Biogeosciences Discuss., referee comment RC2
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Review Comment on bg-2020-480

Anonymous Referee #2

Referee comment on "Long-term spatiotemporal variations in and expansion of low-oxygen conditions in the Pearl River estuary: a study synthesizing observations during 1976–2017" by Jiatang Hu et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2020-480-RC2, 2021

Overall quality

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.

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.

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.

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.

Specific comments:

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.

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

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

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.

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

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

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

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.

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.

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.

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

Figure 11: Please provide a nutrient loading figure along with other variables.

Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2020-480/bg-2020-480-RC2-supplement.pdf