection of each oxygen source or sink process occurring in different locations (e.g. the influence of sediment oxygen demand on adjacent waters and the vertical penetration of re-aeration supplied oxygen).

In the current study, by using the physical modulation method, we found the buffering effects of re-aeration. That is, the re-aeration can respond to the perturbations of riverine inputs rapidly and hence moderate the DO changes caused by these perturbations. In addition, we further depicted the interactions between each source and sink processes quantitatively. For example, in the 'Cont' simulation, the sediment oxygen demand removed surface oxygen by 2.22 mg $L^{-1}$, which switched the re-aeration from the sink (-1.45 mg $L^{-1}$ $day^{-1}$) to the source (0.55 mg $L^{-1}$ $day^{-1}$) of DO in surface waters.

In our revised manuscript, we will provide more details of the physical modulation method and make the definition of its associated variables clearer.

6. I really like the idea that re-aerated surface waters can penetrate to the bottom water and offset the changes in DO caused, for example, by increased nutrient, DO, or OM riverine inputs. The authors discuss that this is not the case on the Gulf of Mexico shelf, where hypoxia occurs as a very thin layer near the bottom. The comparison and emphasis on the Gulf of Mexico seems a bit out of place, since the PRE seems to be more similar to the Chesapeake Bay in many ways. The discussion could be strengthened by making a three way comparison between the Chesapeake, Gulf of Mexico and the PRE. Isn't the re-aeration process described here similarly important in the Chesapeake Bay, where hypoxia occurs as a thick layer, which is not far from the surface in a typical July/August?

**Response**:

Firstly, we appreciate reviewer's encouraging comment on our discussion on the re-aeration. However, there seems some misunderstanding in the discussion about the case on the northern Gulf of Mexico (NGOM). In our manuscript, we conducted the comparison between the PRE and the NGOMR with a purpose to explain and understand the strong re-aeration in the PRE. As we discussed in P17 line 11-17, the strong re-aeration is a result of the high sediment oxygen demand and the shallow waters in the PRE. In the contrast, the re-aeration in the NGOM is overall an oxygen sink to surface waters in summer (Yu et al., 2015b) because of the weaker sediment oxygen demand and deeper waters (P17 line 17-23). However, we did not state that the oxygen supplied by surface re-aeration cannot penetrate to the bottom waters of the NGOM as there is no published studies investigating this mechanism on NGOM. Actually, we think that without applying the physical modulation method or similar, it is hard to estimate the effects of surface re-aeration on the bottom waters because it depends highly on the magnitude of re-aeration, the water depth, and the hydrodynamic conditions.

Secondly, we agree that it will be very interesting to discuss the role of re-aeration on hypoxia in other hypoxic systems (e.g. the NGOM and the Chesapeake Bay) and make the comparisons with the PRE. However, extended discussion is not feasible due to a lack of data and relevant studies in other hypoxic systems. Take the Chesapeake Bay as an example, we don't have re-aeration data to figure out the importance of re-aeration in the Chesapeake Bay. Nevertheless, according to our results, we can speculate that the re-aeration might be quite important in the Chesapeake Bay because the strong water column respiration can draw down the surface DO concentrations and enhance the re-aeration. However, more relevant studies are still required before the conclusions can be drawn.

As suggested, we will show more explanations for not including the Chesapeake Bay into discussions and our speculations of re-aeration in the Chesapeake Bay in our revised manuscript.

7. There is some considerable overlap with the authors' previous publication (Wang et al.,2017, BG). For instance, it appears to me that one of the main points in the abstract of the current manuscript: "Model results showed that hypoxia in the Pearl River Estuary was confined to the shelf off the Modaomen sub-estuary with a hypoxic area of 200km2 mainly due to the combined effect of re-aeration and sediment oxygen demand" was actually a primary result of this previous publication. This should be made clearer in the abstract and introduction. Clearly this study builds off the previous study. Although the previous study is mentioned in the abstract, the differences between the current study and the previous study should be made clearer to the readers.

**Response**:

As suggested, we will clarify the differences between the current and previous studies in the revised manuscript. We kept the sentence that the reviewer pointed out in the original manuscript in order to let readers know the basic information of hypoxia in the Pearl River Estuary before discussing effects of different riverine inputs on hypoxia.

**Minor Comments:**

8. Abstract last sentence – suggest changing this to: "This study highlights the importance of re-aeration in determining the hypoxic extent and in reducing hypoxia variability in shallow estuaries."

**Response**:

We will revise it as suggested.

9. Abstract – Define here (and in introduction) that by re-aeration you mean a flux of oxygen across the air-sea interface. (Currently this doesn't occur until page 6).

**Response**:

We will revise it as suggested

10. Introduction – Authors could mention climate change as another anthropogenic impact, since recent studies are showing that increasing temperatures have a large impact on increasing hypoxia.
**Response**:
We will include the effects of climate changes on the hypoxia in the revised manuscript.

11. P2, line 7: Why is there a ten-year lag? Does this occur in an estuary like the PRE? Or maybe it's not relevant here.
**Response**:
The ten-year lag between eutrophication and hypoxia is estimated on the global scale (Rabalais et al., 2010). There hasn't been study on the time lags between the eutrophication and hypoxia in the Pearl River Estuary. We will delete this sentence in our revised manuscript.

12. Page 4, line 11: This paragraph is talking about how nutrient inputs to the Pearl River Estuary can impact hypoxia, but this line is about particulate organic carbon, which could be moved to the following paragraph talking about organic matter. Page 4, line 16: What is the organic matter? Is it only POC mentioned in line 11? Or does it include PON (nitrogen) and dissolved organic matter? Which type of organic matter primarily contributes to hypoxia?
**Response**:
As suggested, we will move this sentence to the next paragraph describing particulate organic carbon in our revised manuscript. In P4 line 16, organic matter represents the particulate organic carbon only. We will clarify it in our revised manuscript.

13. P4, line 20: How are these models dynamically coupled? If these were dynamically coupled, the estuarine model would provide feedbacks to the riverine model. Is that the case? Also, the model set up seems to assume that there are no freshwater or nutrient sources (from the land) into Mirs Bay, Daya Bay or Honghai Bay. Is there evidence to support this assumption?
**Response:**
The 1-D river network model and the 3-D estuary model are dynamically coupled through the eight river outlets. These two models are run in parallel and their model quantities are exchanged across the coupling interface (eight outlets) during runtime. At each time step, the 3-D model utilizes the simulated discharge obtained from the 1-D model as the river boundary forcing, while the 3-D model sends the simulated water levels to the 1-D model as the downstream boundary forcing for the next time step. More detailed descriptions of the coupling can be seen in Hu and Li (2009) and we will include a bit more details in our revised manuscript.

The riverine input of freshwater and nutrient fluxes entering the Mirs Bay, Daya Bay, and Honghai Bay are much lower than those from the Pearl River Network. In addition, the Mirs Bay, Daya Bay, and Honghai Bay are quite far away from the hypoxic zone. In the wet season, the Pearl River Estuary is dominated by the southwesterly monsoon and the Pearl River plume mainly propagate eastward. Therefore, we neglected freshwater or nutrient sources entering the Mirs Bay, Daya Bay, and Honghai Bay, and also neglected contributions of these regions to the oxygen dynamics in hypoxic zone.

14. P5, "Water quality model" section: In this section the authors need to describe more clearly where their riverine biogeochemical concentrations are derived from, since these are at the very heart of their numerical experiments. Do concentrations of the 26 state variables all come from the riverine model described above? If so, more information regarding the details of the biogeochemistry of the riverine model is needed. Where do the outer boundary conditions come from, for the estuarine model? How about atmospheric deposition of nutrients, like nitrate and phosphate? Are all these assumed to be negligible? How realistic is this assumption?

**Response**:

The river boundary conditions of biogeochemical concentrations are derived from the monthly observations in 2006 collected by the State Oceanic Administration (including DO, NH4, NO2+NO3, and PO4) and the previous study (including different classes of dissolved organic carbon, particulate organic carbon, dissolved organic nitrogen, particulate organic nitrogen, dissolved organic phosphorus) (Liu et al., 2016). The open boundary conditions of biogeochemical concentrations are set according to Zhang and Li (2010). In this manuscript, we did not include the atmospheric deposition of nutrients because the riverine nutrient input is the dominant nutrient source in the Pearl River Estuary.

15. P6, line 1: Since one of the conclusions of the manuscript is the relative importance of SOD compared to WCP (see abstract), here the terms making up "WCP" need to be written out explicitly.

**Response**:

There might be some confusion over whether the conclusions are from our previous study (Wang et al. 2017) or this study, which we will better clarify in the revised manuscript. The relative importance of SOD (sediment oxygen demand) and WCP (water column production) is one of the conclusions from our previous study (Wang et al. 2017), but not in current study. This study builds off Wang et al. (2017) to focus on the response of each oxygen source and sink process to the different riverine inputs and their impacts on the hypoxia. We did emphasize the importance of SOD and the re-aeration to hypoxia in this manuscript but only aim to remind readers some key features of hypoxia formation in the Pearl River Estuary before more extensive investigations through sensitivity experiments.

Following the suggestion, we will add the equations of each component of WCP and some relevant descriptions in our revised manuscript.

16. P6, line 7: Please provide the equations for photosynthesis, respiration, nitrification and oxidation (potentially in an appendix), and provide values of all parameters used. (The reference used here for the model is a white paper from 14 years ago. The model has been adjusted since then. Are the authors really using those original parameters and equations? Please include information on the version of the model that is being implemented.)

**Response**:

As mentioned in our response to comment #4, we will provide the equations for each component of WCP and the descriptions of relevant parameters. The reference here (HydroQual, 2004) is the manual of the RCA. Equations we used in this manuscript are the same as in this document. Parameters are set according to this document and previous studies (Liu et al., 2016; Zhang and Li, 2010) that used the same coupled biogeochemical model for the Pearl River Estuary. Values of the primary parameters will be summarized in a table in our revised manuscript.

17. P6, line 12: Define what is meant by "dissolved matter". Is this dissolved organic matter, i.e. DON and DOC? Or dissolved nutrients, i.e. ammonium? Or both?

**Response**:

Here the 'dissolved matter' is referred to all nutrients considered in our model, i.e. NO2+NO3, NH4, PO4, and DSi. We will clarify it in our revised manuscript.

18. P6, line 18: As above, please provide values of these parameters within this paper (possibly in an appendix.)

**Response**:

We will provide values of primary parameters in our revised manuscript.

19. P6, line 27: As above, please provide equations and parameter values for DO_sed (possibly in an appendix)

**Response**:

We will provide equations and parameters for DO_sed in our revised manuscript.

20. P7, line 2: Earlier the authors stated that this is a dynamically coupled model, but here it sounds as if the water quality model is being run offline from the physical model, which would indicate that there is no dynamic coupling, and the biological simulation cannot impact the physics. Please make it clearer in the text as to whether the models are truly dynamically coupled, or simply run offline.

**Response**:

True, the physical model and the water quality model are only one-way coupling, where the physical model can affect the water quality model but the water quality model cannot impact the physical model. As suggested, we will revise the model description in our manuscript.

20. P 7 line 4: What data is being referred to here and how was it used? Data assimilation? Forcing? Validation?

**Response**:

Here we refer to the data used as the model input (e.g. riverine input and open boundary conditions) and for the model validation. In the revised manuscript, we will include more details about the model input data and make it clearer in the text.

21. P7, line 7: There are actually very few observations of DO presented in Wang et al. (2017). Are these really the only observations available of DO in the PRE region? Is nothing more available since 2006? It also looks like the oxygen data shown in Wang et al. rarely, if ever, actually go hypoxic?

**Response**:

During 2006, we only have observations in July and August. However we do have collected and analyzed the observation data from 1993 to 2009 (Figure r3), finding that there are very limited observations near the Modaomen sub-estuary (where the high frequency zone is located). Nevertheless, the model simulated hypoxia near the Modaomen sub-estuary in this study has also been reported in previous observational (Cai et al., 2013; Lin et al., 2001) and modelling studies (Zhang and Li, 2010), and the simulated spatial extent and characteristics here are consistent with those from previous studies. Additionally, noticing that the available oxygen data for validating simulated hypoxia is insufficient, in Wang et al. (2017) we have thoroughly validated the model against not only physical variables (i.e. water levels, salinity, and temperature), DO concentrations, but also the historical observations of some important biological variables (e.g. chlorophyll and particulate organic carbon) and processes (i.e. re-aeration, sediment oxygen demand, and primary productivity).

For the observational data in Wang et al. (2017), the minimum observed DO concentrations are below the hypoxic level (defined as 3 mg $L^{-1}$ here) and the observed hypoxic area is about 150 $km^2$ (pls. see Figure r3). It should be noted that the hypoxic area shown in Figure r3 was estimated based on the observation available and cannot represent the true state of hypoxia in the Pearl River Estuary.

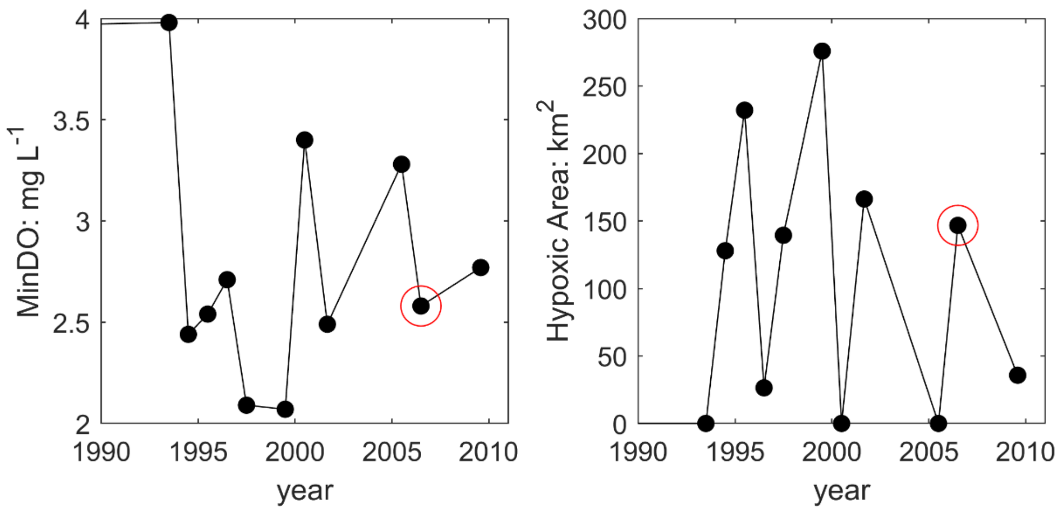


Figure r3. Multi-years variations in the observed minimum DO concentration (left panel) and hypoxic area. Red cycles indicate the July-August 2006.

22. P8, line 3: Because the authors have only evaluated model results for oxygen in July and August 2006, does this mean these results only are valid for that year? Is that a particularly wet year or a dry year? Or an average year? Can you put this year in perspective? (Perhaps in the discussion?)

**Response**:

Please see our response to comment #2.

23. Page 8, line 13: This sentence seems to indicate that this estimation is not straightforward only in river dominant estuaries. How about tide dominant estuaries, which can also be impacted by local and remote source and sink processes? P9, line 8: What does “Cont” stand for? Continuous? I would think “Base” or “Reference” or “Realistic” might be better descriptions of this simulation.

**Response**:

The estimation is not straightforward in the tide dominant estuaries either. According to our results, the oxygen supplied by re-aeration can penetrate to the bottom waters through the vertical diffusion (see section 4.1 in Wang et al. (2017) and also P15 line 13-18 in this manuscript). It follows that in a tide dominant estuary, the tide-induced mixing can facilitate the penetration. In the revised manuscript, we will modify the sentence to include the tide dominant estuaries.

The ‘Cont’ meant control case. As suggested, we will use ‘Base’ instead in our revised manuscript.

24. Section 2.3: The text is not clear here. Are the concentrations of DO and nutrients reduced in all 8 rivers, or only the Humen? Also it is not clear whether the concentrations of DO and nutrients in the experiments are set to what is predicted in 2050, or are simply increased by 50%. In reality, the concentrations in 2050 will depend on management decisions which are very difficult to predict. I think it's best to state here that you increased/decreased the concentrations by 50%, and if you want to convince the reader that these are representative of 2050 and 1970 respectively, then bring this up in the discussion. Please provide the concentrations of DO and nitrate (as an example nutrient) used in each of these experiments. More detail is needed here. If freshwater flows stay the same, this should be stated.

**Response**:

The riverine inputs of DO, nutrients, and particulate organic carbon are reduced in all 8 rivers. In 'RivNtr+50%' simulation, we increased the nutrient loading by 50% in all 8 rivers, which is close to the increase in nutrient loading in 2050 predicted by Strokal et al., (2015). As suggested, we will provide more details about the numerical experiments in our revised manuscript.

25. P11, line 19: Remove HFZ acronym since it is not used elsewhere. Please define the hypoxic frequency zone more quantitatively since this is used throughout the text. Where exactly is this? It's hard for the reader to know. Does it change in time?

**Response**:

We will remove the HFZ acronym in the revised manuscript as suggested. The high frequency zone is defined as the area encompassed by 10% isoline of hypoxic frequency in the Cont case (will be renamed as the Base case in the revised manuscript). We have depicted the high frequency zone with white lines in Figure 6. The high frequency zone remains unchanged in time in our manuscript.

26. P11, line 23: The word "additionally" should come before "occurs" since hypoxia also occurs on the shelf.

**Response**:

We will revise it as suggested

27. P13, line 9: Also list percent changes in hypoxia area and volume, as was done above.

***Response***:

As suggested, we will list the percent changes in our revised manuscript.

28. P13, line 22: Considering using PRE acronym earlier. (It hasn't been used much since very early in the manuscript.)

**Response**:

We will use the acronym earlier as suggested.

29. P14, line 14: Aren't there two POC simulations/experiments, not three?

**Response**:

Here we meant the two POC simulations and the Cont simulation (will be renamed as Base simulation as suggested). As shown in table 1 in the original manuscript, we have two POC simulations with increasing or decreasing the riverine inputs of particulate organic carbon by 50%. We will clarify it in our revised manuscript.

30. P14, line 24: In the results, it would make sense to discuss Figure 7 (the "Cont" results) before the sensitivity experiment results, rather than inside the section 2.3 sensitivity experiment section.
**Response**:

We think the reviewer meant section 3.3 instead of section 2.3 here. In the section 3.3, we found that changing the riverine inputs of particulate organic carbon could affect the re-aeration by changing the sediment oxygen demand and water column production (WCP). This finding leads to the discussion of Figure 7 which is to demonstrate the mechanisms of how the sediment oxygen demand influence the surface re-aeration quantitatively. Therefore we would like to keep the current presentation flow.

31. P14, line 25: The figure shows 0.53, not 0.55?
**Response**:

Right the value should be 0.53. We will correct it in the revised manuscript.

32. P15, line 1: How does the reader compute 0.13 from Figure 7?
**Response**:

As shown in Figure 7, the sediment oxygen demand (SOD) can affect the DO concentrations in the upper layer indirectly through the interactions with the vertical advection, the vertical diffusion, and the horizontal advection (horizontal diffusion is much smaller in magnitude and hence omitted here). First, for the vertical advection, the SOD consumes bottom DO and decrease the upward advective DO fluxes reaching the upper layer by 0.34 mg $L^{-1}$ $day^{-1}$. Second, for the vertical diffusion, the deoxygenation induced by SOD can increase the vertical DO gradient and facilitate the downward vertical diffusion by 0.02 mg $L^{-1}$ $day^{-1}$ from the upper layer. That means the SOD can consume the DO in the upper layer indirectly through the interactions with the vertical advection and vertical diffusion. Finally for the horizontal advection, the decreased upper DO concentrations affect the horizontal outfluxes of DO and ultimately result in a higher net horizontal advective flux by 0.21 mg $L^{-1}$ $day^{-1}$. Consequently, the net effects of the SOD on the upper DO should be (-0.34-0.02+0.21)=-0.15 mg $L^{-1}$ $day^{-1}$. In our original manuscript, we ignored the interactions between the sediment oxygen demand and the vertical diffusion to get -0.13 mg $L^{-1}$ $day^{-1}$. In our revised manuscript, we will correct it and more detailedly describe the calculation.

33. P15, line 2: Based on equation 8, I would think the dark blue DO bar would equal the sum of all the other bars, but this doesn't seem to be the case? Why is this?
**Response**:

This is because the sediment oxygen demand (SOD) is a sink for the oxygen and therefore the $DO_{SOD}$ is a negative term in equations (1), (3), (7)-(9), and (13) appeared in the section 2.1 and 2.2. However, in the other sections, we did not include the negative sign in $DO_{SOD}$, which means that $DO_{SOD}$ becomes a positive term and represents the removal of oxygen caused by the sediment oxygen demand. Accordingly, the equation should be $\Delta DO = \Delta DO_{BC} + \Delta DO_{REA} + \Delta DO_{WCP} - \Delta DO_{SOD}$ (Dark blue bars = light blue bars + green bars + orange bars - yellow bars in the Figure 5 b-c). We will correct the equations in the revised manuscript to be consistent for the sign of $DO_{SOD}$.

For example, compared with the 'Cont' simulation (will be renamed as the Base simulation as suggested), decreasing the riverine inputs of particulate organic carbon in RivPOC-50% will weaken the SOD and lead to a decrease in the magnitude of $DO_{SOD}$ (the removal of oxygen caused by the SOD). That means the bottom DO will be increased by 0.51 mg $L^{-1}$ (decrease in $DO_{SOD}$ but increase in DO). In addition, decreasing the POC inputs weakens the light attenuation and facilitates the primary productivity, leading to an increase of bottom DO by 0.3 mg $L^{-1}$ (increase in $DO_{WCP}$). At the same time, the decrease in riverine POC inputs will weaken the re-aeration and decrease the bottom DO by 0.22 mg $L^{-1}$ (increase in $DO_{REA}$). Considering all of these processes, the bottom DO will be elevated ultimately by 0.51+0.3-0.22=0.59 mg $L^{-1}$.

34. P15, line 11: It is important to qualify the 217km2 statistic by saying that this is true only for a July/August average in 2006. This is not true for other months of the year, and we don't know whether this is true for other years.

**Response**:

Yes, we will clarify it in our revised manuscript.

35. P15, line 15: DO_REA is not a term that your readers will be familiar with (unless they have read this paper carefully). This paper will have a greater impact if this could be reworded such that processes are mentioned, i.e. discuss the re-aeration of surface water via air-sea flux (in units of oxygen per unit time), rather than DO_REA.

**Response**:

The text in P15, line 15 meant the oxygen supplied by the re-aeration can penetrate to the bottom waters and compensate the oxygen loss caused by other processes. We agree that the term $DO_{REA}$ is not familiar to readers, so we will use the full name of $DO_{REA}$ (i.e. the oxygen supplied by the re-aeration) here. We will also define the key variables more clearly (e.g. $DO_{BC}$, $DO_{WCP}$, $DO_{SOD}$, and $DO_{REA}$) in the revised manuscript.

In our original manuscript, we did not use the word 're-aeration' (in units of oxygen per unit time) because re-aeration is the air-sea flux of oxygen which occurs on the air-sea interface, which does not guarantee to penetrate to the bottom waters.

36. P15, line 19: Again where is the hypoxic frequency zone? Where is "the west of the lower estuary"? Also, make it clearer that this is a result of Wang et al. (2017) and not of this paper.

**Response**:

The hypoxic frequency zone is defined as the areas compassed by the isoline of 10% hypoxic frequency in the Cont case (will be renamed as the Base case as suggested). We have depicted the hypoxic frequency zone by the white line in Figure 6. The west of the lower estuary is represented by the red box in the following figure. In the revised manuscript, we will modify the relevant figures to show where the hypoxic frequency zone and the west of the lower estuary are.

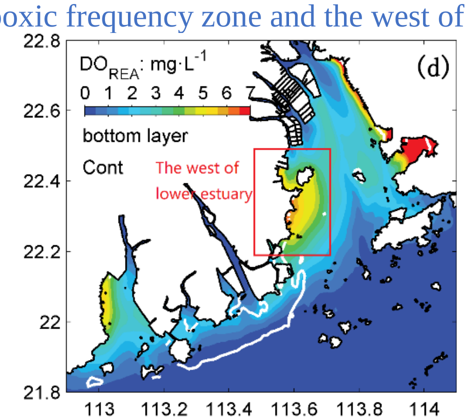


Figure r4. Distributions of bottom DOREA for the Cont case. The west of the lower estuary is indicated as the red box

37. P15, lines 4-8: This is a very interesting result! But unfortunately this paper does not show any statistics on water column respiration, so this is not clear. Please separate out the various terms inside WCP so the reader can see specifically that water column respiration is not large here.

**Response**:

please see our response to the comment 4.

38. P16, line 15: I don't think the authors mean the residence time of the Mississippi River, which extends a great distance, well up into the continent of North America. Do you mean the shelf plume area? This section would be much stronger if the authors compared all three systems mentioned here: the GoM, Chesapeake Bay and the PRE.

**Response**:

Here we meant the residence time of bottom waters in the hypoxic part of the shelf. The residence time of 95 days is cited from the Rabouille et al., (2008) where they compared the hypoxia in four different river systems (i.e. The Yangtz river, the Mississippi River, the Pearl River, and the Rhone River).

We have compared the three hypoxic systems (i.e. the Chesapeake Bay, the northern Gulf of Mexico (NGOM), and the PRE) to explain why the hypoxia in PRE is most sensitive to the riverine input of particulate organic carbon (P16 line 3-18). The main differences between the three hypoxic systems are summarized as below. As we discussed in the manuscript, in contrast to the NGOM and the Pearl River Estuary, the water column respiration induced by excess nutrients is the dominant oxygen depletion process in the Chesapeake Bay. As a result, the hypoxia in the Chesapeake Bay is very sensitive to the nutrient loading. In the NGOM and the PRE, the dominant roles of the sediment oxygen demand have been reported in many previous studies based on both observational (Murrell and Lehrter, 2011; Yin et al., 2004) and modelling studies ((Yu et al., 2015b; Zhang and Li, 2010). Hypoxia in the NGOM can be simulated well even neglecting the water column processes (Yu et al., 2015a). However, in the NGOM, the particulate organic carbon (POC) produced by phytoplankton (autochthonous POC) is the major contribution to the sediment oxygen demand. While in the PRE, the riverine input of POC (allochthonous POC) is the dominant source. The differences in relative contributions of riverine POC versus phytoplankton-produced POC between the NGOM and the PRE result in the different response of hypoxia to the nutrient loading. But as suggested, we will discuss the differences in the relative contributions of allochthonous versus autochthonous POC between the PRE and the NGOM. Details can be seen in Table r1, which will be included in the revised manuscript.

Table r2. A summary of characteristics of hypoxia among three systems (i.e. Chesapeake Bay, northern Gulf of Mexico, and Pearl River Estuary)

| | WCR dominant | SOD dominant | |
|---|---|---|---|
| | | autochthonous POC dominant | allochthonous POC dominant |
| Chesapeake Bay | √ | | |
| Northern Gulf of Mexico | | √ | |

39. P16: Rather than discussing terrestrial vs. marine POC, I think it would be clearer to discuss autocthonous vs. allochthonous POC. "Marine POC" sounds as if it comes from outside the hypoxic zone from the ocean, but I don't think this is what is meant?

**Response**:

We will revise the terms as suggested in our revised manuscript. We had attempted to use the terrestrial POC and marine POC to represent the POC delivered from the river network and produced by the phytoplankton, respectively.

40. P16, line 22: Is July-August a wet or dry season?

**Response**:

As shown in Figure r2, the July-August are two typical wet seasons with the monthly averaged river discharges over 20,000 $m^3$ $s^{-1}$. We will state it clearly in the revised manuscript.

41. Section 4.1: This section needs to describe more completely the difference in marine vs. terrestrial POC in the PRE vs. Gulf of Mexico. Why does terrestrial POC not impact hypoxia? Just because that the POC entering from the river is relatively small and sinks out before making it al the way to the shelf? Or is there something specifically different about the terrestrial matter entering from the Mississippi compare to that being delivered to the PRE? Is this a residence time issue? Is the terrestrial source more important in the PRE because the nutrient inputs are quite low, compared to what they are in the Gulf of Mexico? What about in the Chesapeake Bay?

**Response**:

We will provide more discussions on the differences in the PRE and the Northern Gulf of Mexico (NGOM).

First, the relative magnitudes of allochthonous POC input versus autocthonous are different in the two hypoxic systems. The terrestrial inputs of POC in PRE and NGOM are at same magnitude, $2.5\times10^6$ t $yr^{-1}$ (Zhang et al., 2013) and $3.8\times10^6$ t $yr^{-1}$ (Wang et al., 2004), respectively . However, the marine inputs of POC in the two systems are quite different. According to our model results, the primary productivity in the Pearl River Estuary is 310.8±427.5 mg C $m^{-2}$ $day^{-1}$, which is within the range of 183.9~1213 mg C $m^{-2}$ $day^{-1}$ reported by Ye et al., (2014) (see Table 3 in our manuscript). However, the primary productivity in the NGOM ranges from 330 to 7010 mg C $m^{-2}$ $day^{-1}$ (Quigg et al., 2011), which is much higher than in the PRE. The relatively lower primary productivity in the PRE is a result of the phosphorus limitation (DIN:DIP ratio of 126 versus 33 in PRE and NGOM, respectively, pls. see table 3 in our manuscript) and light shading of the high suspended sediment concentrations. Previous studies have also mentioned the dominant roles of the allochthonou POC in estuaries with high turbidity (Fontugne and Jouanneau, 1987; Middelburg and Herman, 2007).

Second, fates of the allochthonou POC are different in PRE and NGOM. Previous studies have suggested a good correlations between the relative contributions of allochthonou POC and salinity, indicating a general less contributions of allochthonou POC as salinity increased seaward (Fontugne and Jouanneau, 1987; Middelburg and Herman, 2007). Similar correlations between the relative importance of allochthonou POC versus salinity have also been reported in the PRE (Yu et al., 2010) and the NGOM (Wang et al., 2004). According to our model results, the surface salinity in the high hypoxia frequency zone varies between 0 to 10 psu during the wet season. However, the surface salinity in the hypoxic zone of the northern Gulf of Mexico is salter than 24 psu even in the wet season (Yu et al., 2015a). Residence time can also be used here to explain the differences in the two hypoxic systems. In the PRE, the residence time is 3~5 days during the wet season, which is much shorter than in the NGOM (~95 days). This means the allochthonou POC cannot be degraded completely and hence can fuel the sediment oxygen demand in the Pearl River Estuary.

Finally, compositions of the allochthonou POC are different in the two hypoxic systems. A study conducted by Zhang and Li (2010) has pointed out that contributions of labile POC to the allochthonou POC are higher in the PRE than the NGOM.

Unlike the PRE and the NGOM, the hypoxia in the Chesapeake Bay is more controlled by the water column respiration (Hong and Shen, 2013). It follows that the autocthonous POC is more important than the allochthonou POC to the hypoxia formation. One possible reason is probably the relatively long residence time in the Chesapeake Bay (180 days (Du and Shen, 2016)), which allows the complete degradation of the allochthonou POC before entering the hypoxic zone.

42. P17, line 17: Isn't the same likely to occur in Chesapeake Bay? This might be a very interesting discussion point here.

**Response**:

Here we compared the Pearl River Estuary with the northern Gulf of Mexico to demonstrate that the high re-aeration in the Pearl River Estuary is due to the high sediment oxygen demand and shallow waters. Extended discussions on the Chesapeake Bay will be largely limited by the lack of observations and relevant studies on the re-aeration. To our knowledge, few studies have mentioned the direction (source vs. sink) or the magnitude of re-aeration in the Chesapeake Bay. According to our results, we can speculate qualitatively that the sediment oxygen demand should have little impacts on the surface re-aeration considering that the sediment oxygen demand is much less important and the central channel (where hypoxia occurs) is quite deep (~30 m) in the Chesapeake Bay. However, the strong water column respiration may enhance the re-aeration.

As suggested, we will include the discussion on the Chesapeake Bay in our revised manuscript.

**Figures:**

43. Figure 1: The figures do not look to be italicized (as it says in the caption). Fig 1b does not add any significant information to what appears in Fig 1a. In Fig 1c "grids" should be "grid". The Fig 1a caption should note that this is a bathymetric map.

**Response**:

We will modify the caption in the revised manuscript.

We would like to keep Figure 1b which shows the computational cross-sections of 1-D model. In the revised manuscript, we will state the different purposes of showing Figure 1a and Figure 1b.

We will replace 'grids' with 'grid' in Figure 1c and revised the caption of Figure 1a as suggested.

44. Figure 3: The text refers to 3a and 3b, but the left panel is not marked (b), and there is no reference to (b) in the caption.

**Response**:

We will add the missing (b) in the left panel. We will also add the reference to (b) in Figure 3.

Figure 5: The y-axes in (b)-(d) should specify that these are “Changes in concentration”, not concentrations themselves. Also, (b)-(d) should have same y-range to make it easier for the reader to compare all three figures.

**Response**:

We will revise Figure 5 as suggested.

45. Figure 6: Please label figures (a)-(e) and provide captions for each. What is the white line? Axes are not labeled.

**Response**:

We will provide captions for each panel in the revised manuscript. The white line in Figure 6 denotes the high frequency zone. We will add the information in the revised manuscript. Labels will also be added to the axes in Figure 6.

46. Figure 7: This figure is a little confusing, because one would expect that the vertical diffusion out of box 1 would represent the vertical diffusion into box 2. I gather the net diffusion arrows are shown, but maybe it would make more sense to show the middle layer as having a +0.02 diffusion into the middle layer at the top, and a -0.17 diffusion out of the middle layer at the bottom? But why doesn’t this equal 0.48? Maybe I’m confused because this is only DO_SOD, and not total oxygen? Wouldn’t this be a more enlightening figure if all the DO fluxes were shown here?

**Response**:

Yes, fluxes shown in Figure 7 represent the net fluxes of -$DO_{SOD}$. The purpose of Figure 7 is to explain how the sediment oxygen demand can affect the surface re-aeration. Other DO fluxes can be seen in Figure 11 and Figure 12 in our previous study (Wang et al., 2017). In our revised manuscript, we will also make a statement of the purpose of Figure 7 and add a reference of other DO fluxes to our previous study.

As suggested, we will modify the Figure 7 to show the influx and outflux separately. New figure 7 which will be used in the revised manuscript is shown here. The negative values represent the outfluxes and the positive values represent the influxes. Take the vertical diffusion (red arrows) as an example, the outflux from the upper layer is 0.02 mg $L^{-1}$ $day^{-1}$. Considering the mass conservation, the mass of DO (in unit of mg) leaves from the upper layer should be equal to the mass enters the lower layer. As we mentioned in P14 line 22-23, volume of the middle layer is three times as large as upper and bottom layers. Therefore, the influx into the middle layer should be $0.02/3 \approx 0.01$ mg $L^{-1}$ $day^{-1}$. As analogy to the upper-middle layer, the outflux from the middle layer should be one thirds of the influx into the bottom layer ($0.48/3 \approx 0.16$ mg $L^{-1}$ $day^{-1}$). Since the influx and outflux of the middle layer are -0.01 and 0.16 mg $L^{-1}$ $day^{-1}$, respectively, the net flux is 0.15 mg $L^{-1}$ $day^{-1}$ which was shown in the Figure 7 in our original manuscript.

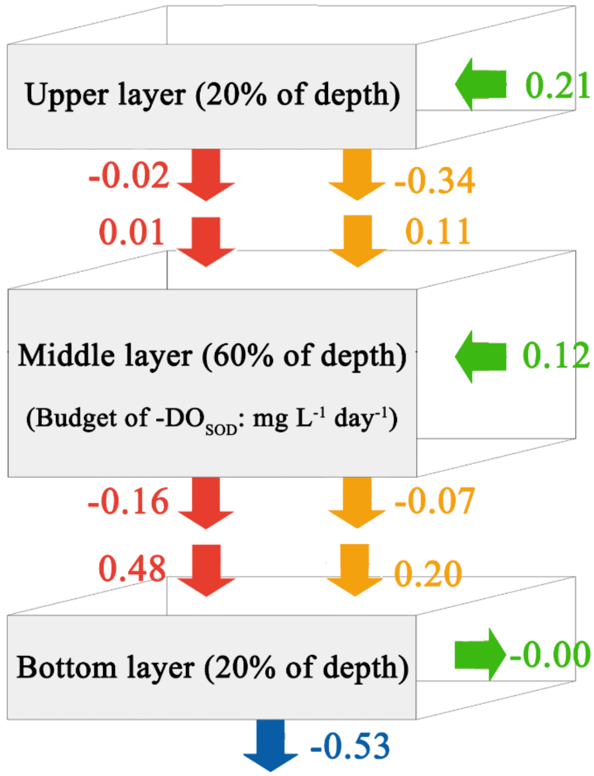


Figure r5. Budget of -DO$_{SOD}$ for the upper layer, middle layer, and bottom layer in the Pearl River Estuary for the 'Cont' case (will be renamed as Base case in the revised manuscript).

**English language comments:**

Throughout, "organic matters" should be changed to "organic matter". And similarly "dissolved matters" should be "dissolved matter".

P 4, line12 – processes should be process
P6, Line 26 – transportation should be transport
P7, line 7 – delete "here" and "as"
P8, line 16 – should be "interacting"
Page 12, line 1: "further" should be "farther"
P14, line 17: should be " layer, exerting a strong constraint"
P15, line 14: supply should be supplies
P15, line 24: most should be "more"
P16, line 5: should be "are the most important processes"

**Response**:

Thank you for the detailed comments. We will modify them as suggested in our revised manuscript.

**Reference**

Cai, S. Q., Zheng, S. and Wei, X.: Progress on the hydrodynamic characteristics and the hypoxia phenomenon in the Pearl River Estuary, J. Trop. Oceanogr., 32, 1–8, 2013 (in Chinese with English abstract).

Du, J. and Shen, J.: Water residence time in Chesapeake Bay for 1980 – 2012 Water residence time in Chesapeake Bay for 1980 – 2012, J. Mar. Syst., 164(December 2017), 101–111, doi:10.1016/j.jmarsys.2016.08.011, 2016.
Fontugne, M. R. and Jouanneau, J.-M.: Modulation of the particulate organic carbon flux to the ocean by a macrotidal estuary: Evidence from measurements of carbon isotopes in organic matter from the Gironde system, Estuar. Coast. Shelf Sci., 24(3), 377–387, doi:10.1016/0272-7714(87)90057-6, 1987.
Hong, B. and Shen, J.: Linking dynamics of transport timescale and variations of hypoxia in the Chesapeake Bay, J. Geophys. Res. Ocean., 118(11), 6017–6029, doi:10.1002/2013JC008859, 2013.
Hu, J. and Li, S.: Modeling the mass fluxes and transformations of nutrients in the Pearl River Delta, China, J. Mar. Syst., 78(1), 146–167, doi:10.1016/j.jmarsys.2009.05.001, 2009.
Hu, J., Peng, P., Jia, G., Mai, B. and Zhang, G.: Distribution and sources of organic carbon, nitrogen and their isotopes in sediments of the subtropical Pearl River estuary and adjacent shelf, Southern China, Mar. Chem., 98(2–4), 274–285, doi:10.1016/j.marchem.2005.03.008, 2006.
HydroQual, I.: User's Guide for RCA (Release 3.0)., HydroQual, Inc., Mahwah, NJ., 2004.
Li, Y., Li, M. and Kemp, W. M.: A Budget Analysis of Bottom-Water Dissolved Oxygen in Chesapeake Bay, Estuaries and Coasts, 38(6), 2132–2148, doi:10.1007/s12237-014-9928-9, 2015.
Lin, H. Y., Liu, S. and Han, W. Y.: Potential Trigger CTB, from Seasonal Bottom Water Hypoxia in the Pearl River Estuary, J. Zhanjiang Ocean Univ., 21, 25–29, 2001 (in Chinese with English abstract).
Liu, D., Hu, J., Li, S. and Huang, J.: Validation and application of a three-dimensional coupled water quality and sediment model of the Pearl River Estuary, Huanjing Kexue Xuebao/Acta Sci. Circumstantiae, 36(11), 4025–4036, doi:10.13671/j.hjkxxb.2016.0145, 2016 (in Chinese with English abstract).
Middelburg, J. J. and Herman, P. M. J.: Organic matter processing in tidal estuaries, Mar. Chem., 106(1–2), 127–147, doi:10.1016/J.MARCHEM.2006.02.007, 2007.
Murrell, M. C. and Lehrter, J. C.: Sediment and Lower Water Column Oxygen Consumption in the Seasonally Hypoxic Region of the Louisiana Continental Shelf, Estuaries and Coasts, 34(5), 912–924, doi:10.1007/s12237-010-9351-9, 2011.
Quigg, A., Sylvan, J. B., Gustafson, A. B., Fisher, T. R., Oliver, R. L., Tozzi, S. and Ammerman, J. W.: Going West: Nutrient Limitation of Primary Production in the Northern Gulf of Mexico and the Importance of the Atchafalaya River, Aquat. Geochemistry, 17(4), 519–544, doi:10.1007/s10498-011-9134-3, 2011.
Rabalais, N. N., Díaz, R. J., Levin, L. A., Turner, R. E., Gilbert, D. and Zhang, J.: Dynamics and distribution of natural and human-caused hypoxia, Biogeosciences, 7(2), 585–619, doi:10.5194/bg-7-585-2010, 2010.
Rabouille, C., Conley, D. J., Dai, M. H., Cai, W. J., Chen, C. T. A., Lansard, B., Green, R., Yin, K., Harrison, P. J., Dagg, M. and McKee, B.: Comparison of hypoxia among four river-dominated ocean margins: The Changjiang (Yangtze), Mississippi, Pearl, and Rhône rivers, Cont. Shelf Res., 28(12), 1527–1537, doi:10.1016/j.csr.2008.01.020, 2008.
Strokal, M., Kroeze, C., Li, L., Luan, S., Wang, H., Yang, S. and Zhang, Y.: Increasing dissolved nitrogen and phosphorus export by the Pearl River (Zhujiang): a modeling approach at the sub-basin scale to assess effective nutrient management, Biogeochemistry, 125(2), 221–242, doi:10.1007/s10533-015-0124-1, 2015.
Wang, B., Hu, J., Li, S. and Liu, D.: A numerical analysis of biogeochemical controls with physical

modulation on hypoxia during summer in the Pearl River estuary, Biogeosciences, 14(12), 2979–2999, doi:10.5194/bg-14-2979-2017, 2017.
Wang, X.-C., Chen, R. F. and Gardner, G. B.: Sources and transport of dissolved and particulate organic carbon in the Mississippi River estuary and adjacent coastal waters of the northern Gulf of Mexico, Mar. Chem., 89(1–4), 241–256, 2004.
Ye, H., Chen, C., Sun, Z., Tang, S., Song, X., Yang, C., Tian, L. and Liu, F.: Estimation of the Primary Productivity in Pearl River Estuary Using MODIS Data, Estuaries and Coasts, 38(2), 506–518, doi:10.1007/s12237-014-9830-5, 2014.
Yin, K., Lin, Z. and Ke, Z.: Temporal and spatial distribution of dissolved oxygen in the Pearl River Estuary and adjacent coastal waters, Cont. Shelf Res., 24(16), 1935–1948, doi:10.1016/j.csr.2004.06.017, 2004.
Yu, F., Zong, Y., Lloyd, J. M., Huang, G., Leng, M. J., Kendrick, C., Lamb, A. L. and Yim, W. W. S.: Bulk organic δ13C and C/N as indicators for sediment sources in the Pearl River delta and estuary, southern China, Estuar. Coast. Shelf Sci., 87(4), 618–630, doi:10.1016/j.ecss.2010.02.018, 2010.
Yu, L., Fennel, K. and Laurent, A.: A modeling study of physical controls on hypoxia generation in the northern Gulf of Mexico, J. Geophys. Res. C Ocean., 120(7), 5019–5039, doi:10.1002/2014JC010634, 2015a.
Yu, L., Fennel, K., Laurent, A., Murrell, M. C. and Lehrter, J. C.: Numerical analysis of the primary processes controlling oxygen dynamics on the Louisiana shelf, Biogeosciences, 12(7), 2063–2076, doi:10.5194/bg-12-2063-2015, 2015b.
Zhang, H. and Li, S.: Effects of physical and biochemical processes on the dissolved oxygen budget for the Pearl River Estuary during summer, J. Mar. Syst., 79(1–2), 65–88, doi:10.1016/j.jmarsys.2009.07.002, 2010.
Zhang, L., Qin, X., Yang, H., Huang, Q. and Liu, P.: Transported Fluxes of the Riverine Carbon and Seasonal Variation in Pearl River Basin, Environ. Sci., 34(8), 3025–3034, doi:10.13227/j.hjkx.2013.08.043, 2013 (in Chinese with English abstract).