We wish to thank the referee for the detailed and constructive comments which will be helpful to the revision of our manuscript. According to the comments, we plan to make revisions and improvements as below: 1) to include more results of validations and comparison of model skills between our model and other published studies; 2) to give clearer descriptions and explanations about 'physical modulation method' and its associated concepts; 3) to have an English editor edit our manuscript and correct grammatical mistakes throughout the text.

Detailed responses to all comments are given below. (Responses are shown in *Italic Font*)

Comment: 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 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.

Response:

We would like to thank the referee for recognizing the importance of our study and the detailed comments. We have carefully read these comments and summarized them into three main criticisms. We believe that most of these criticisms were mainly due to the misunderstanding caused by improper use of words like 'ambient' (which should be 'adjacent') in the text and inadequate description on the 'physical modulation method'. We will provide a further description to the 'physical modulation method' and its associated terms in the revised manuscript and improve our manuscript according to these comments. Responses to the three main comments are given below.

Firstly, for the comments on the 'physical modulation method', it actually means the modulation of biogeochemical effects on the oxygen due to physical transport. In our study, we applied this method to tease apart the biogeochemical and physical aspects of DO transport by using numerical DO tracers and to further demonstrate the spatial connection between processes occurring in different locations in the system. Specifically, we considered that DO concentrations could be affected by the biogeochemical processes by two ways: 1) the biogeochemical processes can produce or consume oxygen to change the local DO concentrations directly; 2) the biogeochemical processes can also change DO concentrations in the adjacent grid cells and hence change the DO fluxes from these adjacent grid cells; as a result, the DO concentrations will be affected by the biogeochemical processes occurred in the adjacent grid cells. We defined the latter mechanism as the modulation of biogeochemical effects on the oxygen due to physical *transport.*

Regarding the 'contributions of ambient biogeochemical processes', it means the advective and diffusive fluxes of oxygen which is produced or consumed by the biogeochemical processes in the adjacent grid cells. We have realized that the word 'ambient' is misleading and we will change it to 'adjacent' as suggested in the revised manuscript.

Secondly, as to the comments on the observation data, we would like to emphasize that our model reasonably reproduced the spatial distributions of oxygen as can be seen from the comparisons between our model results and the available observations. In addition, we compared our simulated results associated with two important biogeochemical processes controlling hypoxia (i.e. the re-aeration process and the sediment oxygen demand) with historical observations (Table 2) in order to make our conclusions more persuasive. We realize that the available data for hypoxia is insufficient. In fact, we have collected the observation data from 1976 to 2009 (Fig. r1) and found that there were very few observation data available near the Modaomen sub-estuary (where the high frequency zone is located) on hand. However, hypoxia near the Modaomen sub-estuary has been reported in previous observational studies (Lin et al., 2001;Cai et al., 2013) and modeling studies (Zhang and Li, 2010). The spatial extent and characteristics of hypoxia in our study is consistent with these previous studies.

Nevertheless, we realize the necessity and importance to strengthen the relevant observations. Therefore, we will put forward some suggestions for further observations near the Modaomen sub-estuary in the revised manuscript. We believe that our study will provide a scientific basis and guidance for conducting these field observations. In addition, we will also provide more results for our model validation (e.g., water elevations, temperature and salinity profiles, chlorophyll-a, primary production, and particulate organic carbon, results can be seen from Fig. r2-r5 and Table r1) in the revised manuscript.

Finally, for the comment on the language, we will have an English editor edit our manuscript and correct grammatical mistakes throughout the text.

Comment: 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 km2— it confusing me how photosynthesis would worsen hypoxia (should be respiration, right? But respiration is not directly linked with photosynthesis).

Response:

Firstly, as mentioned above, the contributions of adjacent biogeochemical processes mean the advective and *diffusive fluxes of oxygen which is produced or consumed by the biogeochemical processes in the adjacent grid cells, not the boundary conditions. We realize that the word 'ambient' is misleading and we will change it to 'adjacent' as suggested in the revised manuscript.*

Secondly, regarding the re-aeration, it is an important source for the oxygen in the water column. In the manuscript (abstract, P1, line 29-30), we mentioned that turning off the re-aeration would lead to a significant expansion of hypoxic area with the occurrence of persistent hypoxia in the west lower estuary (where no hypoxia occurred in the realistic simulation). This means that the re-aeration is good for alleviating the hypoxia in the Pearl River estuary. Moreover, our oxygen budget analysis showed the re-aeration could affect the bottom layer through the vertical diffusion (Fig. 8c). As shown in the Fig. 8a, the re-aeration provided a large amount of oxygen to the surface water, leading to a strong vertical DO gradient between the surface and middle layers. As a result, the majority of oxygen entering into the surface layer by the re-aeration (represented by DO_{Rea}) was transported to the middle layer and ~28% could reach the bottom layer eventually (Fig. 8c).

Finally, regarding the photosynthesis, we realize that there is a mistake here. What it should be is that turning off the photosynthesis and water column respiration would cause an increase in the hypoxic area. We will correct this in the revised manuscript.

Comment: 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).

Response:

Firstly, as mentioned above, the word 'ambient' is misleading and will be changed to 'adjacent' in the revised manuscript. Here we are referring to the DO fluxes from the adjacent water bodies affected by adjacent biogeochemical processes (not from lateral boundary). We took the re-aeration as an example to explain the contribution of adjacent biogeochemical processes on DO.

Secondly, regarding the statement "given the mechanism remains unclear", it is referring to the mechanism of how the adjacent biogeochemical processes affect DO concentrations. We claimed it unclear because traditional methods have not teased apart and quantified the contributions of biogeochemical and physical processes to DO concentrations. We will provide a further description on the mechanism we would like to convey in the revised manuscript.

Thirdly, we recognize that the contributions of adjacent biogeochemical process are indeed a combination of both biogeochemical and physical processes. As mentioned earlier, the 'contributions of adjacent biogeochemical processes' mean the advective and diffusive fluxes of oxygen which is produced or consumed by the biogeochemical processes in *the adjacent grid cells. We realize that this statement is misleading and we will change it to 'the contributions of adjacent biogeochemical processes due to physical transport' in the revised manuscript.*

Finally, regarding the tidal range difference, we agree that it is an important factor affecting hypoxia. Currently, we are actually working on another study addressing the impact of the tides as well as other physical factors (e.g. river discharges, winds) on the hypoxia in the Pear River estuary.

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

Response:

Yes, we agree that the sediment oxygen demand and stratification are two important factors affecting the hypoxia in the Pearl River estuary. However, we note from our model results that the distributions of the sediment oxygen demand and hypoxia were inconsistent and there was a shift between the hypoxic zone and the high sediment oxygen demand areas. This inconsistency could also be found in previous studies (Zhang and Li, 2010) where however there was no further analysis or discussion on it. The shift between the hypoxic zone and the high sediment oxygen demand areas is caused by the modulation of biogeochemical effects on the oxygen due to physical transport. In our study, we introduced the 'physical modulation method' to investigate this process.

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

Response:

RDOC, LDOC, ReDOC, and ExDOC are the abbreviations for four state variables in the RCA water quality model. They represent refractory dissolved organic carbon (RDOC), labile dissolved organic carbon (LDOC), reactive dissolved organic carbon (ReDOC), and algal exudate dissolved organic carbon (ExDOC). The explanations for these abbreviations can be found in P6, line 3-4. To make it easier to notice, we will show these abbreviations using a table along with other main state variables and parameters for the RCA.

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

Response:

As mentioned earlier, we realize that the word 'ambient' could be misleading and will changed to 'adjacent'. What it means is the contributions of adjacent biogeochemical processes, not the contributions from the boundary conditions.

The river boundary conditions of water quality variables were specified using the monthly observations. The open boundary conditions were specified spatial constant according to limited observations.

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

Response:

Yes, the re-aeration should be a physical process and we will modify this statement in the revised manuscript.

Comment: Page 7. I am confused why the author needs to give repeated information in equation (6)-(12). They simply divide DO in to DObc and DObio, and they can just use equation (13), that is enough.

Response:

Okay, we agree that there is repeated information in equations (9)-(12) and we will delete these equations in the revised manuscript. For equations (6)-(8) we argue that they should be kept in the text. Equations (6)-(7) are two important transport equations for DO_{BC} *and* DO_{Bio}*, which explicitly demonstrate how to calculate these two variables. Equation (8) is also needed to illustrate* DO_{Rea}*,* DO_{Phot}*,* DO_{WCR}*, and* DO_{SOD} *as these variables were presented and discussed in the manuscript.*

Comment: 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.

Response:

Firstly, as suggested, we will provide more results for our model validation (e.g., water elevations, temperature and salinity profiles, chlorophyll-a，primary production, and particulate oxygen carbon) in the revised manuscript. Regarding the time series comparison, there were no time series observations (except water elevations) available for July-August 2006 (our study period). However, our datasets used for model validation include salinity and temperature from 146 cruise sites and water qualities from 53 cruise sites. These data were collected at different times of July and August 2006, with a time span of 39 days. The agreement between our simulations and observations shows that our model can reasonably reproduce the temporal and spatial distribution of hydrodynamic and water quality variables in the Pearl River estuary.

Comment: 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 difference between RCA and in-situ data?

Response:

Please note that the physical modulation method is not an improvement of RCA. It is a new method that we applied to tease apart the biogeochemical and physical aspects of DO dynamics by using RCA and the numerical oxygen tracers. Theoretically, there should be no differences between the two models since the transport equation is a linear equation. However, in the numerical model, the partial differential equations are approximately represented by difference equations. Some nonlinear numerical methods are introduced to restrain the errors caused by discretizing *the partial differential equations. We found that the differences between the two models are caused by these nonlinear numerical methods. We have conducted a test by turning off these nonlinear methods and found that the differences between the two models are removed but the model results become inaccurate. We would like to emphasize that the differences between the two models in our study are small and have little impact on the analysis of the hypoxia.*

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

Response:

It is the agreement between the DO fluxes calculated by RCA and those calculated using the physical modulation method. We will clarify this in the revised manuscript.

Comment: 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”?

Response:

We realize that this statement is repeated because by definition the HFZ (high frequency zone) refers to the area where hypoxia is most likely to occur. We will delete it in the revised version.

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

Response:

Here what it should be mean is that the HFZ is the area encompassed by the 10% isoline of hypoxic frequency. We will clarify this in the revised manuscript.

Comment: Page 11. Line 7-16. Should not these abbreviation goes to the figure caption?

Response:

Okay, these abbreviations will be removed in the figure caption in the revised manuscript.

Comment: Page 13. Line 4. “Although*: : :*. However *: : :* “very awkward wording

Response:

As suggested, we will modify this in the revised manuscript and have an English editor edit our manuscript.

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

Response:

We realize that there is a grammatical mistake here. The sentence should be corrected “the sediment oxygen demand and the re-aeration contribute 79% and 25% of vertical diffusive flux, respectively.” We will correct it in the revised manuscript.

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

Response:

Please note that the results related to the horizontal and vertical advective fluxes of oxygen are shown in Fig. 8a, not Fig. 8e. We will include a reference therein to Fig. 8a in the revised manuscript. Fig. 8a shows that the horizontal advection brings oxygen to the bottom layer in the PRE while the vertical advection brings oxygen out of the bottom layer. The horizontal advective flux of DO is close to the vertical advective flux of DO.

Comment: 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).

Response:

Please note that Fig. 10a shows the spatial distribution of the sediment oxygen demand in the PRE. As shown in Fig. 10a, the blue indicates high sediment oxygen demand and the red indicates low sediment oxygen demand. There are two areas highlighted by the blue, one of which is located inside the Lingdingyang sub-estuary and the other is located at the Modaomen sub-estuary. These two areas correspond to the 'two distinct areas characterized by the high level of sediment oxygen demands'.

Regarding the area delineated by the white line, it represents the HFZ (please see the caption of Fig. 10a). In Fig. 10a the HFZ was overlapped with the sediment oxygen demand in order to explicitly compare the spatial distributions between the sediment oxygen demand and the hypoxia. It can be seen that there is a shift between the hypoxic zone and the high sediment oxygen demand areas as mentioned earlier, which is caused by the modulation of biogeochemical effects on the oxygen due to physical transport.

Comment: 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?

Response:

Firstly, regarding this sentence "we therefore add sediment oxygen demand which is divided by the depth to water column respiration and photosynthesis to represent the gross depletion rate", it means that we used "SOD/depth+WCR+Phot" to represent the gross depletion of oxygen. Here, the SOD (sediment oxygen demand) and WCR (water column respiration) are negative to indicate that they are sinks for oxygen. In the revised manuscript, we will clarify this sentence.

Secondly, in previous studies (Murrell and Lehrter, 2011;Shen et al., 2013) the sediment oxygen demand was estimated using the sediment oxygen consumption divided by the depth below the pycnocline, assuming that the water below the pycnocline is well-mixed. In the Pearl River estuary, the depth of pycnocline significantly varies in time and space, which makes it difficult to determine. Alternatively, we estimated the sediment oxygen demand by using the *sediment oxygen consumption divided by the total depth of the water column. The result was underestimated compared to one calculated using the depth below the pycnocline. We will clarify this in the revised manuscript. It should be noted that this underestimated value in the Pearl River estuary is larger than in other hypoxic areas (e.g. the Chesapeake Bay and northern Gulf of Mexico) so that the underestimation has little impact on the qualitative comparisons between the Pearl River estuary and the other hypoxic areas.*

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

Response:

Please note that in the text we mentioned "This indicates that only the biogeochemical processes are not sufficient to explain the hypoxia in the HFZ since the effects of physical processes are also important" (P14, Line 27-28).

According to our model results, the distributions of the sediment oxygen demand and hypoxia are inconsistent. There is a shift between the hypoxic zone and the high sediment oxygen demand areas as mentioned earlier, which is caused by the modulation of biogeochemical effects on the oxygen due to physical transport. We will include the discussion above in the manuscript to make it clear.

Comment: 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 shows low oxygen in the so-called HFZ zone.

Response:

Please note that in the text we mentioned "Figure 10c shows the simulated DO_{Bio} distribution in the bottom is in reasonable agreement with DO distribution ……"(P14, Line 34-36). Here, the distribution of DO_{Bio} (Fig. 10c) was compared to hypoxia (Fig. 7 b, d) in order to demonstrate that DOBio could resemble the occurrence of hypoxia in the PRE.

For the comments on the observation data, please see responses to the three main criticisms above (we would like to emphasize that our model reasonably reproduced the spatial distributions of oxygen as can be seen from the comparisons between our model results and the available observations. In addition, we compared our simulated results associated with two important biogeochemical processes controlling hypoxia (i.e. the re-aeration process and the sediment oxygen demand) with historical observations (Table 2) in order to make our conclusions more persuasive. We realize that the available data for hypoxia is insufficient. In fact, we have collected the observation data from 1976 to 2009 (Fig. r1) and found that there were very few observation data available near the Modaomen sub-estuary (where the high frequency zone is located) on hand. However, hypoxia near the Modaomen sub-estuary has been reported in previous observational studies (Lin et al., 2001;Cai et al., 2013) and modeling studies (Zhang and Li, 2010). The spatial extent and characteristics of hypoxia in our study is consistent with these previous studies.

Comment: Page 15. Line 2-3. I could not tell that the lowest DOsod 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.

Response:

Please note that Fig. 11 shows the DO_{SOD} concentrations averaged over July-August 2006 and Fig. 4 shows comparison of the simulated and observed DO concentrations during the cruise periods. These two figures show results for different time periods.

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

Response:

It should be "Fig. 12'. We will correct this to "Fig.12" in the revised manuscript.

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

Response:

Yin et al. (2004) suggested that the occurrence of hypoxia in the Pearl River estuary was restricted due to phosphorous limitation because phosphorous limitation inhibited the complete utilization of nutrients and eventually limited oxygen consumption in the water column. In our study, we found that the sediment oxygen demand was the dominant source of oxygen consumption at the bottom layer in the Pearl River estuary, which was consistent with previous studies(Zhang and Li, 2010). The total oxygen consumption rate in the Pearl River estuary was higher than those in the Chesapeake Bay and the northern Gulf of Mexico while the hypoxia in the Pearl River estuary was less severe (see comparison in Table 3). This indicates that there should be a different mechanism rather than phosphorous limitation controlling the hypoxia in the Pearl River estuary.

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

Response:

Please note that Fig. 10c shows the DO_{Bio} averaged over July-August 2006 and Fig. 4 shows comparison of the simulated and observed DO concentrations during the cruise periods. These two figures show results for different time periods.

In the text, the distribution of DO_{Bio} (Fig. 10c) was compared to hypoxia (Fig. 7 b, d) in order to demonstrate that DOBio could resemble the occurrence of hypoxia in the PRE.

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

Response:

As suggested, we will modify this in the revised manuscript.

Comment: 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"?

Response:

In our study, the physical modulation method was applied to tease apart the contributions of boundary conditions, physical and biogeochemical processes to the oxygen. The oxygen originating from the boundaries was represented by DO_{BC}. Fig. 8d and Fig. 9d showed that at the bottom of the PRE and HFZ, the horizontal advection fluxes of DO_{BC} were ~0.07 and 0.90 mg L^{-1} day^{-1}, respectively.

Reference

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Shen, J., Hong, B., and Kuo, A. Y.: Using timescales to interpret dissolved oxygen distributions in the bottom waters of Chesapeake Bay, Limnology and Oceanography, 58, 2237-2248, 2013.

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Zhang, H., and Li, S. Y.: Effects of physical and biochemical processes on the dissolved oxygen budget for the Pearl River Estuary during summer, Journal of Marine Systems, 79, 65-88, 2010.

Table r1 Comparisons between the simulated and observed chlorophyll-a, primary production, and particular organic carbon (POC)

	Simulated	Historical observed	period
Chl-a (μg L^{-1})	1.92±1.96	1.64[a]	June 2012
PP (mg m^2 day^{-1})	310.8±427.5	302.9[a]	June 2012
POC (mg L^{-1})	<0.5-3.01	0.40~>2.50[b]	November 2013, February 2014 May 2014 and August 2014

[a] *Ye et al. (2015)*

[b] *Guo et al. (2015)*

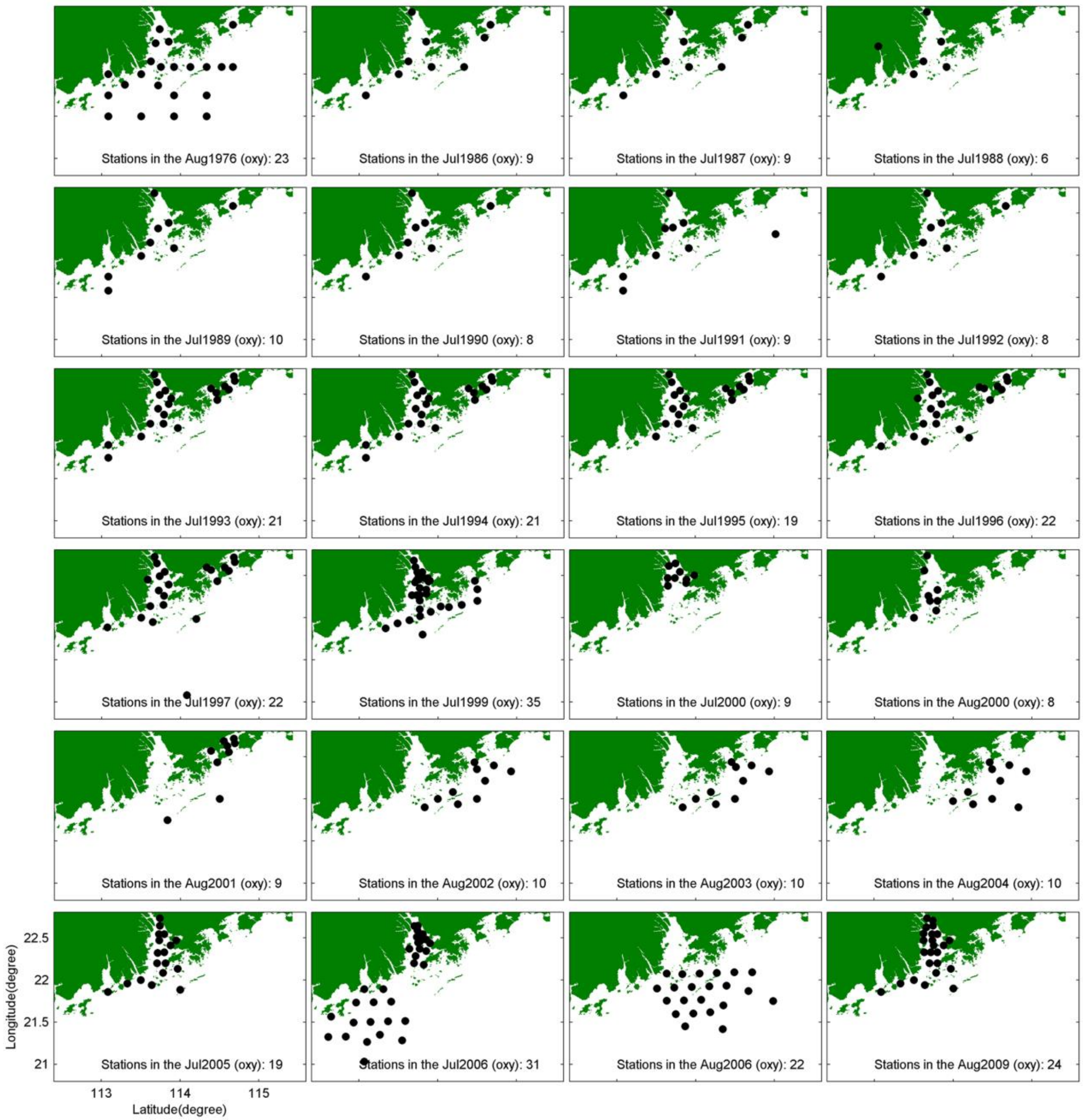

Fig. r1 Survey stations of oxygen in the Pearl River estuary from 1976 to 2009

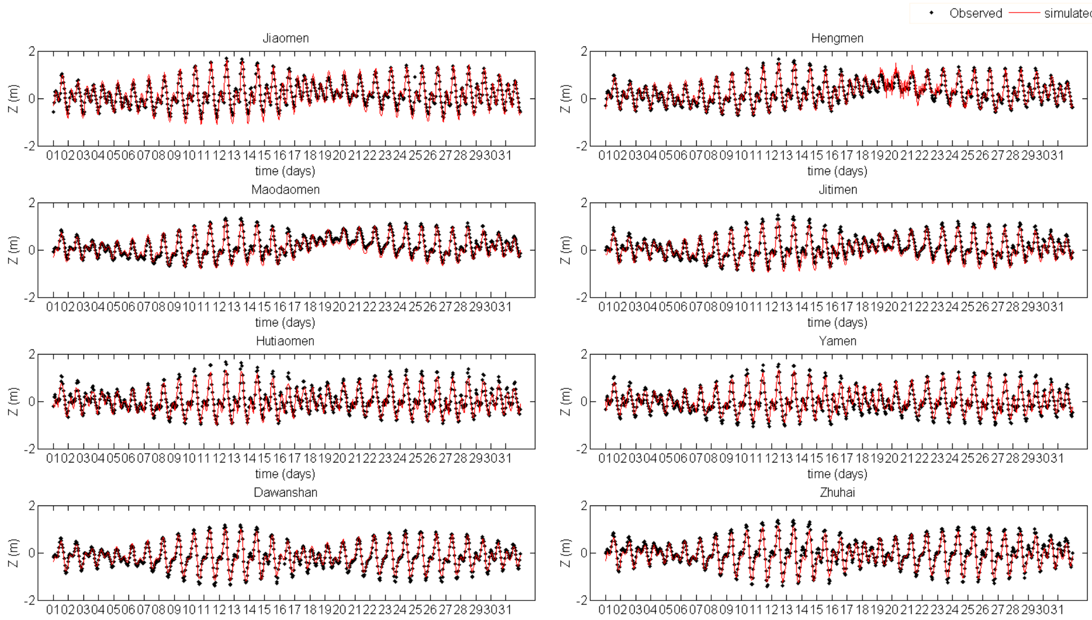

Fig. r2 Model-data comparisons of water elevation for 8 tidal stations (July 2006; the locations for these stations will be shown in Fig. 3a in the revised manuscript)

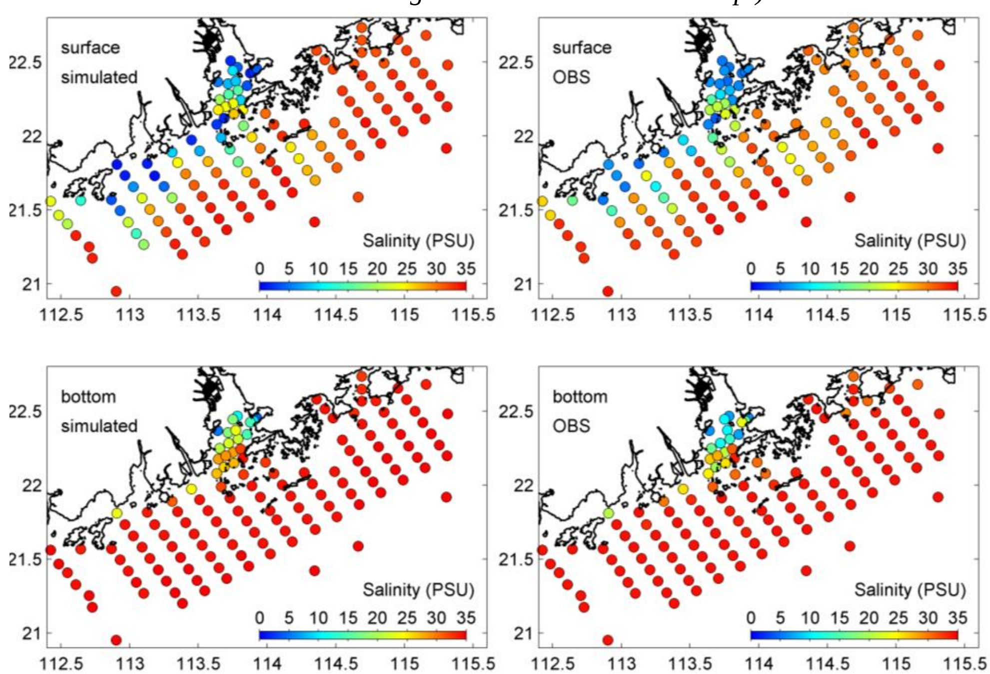

Fig. r3 Model-data comparisons of salinity at the surface and bottom layers (July and August 2006)

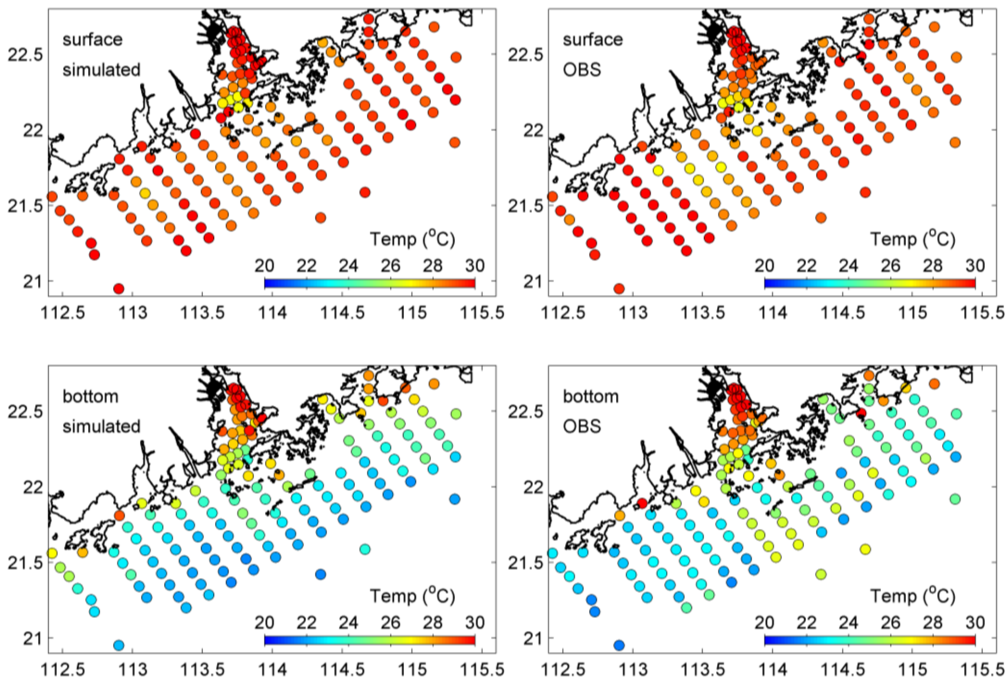

Fig. r4 Model-data comparison of temperature at the surface and bottom layers (July and August 2006)

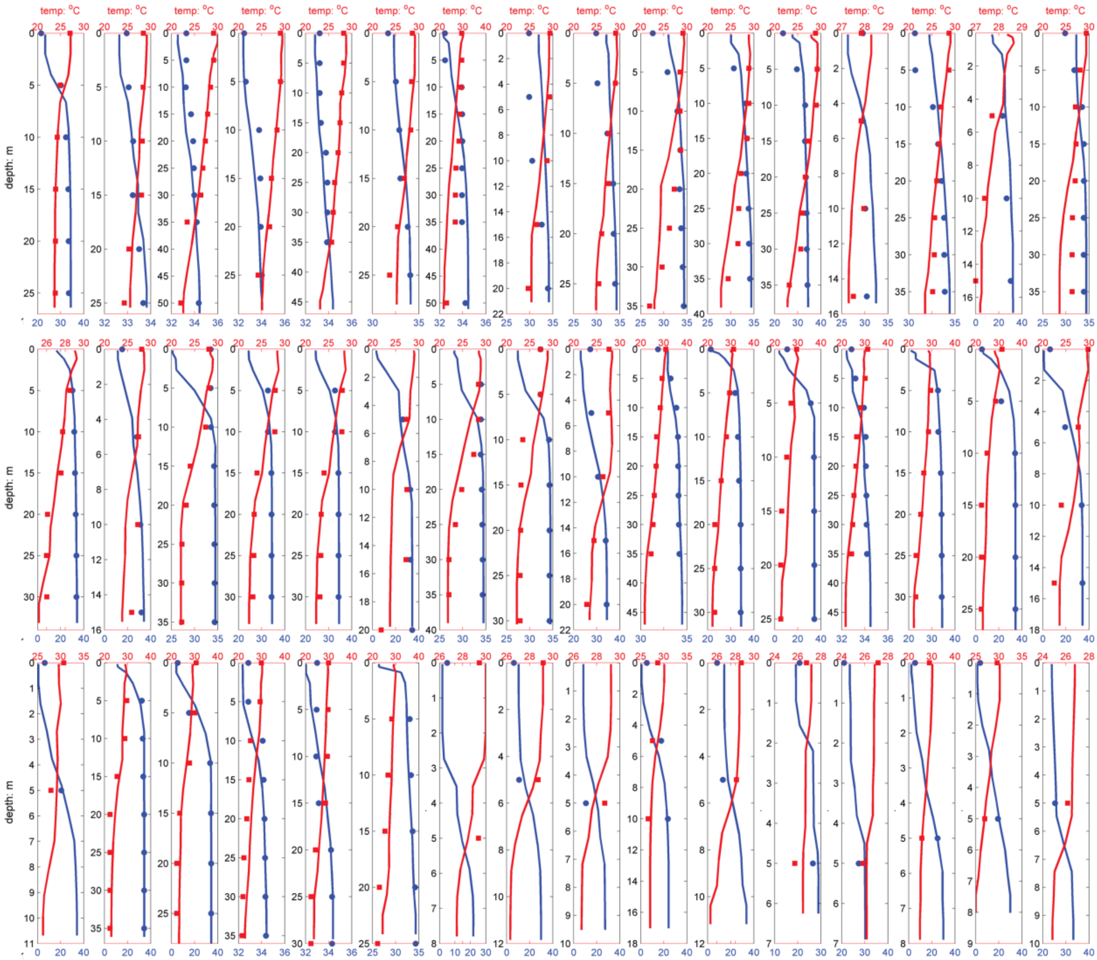

Fig. r5 Model-data comparisons of salinity (blue) and temperature (red) profiles at 48 stations (July and August 2006; the locations for these stations are shown in Fig. r6)

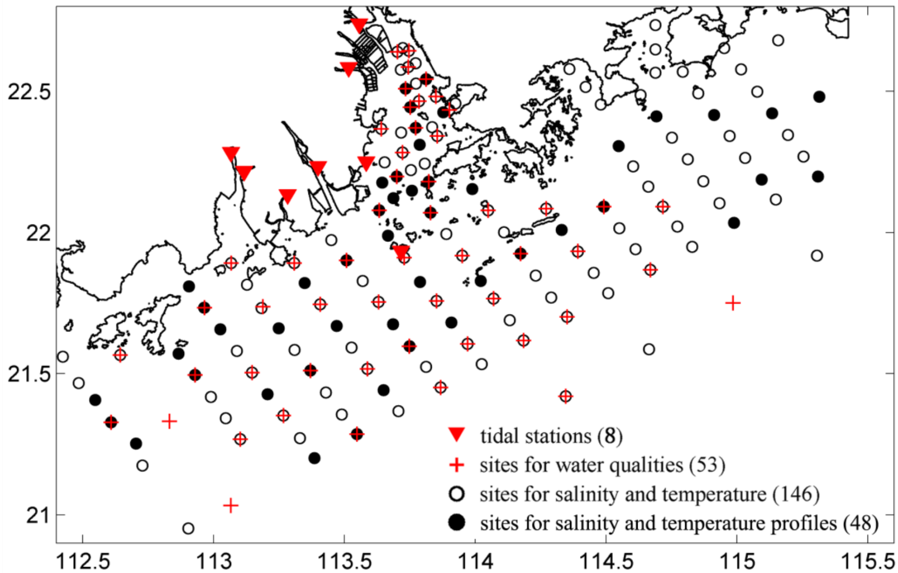

Fig. r6 Survey stations of water elevation (red triangles), salinity and temperature (black circles), salinity and temperature profiles shown in Fig. r5 (black solid circles), and water quality variables (red crosses) during July and August 2006