We wish to thank the editor and reviewers for their constructive comments and suggestions, which have significantly improved our manuscript. We have carefully revised the manuscript accordingly. Detailed response to all comments is given below (the reviewer's comments are shown in black, while authors' response is marked in blue, the text in the manuscript is marked as italics, and the text revised is highlighted in yellow).

Response to comments from Referee#1

Line64: As the author addressed in the "Response", it is better use "increased frequency" instead of "increased intensity".

We have revised it as advised.

Line77: Put "In addition," before "human activities". And use "Hence" instead of "In addition" on line79.

We have revised it as advised.

Line161: Please check it is online coupling or offline coupling between the 1D and the 3D models. It should be offline as on line167.

We appreciate the reviewer's comments. The 1D and the 3D models are online models. The 1D and 3D models are run simultaneously. During the simulation, the 1D model supplies discharge information to the 3D model, while the 3D model returns water level data to the 1D model, enabling dynamic two-way coupling between the systems. We have corrected the sentence as: "*…operating in an online coupling mode…*"

Line288-290: Please add a citation here to indicate the stable concentration of the riverine OM.

We appreciate the reviewer's comments. We have added a reference for the riverine OM: … *compared to the marked increases in nutrients and decreases in DO (Lai et al., 2022), indicating stable oxygen-consuming…*

Line263: Why the authors conduct "Projections to the 2020s"? It seems unnecessary.

We apologized for the typos. The term "2020s" was incorrect and has been revised to "2010s" (our study period). The corrected sentence now reads:

"*For the 2010s, simulated HA3 doubled to 617.2 km², consistent with observational uncertainty ranges (901±591 km²; 2013-2017 data), …*"

How about the initial and boundary conditions (including physical and biological fields) in these model experiments? It is better to add their descriptions.

We appreciate the reviewer's comments. We add the description in Supporting Information, L65*: The conditions at the open boundary were set to fixed values: salinity at 35 PSU, phytoplankton at 0 μg/L, and nutrients at 1.0 × 10^{-5} mg/L.*

Line288-290: Full description of COD and OM are needed when they first emerge.

We have revised it as advised:

Long-term monitoring at river outlets showed no significant temporal trend in chemical oxygen demand (COD) compared to the marked increases in nutrients and decreases in DO, indicating stable oxygen-consuming organic matters (OM) inputs.

Line532: It should be "increase" not "decrease".

We have revised it as advised.

Line534: I don't think "expanding vertically by ~1-4 m and (Table 3)"-line534 is concluded from Table 3.

Thank you for pointing this out. We have made corrections as follows:

Low-oxygen events also become more persistent, lasting longer (~15-35 days during June-August) and expanding vertically by ~1-4 m (Fig 5.g-h).

The authors are advised to further polish the language. Some sentences are confused, for example "Our model revealed distinct temporal shifts in summertime DO patterns and hypoxia distribution" on line393.

We appreciate the reviewer's comments. During the early stages of manuscript preparation, the draft was polished with input from co-authors, including Jiatang Hu and Bin Wang, both of whom have academic and professional experience in English-speaking countries. We carefully reviewed the manuscript and refined several expressions—for example: "*Our model revealed distinct shifts in summertime DO patterns and hypoxia distribution (Fig. 5).*"

Response to comments from Referee #2

I appreciate the efforts made by the authors to respond to the extensive review comments. They have added many important new details. I found that 2 of the responses to my comments were inadequate, and I think I found an error in a new figure. Please see below:

(1) Figure 4 gives negative numbers for changes in the euphotic depth. The label says "2010s-1990s", where a negative value would indicate a higher value for the 1990s relative to 2010s. I think you concluded the opposite, including in your graphical abstract. Is this a typo?

Thank you for pointing this out. We confirm that the negative values in Figure 4 are indeed correct and result from our consistent depth convention throughout the study.

In our analysis, all depth values are defined as negative numbers increasing downward from the sea surface (z=0). Therefore, when we calculate the difference between decades (2010s minus 1990s), a more negative value indicates a deeper euphotic zone in the 2010s compared to the 1990s. This convention aligns completely with the data presented in Table 2 and Figure 4b, d.

To improve clarity, we have explicitly stated this depth sign convention in the caption of Figure 4 in the revised manuscript:

Note: Euphotic depth is measured as negative values increasing downward from the sea surface; thus, more negative differences in (f) indicate deeper light penetration in the 2010s.

We also added the following note in Table 2:

The euphotic depth is measured as negative values increasing downward from the sea surface.

(2) In my first review, I made this comment:

"I think the authors point about the importance of turbidity in influencing hypoxia in particular is an interesting finding, and worth highlighting. But I do think they authors overstate it's potential importance in other estuaries. The changes in sediment concentrations in the PRE were quite high, and I am not convinced that similar changes have occurred in many other hypoxic systems within the time of hypoxia expansion. The text on line 554-570 is helpful, but it would be better if hypoxia-specific details from the literature could be added to the discussion."

The edits made by the authors on Lines 597-617 add examples of the potential for their argument to be more widespread, and they are helpful to the manuscript. But on line 583, this sentence is written:

"This oversight suggests that previous studies may have overestimated nutrient impacts when failing to account for SSC-mediated processes required to align model simulations with observed deoxygenation patterns."

The authors do not present any evidence to support this statement, and the many models I am aware of account for interannual changes in TSS in their simulations. It is alternatively possible that the lack of a recognized sediment effect is that other systems have not had such a large change in sediment loading over the past timeframe, either due to lower overall sediment loads or no comparable changes in sediment loads because land use changes have not been as drastic as in the examples given from regions where land use changes have been rapid and extreme. I just think this language needs to recognize the context of the PRE example (huge sediment load changes) and that just because these authors found significant SSC effects on light, productivity and hypoxia, that does not mean that models in other systems have erroneously compensated for SSC effects.

We sincerely appreciate the reviewer's thoughtful comments, which have significantly strengthened our manuscript. We have carefully considered the feedback and revised the text accordingly.

Regarding the concern about overstating the global importance of SSC effects, we agree with the reviewer that these findings are particularly relevant to systems like the Pearl River Estuary (PRE) that have experienced dramatic sediment declines. In response, we have revised Section 4.2 to better contextualize our results. The updated discussion now emphasizes that SSC-mediated hypoxia effects are most pronounced in systems with high baseline sediment loads and rapid anthropogenic declines, such as the PRE and Yangtze River Estuary. We have removed generalized statements about previous models and instead focus on providing system-specific recommendations for estuaries experiencing similar sediment reductions.

Concerning the evidence for model overcompensation, we have modified our language to more carefully frame these conclusions. Furthermore, we highlight documented cases of SSC changes in other systems while acknowledging that effects may differ in estuaries with different trends in sediment loads.

The revised discussion (in Section 4.2) now reads as follows (since the entire section has been substantially revised, we have not highlighted the new text in yellow):

"*Our results highlight substantial spatial variability in how riverine inputs influence deoxygenation, emphasizing the need for more targeted strategies to mitigate hypoxia. While the effects of riverine nutrients on hypoxia have been widely studied, the role of SSC in modulating eutrophication and hypoxia has received comparatively less attention. This is particularly relevant in systems like the PRE, which has experienced a dramatic 60% decline in sediment since the 1980s due to dam construction and land-use changes.*

In the PRE, our model simulations demonstrate that SSC-mediated light limitation critically influences deoxygenation dynamics. When SSC declines are omitted, model simulations overestimate nutrient-driven productivity and underestimate hypoxia expansion. This suggests that for systems experiencing pronounced sediment reductions, overlooking SSC effects could lead to overly optimistic assessments of nutrient control efficacy. It is therefore critical to disentangle the relative contributions of riverine nutrients versus SSC changes to coastal deoxygenation trend. As demonstrated in the PRE, the current low-SSC environment suggests that more stringent nutrient reductions might be required to effectively curb deoxygenation compared to conditions with higher SSC.

Although dam constructions in the Pearl River Basin, mostly completed before the 2000s, have driven significant declines in SSC, the future trends of riverine SSC remain uncertain. For instance, recent reforestation efforts have effectively reduced summer freshwater discharge and sediment load in the Pearl River Basin (Cao et al., 2023). This evolving situation underscores that changes in SSC will continue to shape future oxygen dynamics, introducing compounding uncertainties for hypoxia mitigation.

Similar relationship between SSC and eutrophication or hypoxia have been observed in other systems facing rapid anthropogenic changes. For example, the Yangtze River Estuary has seen a ~56% decrease in SSC over the past decades, which has been linked to a 61% increase in Chl a concentration, indicating intensified eutrophication (Wang et al., 2019). In addition, several modelling studies have shown that dam constructions in the upper regions of the Guadiana Estuary have led to reduced water turbidity and exacerbated eutrophication in the lower estuary (Domingues et al., 2012; Barbosa et al., 2010). A global survey revealed that sediment loads in 414 major rivers have decreased by approximately 51% since the 2000s due to human activities (Dethier et al., 2022). This trend highlights the need for further investigation into how sediment declines impact eutrophication and deoxygenation on a global scale.

It is also important to recognize that human activities can increase sediment loads in estuaries. For example, land-use changes such as deforestation (Kasai et al., 2005) and industrialization (Syvitski and Kettner, 2011) can exacerbate soil erosion and sediment transport, leading to higher suspended sediment concentrations in the water. In such cases, light attenuation due to increased turbidity may suppress phytoplankton growth and reduce primary production, potentially mitigating hypoxia. Therefore, the effects of SSC are system-specific and depend on the direction and magnitude of sediment trends."

We also revised the tone of the final sentences in both the Abstract and the Conclusion to more clearly articulate the management implications.

In the Abstract, it now reads:

"…*Our results revealed that SSC declines, by improving light availability for productivity, play a larger role than nutrient increases in exacerbating deoxygenation off the PRE. This synergy complicates hypoxia mitigation efforts focused solely on nutrient controls. Given the widespread global declines in riverine suspended sediments, our findings underscore the importance for incorporating sediment-mediated processes—a relatively overlooked factor—in coastal deoxygenation studies."*

In the Conclusion section, it now reads:

"…*Our study demonstrates that declined suspended sediments have significantly exacerbated low-oxygen conditions off the PRE, with effects that synergistically intensify when combined with increasing nutrient loads. These findings highlight the need for dual-control strategies addressing both nutrient inputs and sediment-mediated light availability in coastal management. Give the global declines in riverine suspended sediments, we emphasize that effective hypoxia mitigation requires integrated approaches accounting for these interacting drivers."*

(3) In my first review, I emphasized how important a validation of the computed photic depths or kd values were to the conclusions of this paper. The authors stated that no such validation data are available. I find this difficult to believe in a system that has had many research cruises in recent decades. Also, the fact that Wang et al. (2018) were able to validate other biogeochemical variables doesn't replace a validation of the light fields in the model.

We appreciate the reviewer's comment on validation. In the revised version, we have now validated the euphotic depth using Sentinel satellite data provided by the EUMETSAT Ocean Color Thematic Assembly Centre. The validation steps are as follows:

1) We obtained diffuse attenuation coefficient at 490 nm (Kd(490)) data for the PRE region from the EUMETSAT Ocean color Thematic Assembly Centre (https://www.oceancolour.org/);

2) We estimated photosynthetically active radiation attenuation coefficient (K_{par}) from $K_d(490)$ using the empirical formula proposed by Lee et al. (2005), and calculated the euphotic depth based on the estimated K_{par};

3) The satellite-derived euphotic depth was then compared with the results obtained in this study. The comparisons are shown in the modified Figure 4.

The validation results show good agreement between our model-simulated euphotic depths and the satellite-derived estimates (see modified Fig. 4). In addition, to enhanced clarity, we have integrated the model-data comparisons of nutrient, chlorophyll-a, and oxygen concentrations (previously Supplement Fig. S1) directly into the Figs. 3 and 5. In the main text, we have added the following in Section 2.2.2 Model validation:

"For validation, comparisons with the water quality data from cruise surveys indicated that the biogeochemical module was robust to reproduce the spatial distributions of nutrient, chlorophyll a, and oxygen concentrations in the PRE (Figs. 3, 5). To further assess light attenuation dynamics, we obtained diffuse attenuation coefficient at 490 nm (Kd(490)) data for the PRE from the EUMETSAT Ocean color Thematic Assembly Centre (https://www.oceancolour.org/). We converted Kd(490) to photosynthetically active radiation attenuation coefficient (Kpar) using the empirical formula proposed by Lee et al. (2005), and then calculated the euphotic depth using Eq. 5 in Section 2.2.1. Model-simulated and satellite-derived euphotic depths show close agreement in both the 1990s and 2010s (Fig. 4b, d), demonstrating consistent model performance across decades."

We also revised the following statements in Section 3.1.2:

"In contrast, light limitation attenuated along the river plume transport pathway (Fig. 4b), largely ascribed to the decreasing SSC (Fig. 3a-b). Both observations and model simulations revealed consistent spatial patterns, with the eutrophic depth progressively increasing from severely light-limited regions near river outlets (Lingdingyang Bay: 1.3 m; Modaomen sub-estuary: 9.5 m) to the less-limited Hong Kong waters (20.7 m; Fig. 4b)."

"…Both model simulations and observations revealed significantly greater deepening of the euphotic depth in the Lingdingyang Bay compared to the lower estuary (Fig. 4b, d, f)…"

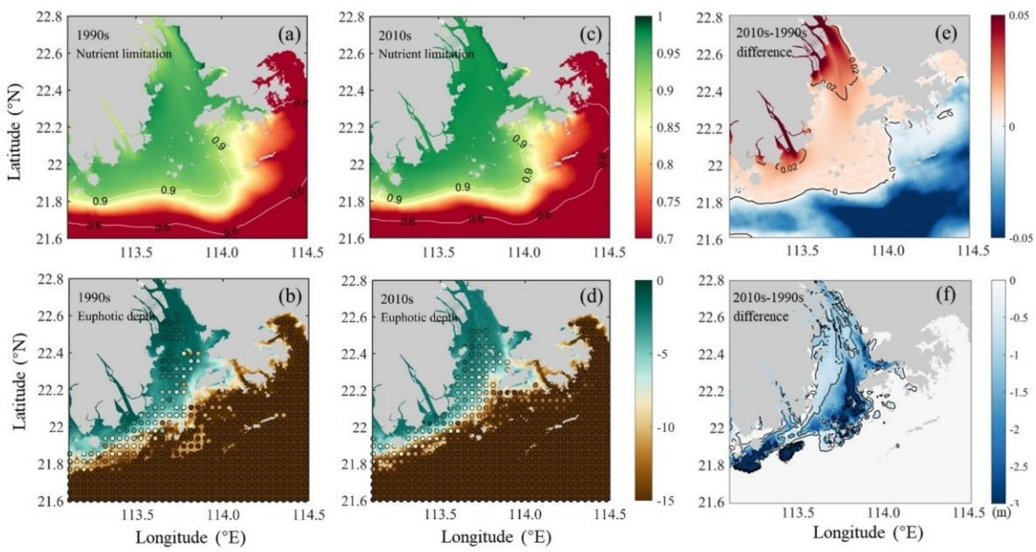

Fig. 4. Comparisons of (a, c) simulated nutrient limitation index for phytoplankton growth and (b, d) euphotic depths (in meters) between the 1990s and the 2010s, with their differences shown in (e) and (f), respectively. Colored dots in (b) and (d) represent corresponding euphotic depth observations. Note: Euphotic depth is measured as negative values increasing downward from the sea surface; thus, more negative differences in (f) indicate deeper light penetration in the 2010s.

Reference

Lee, Z.-P., Du, K.-P., and Arnone, R.: A model for the diffuse attenuation coefficient of downwelling irradiance, Journal of Geophysical Research: Oceans, 110, https://doi.org/10.1029/2004JC002275, 2005.

Response to comments from Referee #3

The authors answered most of my comments, and the manuscript will be suited for publication after the following minor comments are addressed.

Please note that the line numbers I provide refer to the track-changes document.

Abstract

I strongly suggest that the authors provide a quantitative number for the impact of not only DO input (44%) but also SSD and nutrient input. These numbers should also be provided in the conclusion. For instance, at L42, we could read "… in the lower reaches, accounting for XX of the observed change.".

We appreciate the reviewer's comments. In the revised version, we have incorporated the relevant information into the abstract (note that we have polished the language in the entire abstract to enhance clarity):

"…*Single-factor experiments suggested that decreased riverine DO content (46%) alone expanded low-oxygen areas in the upper estuarine regions by 44%, the decreased SSC (by 60%) alone cause a 47% expansion in the lower reach of PRE, and the increased nutrients alone (100% in dissolved inorganic nitrogen and 225% in phosphate) drove a 31% expansion. In comparison, the combined nutrient increases and the SSC declines synergistically enhanced primary production and bottom oxygen consumptions (dominated by sediment oxygen uptake), amplifying low-oxygen (104%) and hypoxic (192%) area growth in lower estuaries. …"*

And in the Conclusion, we have revised as follows:

"…*Single-factor experiments a 46% decrease in riverine DO alone expanded low-oxygen areas by ~44% in the upper PRE, a 60% SSC reduction alone caused a 47% expansion in the lower PRE, and nutrient increases alone (100% DIN, 225% phosphate) drove a 31% expansion. By comparison…"*

L42: More compared to what? Please specify.

We appreciate the reviewer's comments. We have revised the sentences as follows:

"*Our results revealed that SSC declines, by improving light availability for productivity, play a larger role than nutrient increases in exacerbating deoxygenation off the PRE. …"*

L141: "level" is repeated twice in the sentence. Remove the second occurrence.

We have revised it as advised.

Fig. 1 caption:

1. The authors should specify that the points indicate the yearly mean from the different

river outlets. This raises another question. If it is the mean of only a few points, are there always the same number of data points from the same rivers? Otherwise could some of the observed variability be due to variations in which rivers are sampled?

We appreciate the reviewer's comment. The DIN, DIP, and DO data presented are from the Humen Outlet exclusively, as it is the only Pearl River outlet with consistent monitoring spanning the 1990s-2010s period. While the number of sampling data points in summertime varied annually (now clarified by adding standard deviation bars in the revised Fig. 1), Humem Outlet maintains the longest continuous monitoring record, enabling robust trend analysis.

We have added the following statement in the caption of Figure 1 to make it clearer. *"…(d-f) Summer-averaged (June to August) surface-layer (within the upper 2 m of water column) concentrations of nutrients (DIN, DIP) and dissolved oxygen (DO) at Humen Outlet (the primary monitoring site). Error bars indicate intra-summer variability across sampling dates. Data sources are provided in Table S1."*

2. It is currently not indicated where the data is from. The authors should indicate that the data source is provided in Table S1.

We appreciate the reviewer's comment. We have added this information to the caption of Figure 1:

"*Data sources are provided in Table S1.*"

3. On panel a, units are missing for depth.

We appreciate the reviewer's comment. We added the unit of depth (m) in Fig. 1a.

L151-153, "After the 2000s, … sediment loads (Fig. 1c-e)" repeats the sentence right before. Please consider combining the two.

We appreciate the reviewer's comment. We revised this paragraph as follows:

"*During the 1990s, the Pearl River Estuary (PRE) experienced low eutrophication levels, consistent with limited upstream urbanization and relatively high sediment loads. This period saw extensive construction of water infrastructure—mostly completed by 2000—including over 8,636 reservoirs in the Pearl River Basin (Wu et al., 2016), which drove a significant decline in riverine suspended sediment concentration (SSC) (Zhang et al., 2008). After 2000, accelerated urbanization and continued hydraulic development further altered river inputs, with monitoring data showing decreased sediment loads (Fig. 1c) and increased nutrient concentrations (Fig. 1d–e). These changes collectively enhanced phytoplankton bloom potential, exacerbating eutrophication and hypoxia.*"

Fig. 2: The authors should indicate somewhere how the area was calculated, and indicate which type of interpolation was used (DIVA, linear, etc), as this will influence the number and is necessary for repeatability. This can be a short sentence, for instance "… area (HA4, DO < 4 mg/L, calculated based on a XX interpolation of oxygen on a 0.01° × 0.01° grid) and …"

We appreciate the reviewer's comment. We have revised the caption of Fig. 2 as follows:

Fig. 2. (a) Interannual variations of low-oxygen area (HA4, DO < 4 mg/L) and hypoxic area (HA3, DO < 3 mg/L) in the bottom waters (~1-2 m above sediments) of the PRE, calculated via liner interpolation on a 0.01° × 0.01° grid using summer cruise observations (note that the grey patches indicate data gaps)...

This is probably my most significant comment. Since the paper is already quite long, and since the model has been used and therefore described in previous papers, I strongly suggest that the authors shorten section 2.2.1 on the model description, and refer the reader to previous papers describing the model. Alternatively, they could move some of the text to the supplementary material, but I believe that simply pointing to another paper would be a better option. More specifically, I suggest that the authors remove L190-219 ("As for the oxygen dynamics, […] phytoplankton biomass (mg C L-1)."). I suggest they keep the introduction of the light limitation factor and of the nutrient limiting factor since they calculate them, but shorten the text in L220-247.

We appreciate the reviewer's comments. Based on the suggestions, we have revised the section '2.2.1 Model descriptions and settings' as follows:

We deleted the original L191-205 in the revised manuscript. Instead, we have added the following reference:

The schemes for DO dynamics and phytoplankton growth in the model can be found in previous studies (Wang et al., 2017; Wang et al., 2018).

For L220-247, we revised as follows:

The nutrient limitation factor ($G_N(N)$)is parameterized as:

$$G_N(N) = Min\left(\frac{DIN}{K_{mN}+DIN}, \frac{DIP}{K_{mP}+DIP}, \frac{Si}{K_{mSi}+Si}\right) \quad (1)$$

where DIN, DIP, and Si are concentrations (mg L^{-1}) of dissolved inorganic nitrogen (NO_3^-, NH_4^+), phosphorus (PO_4^{3-}), and silicon (SiO_3^{2-}), respectively. K_{mN}, K_{mP}, and K_{mSi} are their corresponding half-saturation constants. A higher $G_N(N)$ indicates weaker nutrient limitation. Given the stronger N and P limitation compared to Si in the Pearl River Estuary (PRE), this study emphasizes N and P.

The light limitation factor $G_I(I)$ *is parameterized as:*

$$G_I(I) = \frac{e}{k_e H}\left[exp\left(\frac{-I_0(t)}{I_s}e^{-k_e H}\right) - exp\frac{-I_0(t)}{I_s}\right] \qquad (2)$$

with the light extinction coefficient:

$$k_e = k_{ebase} + k_c * a_{cchl} * P_c + k_{sed} * SSC + k_{POC} * POC \qquad (3)$$

and the surface light at depth:

$$I_0 = I_{surf} * e^{-k_e * H} \qquad (4)$$

Here, H *is water depth (m),* I_s *the saturation light intensity (ly day*$^{-1}$*),* I_{surf} *is the surface light (ly day*$^{-1}$*), and the k-terms are light attenuation coefficients due to water, Chl a, SSC, and POC.*

To assess light conditions, the eutrophic depth H_E *is computed as the depth where light is 1% of surface intensity:*

$$I_{surf} * e^{-k_e * H_E} = I_{surf} * 1\% \qquad (5)$$

A deeper H_E *implies better light conditions for phytoplankton growth.*

To ease the read and shorten the paper, I suggest replacing L295-301 ("Specifically in the summertime […] between the 1990s and the 2010s (2.5 times).") and the numbers in L305 ("organic carbon: 2 mg/L ...") with simply a reference to Table 1, where the readers interested in the specific numbers can find them.

We appreciate the reviewer's comments. In the revised version, we make several changes in this part. It now reads as follows:

The riverine boundary conditions for DIN, DIP, DO, and SSC during summertime are listed in Table 1 for both the 1990s and 2010s cases. Notably, the 2010s SSC was set based on field data (Chen et al., 2020), whereas the 1990s SSC was back-calculated from the 2.5-fold difference in sediment loads.

About the links to the data, the authors should provide the exact link where the data can be downloaded, instead of the general links to the agencies providing the data. This is essential for repeatability and to respect FAIR principles for data availability.

We appreciate the reviewer's comments. In the revised version, we have updated the relevant links.

Table S1. Data source for long-term water quality parameters in the Pearl River Estuary (PRE).

Data	*Year*	*Source*
Bottom DO	*1985-2013*	*Hu et al., 2021 (DOI: 10.5194/bg-18-5247-2021)*

Bottom DO	*2014*	*Su et al., 2017 (DOI: 10.5194/bg-14-4085-2017)*
Bottom DO	*2015&2017*	*Li et al., 2021 (DOI: 10.1029/2020JC016700)*
Bottom DO	*1990-2017*	*Environmental Protection Department of Hong Kong (https://cd.epic.epd.gov.hk/EPICRIVER/marine/)*
Surface DIN	*1990-2013*	*Hu et al., 2021 (DOI: 10.5194/bg-18-5247-2021)*
Surface DIN	*2017*	*Chen et al., 2020 (DOI: 10.1029/2019JG005596)*
Surface Chl a	*2015&2017*	*Li et al., 2021 (DOI: 10.1029/2020JC016700)*
Riverine DIN, DIP, DO	*1990-2017*	*Hu et al., 2021 (DOI: 10.5194/bg-18-5247-2021); Department of Ecology and Environment of Guangdong Province (https://gdee.gd.gov.cn/hjjce/jahy/index.html)*
Riverine SSC	*1990-2017*	*China River Sediment Bulletin (http://www.mwr.gov.cn/sj/tjgb/zghlnsgb/)*

The Data Availability statement has been revised as:

The dissolved oxygen observation datasets off the Pearl River Estuary were obtained from published studies (Hu et al., 2021, DOI: 10.5194/bg-18-5247-2021; Su et al., 2017, DOI: 10.5194/bg-14-4085-2017; Li et al., 2021, DOI: 10.1029/2020JC016700; Chen et al., 2020, DOI: 10.1029/2019JG005596) and the Hong Kong Environmental Protection Department (https://cd.epic.epd.gov.hk/EPICRIVER/marine/). The observed nutrients, oxygen, and suspended sediments data in the Pearl River are available from Hu et al. (2021) and publicly accessible databases maintained by Department of Ecology and Environment of Guangdong Province (https://gdee.gd.gov.cn/hjjce/jahy/index.html) and the China River Sediment Bulletin (http://www.mwr.gov.cn/sj/tjgb/zghlnsgb/)

L646: Please specify more important compared to what.

We appreciate the reviewer's comments. In the revised version, we make the sentence clearer:

"*In the PRE, the riverine SSC reduction played a more important role in driving the long-term low-oxygen expansion than nutrient increase.*"

I could suggest to include a sentence on the non-linearity of the response to sediment and nutrient changes (the fact that they are not additive), since this has important implications on our capacity to predict deoxygenation in coastal areas, and a nice conclusion from this work is that we have to consider them together and not separately.

We appreciate the reviewer's comments. This non-additivity is also one of the key points the study aims to highlight. In the revised version, we have made the following changes:

1) After introducing this non-additive phenomenon in Section 4.1 of the manuscript, we added the following:

L567: *This non-additive characteristic underscores the need for integrated management approaches that simultaneously address both nutrient loads and suspended sediment-mediated light conditions.*

We have also included this point in the revised Conclusion section.

"…*Our study demonstrates that declined suspended sediments have significantly exacerbated low-oxygen conditions off the PRE, with effects that synergistically intensify when combined with increasing nutrient loads. These findings highlight the need for dual-control strategies addressing both nutrient inputs and sediment-mediated light availability in coastal management. Give the global declines in riverine suspended sediments, we emphasize that effective hypoxia mitigation requires integrated approaches accounting for these interacting drivers.*"