Anonymous Referee #2

We wish to thank the referees for the constructive comments and suggestions which are helpful to the revision of our manuscript. Detailed response to all comments are given below (responses are shown in blue)

General Comments

The low oxygen condition in the Pearl River Estuary has been frequently happened due to large inputs of freshwater, nutrients, and diverse contaminants from the Pearl River in recent years. With the rapidly growing population and socio-economic development at the Guangzhou, Shenzhen, and Hongkong Great Bay Area, the problem aroused many scientific community and government attentions. There has been a lot of studies on the low oxygen zone using observational data and a variety model. However, most of them were focused on short-time scale events and the associated controlling mechanisms. As far as I know, the only long-term trend study was Qian et al. (2018), but the discussion was only limited to one monitoring station south of Hong Kong rather than the entire Bay. The paper collected over four decade of cruise observations to investigate spatiotemporal variability of low oxygen condition in PRE to investigate the long-term low oxygen condition variability. It also reported that an early Autumn hypoxic event in the year 2006 and revealed the controlling mechanisms. The work is noval and the story is interesting. The manuscript is well written, flows well from topic to topic, is clear and understandable. It also structured well and the figures presented can back up the conclusion reached. I suggest acceptance after a moderate revision after considering the following points.

Response: We are very grateful to the reviewer for the positive comments and recognition of the novelty and significance of this work. We will revise the manuscript as suggested.

Major comments:

1. My major concern of the work is the inconsistency in data sampling for the long-term hypoxic area variability reported. The multi-year cruise data were not at closer stations like Gulf of Mexico or Chesapeake Bay. For example, Aug 1999 (Figure 6d2) had only five data in the Lingding Bay. All data in July 2017 are outside the Bay (Figure 6e4). This bring a problem that the area number (HA2, HA3, HA4) are lack of consistency between years. One suggestion here is putting all stations together, and finding ways to derive an oxygen number for no observation stations, and then do the calculation again. There are many of research papers for interpolation method to generate hypoxia area/volume in the Gulf of Mexico and Chesapeake Bay. The authors can introduce one of them to remedy the data inconsistency issue in the research.

Response: We totally understand the reviewer's concern. In fact, although the summertime hypoxia in the Pearl River estuary (PRE) has been reported since the 1980s, there is still a lack of understanding of its long-term evolution. One major reason is the lack of accessible continuous observations for oxygen and a synthesis of relevant historical data. To the best of our knowledge, this is a first study to collect the estuary-wide historical observations over 40 years and attempt to elucidate the long-term evolution of low-oxygen conditions in the PRE. Since these observations were conducted by different institutions for different scientific purposes, there is inevitable inconsistence in the sampling stations.

We would like to thank the reviewer very much for the suggestions. However, the extrapolation of oxygen data into unobserved stations will introduce large uncertainties, especially when the sampling stations are often limited and localized before the 2010s. We have fully realized the data limitations in use. As we discussed in section 4.4 of our manuscript, the data gaps in some years and the lack of conformity in observational coverage largely limit our ability to quantify the long-term changes of low-oxygen conditions in the PRE. We are also very cautious about our conclusions and try not to overinterpret them. Nevertheless, our findings on the declining trend of bottom DO and its spatial expansion in the PRE emphasize the importance of conducting estuary-wide surveys to collect extensive oxygen data in a consistent way. Based on the conclusions from our study, we would also suggest to build the estuary-wide estimates of oxygen by combining these long-term observations with the numerical models and/or machine learning systems to better quantify the long-term oxygen changes and the associated mechanisms; however, this is beyond the scope of this study.

2. Another concern of me is the early autumn low oxygen condition. To me, it seems only exist in September 2006, not other years. It should be careful for the conclusion that hypoxia undergoing a transition from episodic to seasonal regarding the time scale.

Response: It is our statement that has caused this misunderstanding. Actually, the conclusion refers to the potential transition of the summertime hypoxia in the PRE, which was deduced by the long-term observational oxygen data in summer. As pointed out by the reviewer, we also realized that this conclusion is not applicable to the hypoxic conditions in early autumn due to the data limitations in this period. Based on the reviewer's suggestion, we will revise the conclusion to make it explicitly referring to the summertime hypoxia.

3. Lastly, I would expect to see a discussion about comparing long-term variability hypoxia study with other systems, like Chesapeake Bay and Gulf of Mexico.

Response: We agree that it would be interesting to compare the long-term variabilities of hypoxia across different systems under the context of global oxygen declining. However, the main focus of this study is on the long-term variations of hypoxia in the PRE and the underlying mechanisms, and due to the data limitations that largely limit our ability to quantify the long-term oxygen changes, it is immature for us at the current stage to have an in-depth comparison between the PRE and other hypoxic systems (please note that comprehensive comparisons between other different hypoxic systems have been conducted in previous studies (e.g., Rabouille et al., 2008; Fennel and Testa, 2019)). Alternatively, we will add some moderate discussions in the first two paragraphs of section 4.3 in our manuscript as follows:

"Apparent long-term expansions of hypoxic conditions have been documented in several coastal systems where sustained seasonal hypoxia has been reported. For instance, the hypoxic volume in the Baltic Sea has expanded dramatically with increasing nutrient inputs from its watershed and enhanced water-column respiration resulting from warming (Fennel and Testa, 2019 and references therein). In the Chesapeake Bay, the hypoxia can be tracked back to the 1930s and has witnessed an expansion of its volume since the 1950s due to the increased nutrient loads (Hagy et al., 2004). Moreover, the hypoxia in the northern Gulf of Mexico has been documented since 1985 (Rabalais et al., 2002). However, models suggest that the occurrence of large-scale hypoxia can be as early as the 1970s (Rabalais et al., 2007). Despite large interannual variability, the hypoxic area has increased from an average of 8,300 km^2 in 1985-1992 to 16,000 km^2 in 1993-2001 (Scavia et al., 2003). To mitigate the hypoxia, nutrient reduction plans have been proposed. In addition to the nutrient loads, the long-term climate change can also exaggerate hypoxia and reduce the positive impacts from nutrient reduction. Modeling studies have suggested that the worsened physical conditions since the 1980s in the Chesapeake Bay, e.g. prolonged vertical exchange time and elevated temperature, can contribute to the increased hypoxia (Du and Shen, 2015; Du et al., 2018). A more recent study shows that the impacts from climate change and nutrient reduction cancel out and therefore the hypoxic volume in the Chesapeake Bay shows no significant long-term trends in the past three decades (Ni et al., 2020). Similar findings have been also archived in other hypoxic systems, e.g. the northern Gulf of Mexico (Kemp et al., 2009; Obenour et al., 2013). However, it has to be noticed that the susceptibility of hypoxia to increased anthropogenic activities varies across different coastal systems due to their physical and biological features.

It is commonly recognized that the PRE did not develop similar large-scale, persistent low-oxygen zone as in other hypoxic systems (e.g., the northern Gulf of Mexico, the Yangtze River estuary). A combination of intriguing features including shallow and turbid waters, rapid physical exchanges, and unstable vertical stratification provides good buffering capacity for the PRE to mitigate eutrophication and hypoxic conditions in summer…."

Specific Comments:

(1) **Line 98-Line 101:** the measure of low oxygen condition (< 2 mg/L, 3 mg/L and 4 mg/L) should be placed in the material and method section. The potential ecological consequence should also be mentioned.

Response: Thank you for the comment. We will revise it as suggested. As for the potential ecological consequence, it has been mentioned in the first paragraph of the Introduction section (please see lines 41-46).

(2) **Line 116-Line 120:** Using DO saturation state as one of the low oxygen condition measure. The meaning of the new metrics should be better stated. It will be better to state how the PRE hypoxia is different from the Chesapeake Bay and Gulf of Mexico system; therefore, different

measure was taken in the research.

Response: Please note that the oxygen saturation state (DOs, %) in use, defined as the ratio of the in situ oxygen concentration to its saturation level at a known temperature and salinity, is not a new metric we propose. It has been documented in the specifications for marine monitoring (e.g., GB 17378-2007) issued by the National Standard of P.R. China and has also been applied in previous studies (e.g., He et al., 2014; Qian et al., 2018) to represent the state of oxygen deficits, similar to the apparent oxygen utilization (AOU). For example, in the first paragraph of section 2.3.2 in He et al. (2014), they mentioned “The DO saturation (DO%) was calculated from the field-measured DO concentration divided by DO concentration at equilibrium with the atmosphere …”; in the second paragraph of section 3. 2 in Qian et al. (2018), they mentioned “In the winter, DO values were >200 mmol kg^{-1} (>85% saturated) in the lower estuary throughout Transect C. In the spring, surface water DO values increased to >300 mmol kg^{-1} and the DO saturation state reached >120% by May, …”.

As suggested, we will provide further explanations to this metric (DOs) in our manuscript.

(3) **Section 3.1 and Figure 2:** Why not think about show AOU in the analysis?

Response: As we mentioned above, the oxygen saturation state (DOs, %) is also an indicator frequently used to reflect the state of oxygen deficits. In fact, it has a close relation with the AOU: AOU = (1-DOs)*DOsat (please note that DOsat represents the oxygen saturation concentration). According to the reviewer’s suggestion, we have added AOU in Figure 2 (please see the revised figure below) and used for analysis as well in our manuscript.

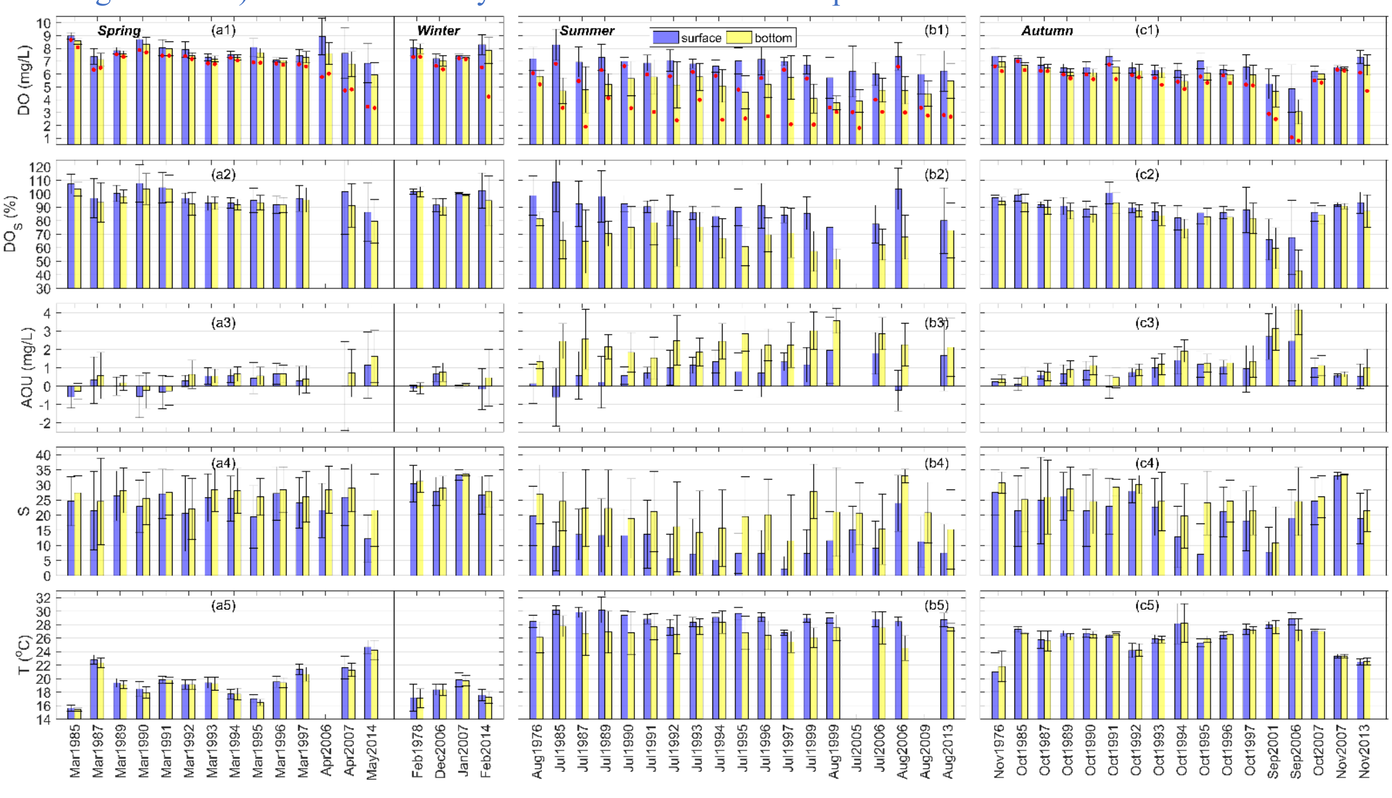

Figure r1. Spatial means and standard deviations of DO concentrations, DO saturation (DOs), apparent oxygen utilization (AOU), salinity (S), and temperature (T) in the surface and bottom waters of the PRE in (a) spring (March-May) and winter (December-February), (b) summer (June-August), and (c) autumn (September-November) during 1976-2014. Note that the red dots in the first row of the figure represent the lowest DO values measured in each time period.

(4) **Line 148:** "The existence of hypoxic events in periods other than summer". The statement was kind of misleading. It seems it only happened in September 2006, not something unified exist. Please emphasize and rewrite.

Response: As suggested, this sentence will be rewritten as "*This reveals the existence of potential hypoxic events in periods other than summer*".

(5) **Line 165:** "the observed areas" and the following area number reported. The software used for the plots, and interpolation method to generate the low oxygen area should be well reported in the method section.

Response: The software we used for plotting includes MATLAB and EXCEL. As for the estimation on the low-oxygen areal extents in the PRE, our processing procedure is as follows: firstly, we divided the sea area of the PRE into a number of grid cells with a resolution of 0.01°, and then used the scattered-data-interpolation method (namely, the 'scatteredInterpolant' function) provided by MATLAB to interpolate the observational oxygen data onto the grid cells; secondly, we calculated the total areas for all the grid cells being hypoxic (with DO < 2 mg/L) to estimate the hypoxic areas. Same procedures were applied to compute the areal extents for oxygen deficiency and low oxygen by using a DO threshold of 3 and 4 mg/L, respectively.

As suggested, we will provide the above information on the software used for plotting and the interpolation method to estimate the low-oxygen areas in our manuscript.

(6) **Line 175:** I am confused about the statement "of which 1997, 2006 and 2013 have been shown earlier and will not be repeated here" please rewrite and clarify.

Response: As suggested, this sentence will be rewritten as "*note that the distributions in 1997, 2006 and 2013 have been shown in Figures 3-4 and will not be repeated here*".

(7) **Line 180:** This is a very interesting phenomenon reported. Figure 11a should be cited here also.

Response: The phenomenon reported in line 180 is about the low-oxygen levels in the surface waters observed in July 2005 and August 2013. We guess that what the reviewer actually wants to suggest is to cite Figure 2b1, not Figure 11a (please note that this subplot shows the wastewater discharge, not the oxygen data). Accordingly, we will add a citation of Figure 2b1 here.

(8) **Line 266-269:** The explanations of Figure 7b1 and 7b2. This was also because of the convergence induced by cyclonic vortices in the coastal transition zone (CTZ). Please add

some discussions.

Response: Thank you for the comment. We will add some discussions on the convergence induced by cyclonic vortices in the coastal transition zone (CTZ).

(9) **Section 4. Discussion.** I would expect to see a discussion on comparing long-term trend hypoxia variability with other systems, including both Chesapeake Bay and Gulf of Mexico. Please add section in this part.

Response: We will revise it as suggested. Please see our response to the General Comment 3 for details.

(10) **Table 2:** The definition of Pearson correlation coefficient should be explained in the method section. The correlation with NH4, NO3, PO4, is it with the nutrient concentration or with the loading? The details like this should be provided.

Response: The correlation analysis was performed on the oxygen and nutrient concentrations. As suggested, we will explain the definition of Pearson correlation coefficient in the method section of our manuscript, and will make it clear that the correlation analysis was carried out on the oxygen and nutrient concentrations in Table 2 of our manuscript.

(11) **Figure 10:** why the comparison was done between July 1999 and Sep 2006 in this figure? different year and different season. The pure bottom dissolved oxygen concentration should also be placed along with other variables

Response: Here we intended to use a combination of physical factors (including salinity and vertical density gradient) and biochemical-related factors (including the oxygen saturation states, SSC, and chlorophyll concentrations) to illustrate the differences between the summer and early autumn in terms of the mechanisms and key factors controlling the occurrence of low-oxygen conditions in the PRE. The reason for comparing these seasonal data in different years is simply due to the data limitations (a lack of long-term chlorophyll data). Currently, we only have chlorophyll data in July 1999 and September 2006 on hand (as listed in Table 1 of our manuscript). In spite of the data limitations, Figure 10 did show the distinct patterns of physical environments, SSC and chlorophyll distributions and their plausible effects on the low-oxygen conditions for different seasons (i.e. the summer and early autumn).

In regard to the bottom oxygen concentrations in July 1999 and September 2006, please note that they have already been presented in Figures 6d1 and 8b2. Therefore, in order to avoid the repeated display of the same information, the DO distributions were not added into Figure 10.

(12) **Figure 11:** Please provide a nutrient loading figure along with other variables.

Response: Thank you for the comment. We agree that it will be helpful to further explore the link between the long-term expansion of low-oxygen conditions and the environment changes in the Pearl River region by incorporating the nutrient loading (if applicable) along with river discharge and sediment load into Figure 11. Unfortunately, the long-term nutrient loading data are not available. Instead, we provided the nutrient concentrations near the eastern four river outlets along with the wastewater discharge to reflect the pressure of anthropogenic pollutant inputs. Please see the revised figure (Figure r2) below (please note that the estimated areas of low-oxygen conditions during 1985-2017 were also added into the figure as suggested by the reviewer #1). There was an increasing trend in the nutrient concentrations along with the wastewater discharge. Although there existed data gaps in certain years, it is still clear that the nutrient concentrations after 2000 are higher than those before. This finding is also supported by Li et al. (2020). We have cited their findings regarding the changes in nutrients in our manuscript (lines 361-364).

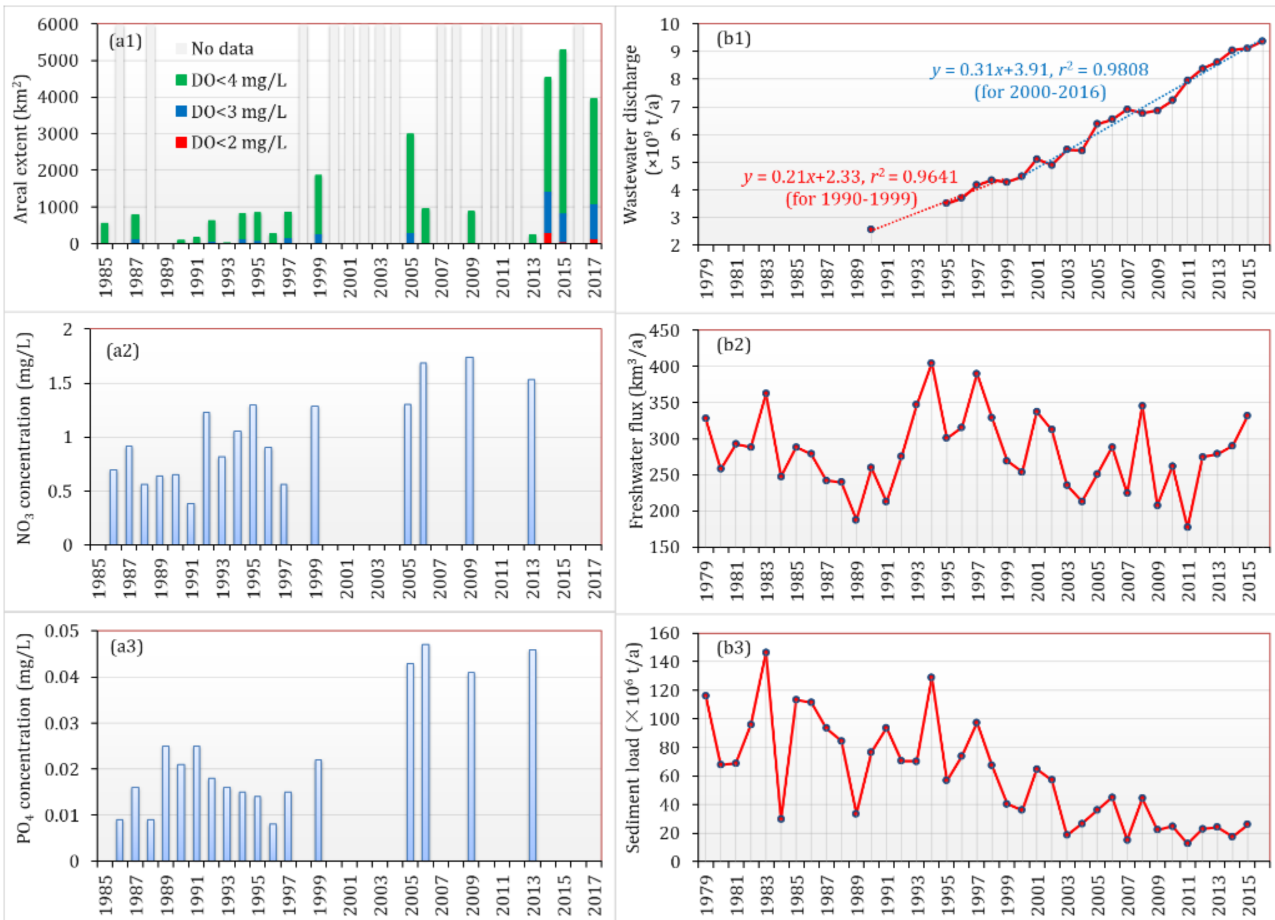

Figure r2. (a1) The estimated area extents of low-oxygen conditions in the bottom waters of the PRE and the (a2) NO_3 and (a3) PO_4 concentrations near the eastern four outlets in summer during 1985-2017. (b1) Annual wastewater discharge in Guangdong Province during 1990-2016. The data before 1998 were taken from Li et al. (2020), and the remaining data were obtained from the Environmental Statistics Bulletin published by the Department of Ecology and Environmental of Guangdong Province (http://gdee.gd.gov.cn/tjxx3187/index.html). (b2) Freshwater discharge and (b3) sediment load of the Pearl River from 1979 to 2015, adopted from Wu et al. (2020).

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