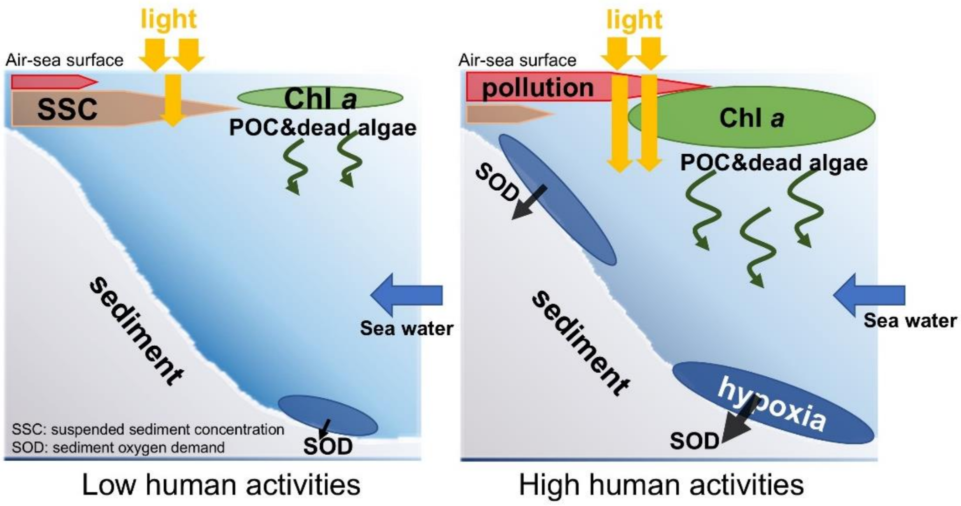
We wish to thank the reviewer for the constructive comments and suggestions. We have carefully revised the manuscript according to the reviewers' comments. Detailed response to all comments is given below (the reviewer's comments are shown in black, while authors' responses are marked in blue. Text from the manuscript is italicized, and the proposed revision are highlighted in yellow).

Reviewers' comments

Reviewer #2: The authors provide a quantitative analysis of various impacts on hypoxia in the Pearl River estuary. I think this is a worth topic and a nice analysis, but some key aspects of the model need to be better described and the discussion could better cite the global literature. Some general and specific comments are provided below:

Graphical abstract: The way you have included light in this document, it gives the impression that the amount of light forced on the water has gone up. I think you should express this as the same downward light flux to the surface, but more light reaching into the water to fuel additional primary production

Response: We will revise the graphical abstract to better illustrate the light penetration as follows:



Abstract Figure. Comparative schematics of oxygen dynamics under low human influence (left) and high human influence (right) conditions. Note that pollution denotes inputs of DIN, DIP and low-oxygen water from anthropogenic discharge.

General Comments:

1. What is the 1D part of the 1D-3D model?

Response: The 1D component of the model is described in detail in Text S1 of the Supplementary Materials as follows：

"The 1D component for the Pearl River network adopts a Preissmann implicit scheme and an iterative approach to solve the Saint-Venant equations of water mass and momentum conservation. A salinity transport module was also incorporated. The river network was discretized into 299 reaches and 1726 computational cross sections,

with five upstream boundaries (including the Gaoyao, Shijiao, Boluo, Laoyagang, and Shizui hydrological stations). The Gaoyao, Shijiao, and Boluo sites were forced by real-time river discharge data, while the Laoyagang and Shizui sites were forced by real-time water level data. The riverine salinity at the upstream boundaries was set to zero. The initial conditions of water levels and salinity in the 1D model domain were set to zero."

To make it clearer, we will add the following text in Section 2.2.1 Line 160:

"*... and oxygen content (Fig. 1c-f). This 1D-3D coupled model integrates the Pearl River network (1D) and the Pearl River Estuary/shelf region (3D). The 1D component solves Saint-Venant equations using a Preissmann scheme, discretizing the river into 299 reaches with five upstream boundaries (discharge/water level inputs). The 3D component employs the ECOM model with 16 vertical layers and adaptive horizontal resolution (400m-3km), forced by tides, atmospheric data, and observed salinity/temperature profiles. Both domains exchange boundary conditions at eight river outlets: the 3D model receives 1D river discharge, while the 1D model uses 3D water levels for downstream boundaries at each time step. Originally developed for nutrient flux studies, the framework has been extended to hypoxia and sediment-nutrient exchange research."*

2. Figure 4 should have a difference plot like figure 3 and 5. This is especially important given that the differences in these growth factors are far smaller than those variables in figs 3 and 5

Response: We appreciate the reviewer's suggestion regarding the utility of difference plots for visualizing these changes. In response, we will revise Figure 4 to incorporate difference plots that more clearly illustrate the spatiotemporal variations in growth factors. The updated figure is shown below.

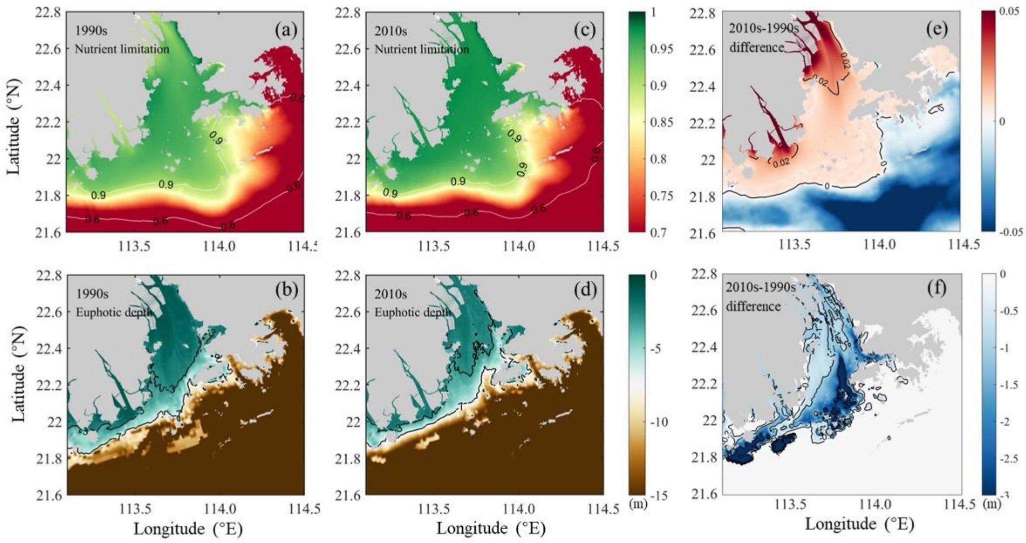


Fig. 4. Simulated distributions (a, c) and difference (e) of nutrient limitation index for the growth of phytoplankton in the 1990s and the 2010s; (b, d) euphotic depth and its difference (e) (in unit of m) in the 1990s and the 2010s.

3. I found the first paragraph of the discussion, lines 488-505 to be simply a repetition of material already presented. I think it could be deleted.

Response: We appreciate the reviewer's valuable suggestion. In revising the manuscript, we have made concerted efforts to streamline this paragraph. However, to ensure clarify for readers and facilitate the subsequent discussion on this study's core finding, the spatial variability in hypoxia drivers across different regions of the estuary, we will retain a concise summary of key results. The revised paragraph now states:

"By integrating long-term observations with physical-biogeochemical model simulations, we revealed significant bottom-water deoxygenation in the Pearl River Estuary over the past three decades, driven by changes in riverine inputs. From the 1990s to 2020s, summer inflows of DIN and DIP increased by ~100% and ~225%, while SSC decreased by ~60% due to human activities like dam construction (Liu et al., 2022) and reforestation (Cao et al., 2023). Concurrently, oxygen depletion from terrestrial pollutants reduced riverine DO concentrations by 46% (Ma et al., 2024). These shifts collectively intensified bottom-water low-oxygen conditions in PRE (Fig. 5), with model simulations showing a 148% expansion in summer low-oxygen areas (DO < 4 mg/L) and a 192% decrease in hypoxic areas (DO < 3 mg/L). Low-oxygen events also become more persistent, lasting longer (~15-35 days during June-August) and expanding vertically by ~1-4 m and (Table 3)."

4. 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.

Response: We appreciate the reviewer's recognition of the role of turbidity in modulating hypoxia in the Pearl River Estuary (PRE). We agree that the magnitude of suspended sediment concentration (SSC) decline in the PRE is particularly pronounced compared to many other systems. However, emerging evidence suggests that the impact of sediment-driven turbidity changes on hypoxia may be important in other systems. For example, Dethier et al. (2022) found that dam construction has reduced global river sediment fluxes in the Northern Hemisphere to 49% of pre-dam levels, highlighting that substantial sediment decline has become a globally significant phenomenon, particularly in basins with extensive water infrastructure development. Although direct linkages to estuarine hypoxia remains understudied, regions experiencing both significant sediment declines and eutrophication (e.g., Changjiang River, …) may exhibit similar dual controls on oxygen dynamics. Our findings thus highlight that hypoxia may be governed by more complex mechanisms, with dam-induced sediment declines potentially exerting more far-reaching ecosystem impacts than previously recognized.

In the revised manuscript, we have refined our tone to better reflect the PRE's unique context while suggesting broader implications for comparable estuaries. The relevant discussion will be revised as follows:

Line 540: "*Our results revealed spatial variability in the regulation of riverine inputs on deoxygenation, highlighting the need for more refined and targeted hypoxia mitigation strategies. Compared to the well-documented role of riverine nutrients, the regulatory effects of SSC on eutrophication and hypoxia have received relatively limited attention. 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. Such model overcompensation could lead to potentially optimistic assessments of hypoxia mitigation effectiveness under proposed nutrient control plans. It is therefore critical to disentangle and quantitatively re-evaluate the relative contributions of riverine nutrients versus SSC changes to coastal deoxygenation dynamics over recent decades. As demonstrated in the PRE case study, the current low-SSC regime suggests more stringent nutrient reductions than previously estimated might be required to effectively curb deoxygenation.*"

Line 556: "*This evolving context underscores that SSC variations will continue to play a defining role in future oxygen dynamics, introducing compounding uncertainties for hypoxia mitigation strategies. Notably, analogous sediment-oxygen coupling mechanisms are emerging in other hypoxic systems.*"

5. Paragraph on lines 571-579: Can you report any information about temperature trends that have been seen in the PRE that could be cited here?

Response: Following the suggestion, we will revise the relevant discussion to incorporate the recorded temperature trends from previous work, as follows:

Line 571: "*Several caveats warrant further investigation. First, alongside anthropogenic influences, climate-mediated changes in regional physical conditions (e.g., wind patterns and freshwater discharge) may also modulate long-term deoxygenation trends in coastal systems (Chen et al., 2024). Second, while ocean warming has already been observed in the PRE (Cheung et al., 2021), its synergistic interactions with other deoxygenation drivers remain poorly constrained in the PRE. Third, compounding factors such as sea-level rise (Hong et al., 2020) may introduce additional complexity to hypoxia evolution through cascading ecosystem effects, highlighting a critical need for future research.*"

6. I think my major comment is that the authors need to bring out the validation of their light model into the paper. The entire model analysis hinges on how well the light model performs, and how realistic it is. How was the base kd estimated? Was CDOM considered? Does the model get the photic depth correct? These are essential details that need to be discussed.

Response: We appreciate the reviewer's insightful comments regarding the light model validation. The light attenuation coefficient (kd) in our study follows the empirical formulation of Di Toro (2001), which has been specifically validated for the PRE by Wang et al. (2018). Their work demonstrated that this light parameterization help reproduce the observed biogeochemical conditions in the PRE, supporting the robustness of our approach.

The euphotic depth in our analysis is derived directly from the simulated light field in the RCA model, which is driven by ERA5 solar radiation data. While unfortunately direct observations of photic depths for validating our simulation are unavailable, the model's ability to reproduce observed biogeochemical conditions (presented in Wang et al. (2018)) provides indirect confidence in the model-simulated light fields.

Regarding the CDOM, we acknowledge this as a potential limitation. While CDOM can influence light attenuation, its role in the PRE remains poorly quantified due to limited observational constraints. Given this uncertainty, we followed the established approach of Wang et al. (2018) that successfully reproduced PRE biogeochemistry without explicit CDOM treatment. We agree this warrants future investigation as more data become available and will add this caveat to the revised Discussion.

Line 571: "*Some caveats to our work require further studies. First, our light attenuation parameterization builds upon the empirical formulation of Di Toro (2001), previously validated for the Pearl River Estuary (PRE) through biogeochemical consistency checks (Wang et al., 2018). While this approach successfully captured observed oxygen dynamics in our simulations, two key simplifications require explicit discussion. The exclusion of chromophoric dissolved organic matter (CDOM), implemented due to limited observational constraints in the PRE, introduces potential uncertainties, though its influence is likely subordinate in this sediment-dominated system where mineral particles govern light attenuation. Additionally, our bulk attenuation approach inherently differs from spectral radiative transfer models by neglecting wavelength-dependent processes, a simplification justified by the overwhelming control of suspended sediments on turbidity dynamics. We contend that these methodological choices provide a pragmatically balanced representation for hypoxia studies, prioritizing computational efficiency while maintaining essential physical realism in high-turbidity environments.*

Apart from anthropogenic activities, alterations in regional physical conditions aligning with climate changes could also regulate the long-term deoxygenation in coastal regions …"

7. Following on #6 above, I think the authors need to discuss their light model in the context of other light models out there. Would their result be different if they used a spectral light model that was less empirical?

Response: We appreciate the reviewer's insightful suggestion. As noted in our response to Comment #6, our empirical light model (Di Toro 2001) provides a robust representation of light attenuation in the PRE. While spectral light models can indeed provide more detailed insights by resolving wavelength-dependent attenuation processes, their implementation in the PRE presents two practical challenges: 1) reliable spectral modeling demands comprehensive input data (e.g., spectral absorption coefficients of water, chlorophyll, CDOM, and suspended sediments), which are currently limited for the PRE; and 2) spectral resolution would significantly increase model complexity and computational demands.

In highly turbid systems like the PRE where light attenuation is predominantly governed by suspended sediments, empirical models have been shown to provide reasonable accuracy. Nevertheless, we agree that future comparative studies using spectral approaches would be valuable to evaluate potential biases in empirical models. This represents an important direction for future research as more comprehensive optical datasets become available for the PRE. We will incorporate this perspective in the caveat part of the Discussion.

Line 571: "*Some caveats to our work require further studies. First, our light attenuation parameterization builds upon the empirical formulation of Di Toro (2001), previously validated for the Pearl River Estuary (PRE) through biogeochemical consistency checks (Wang et al., 2018). While this approach successfully captured observed oxygen dynamics in our simulations, two key simplifications require explicit discussion. The exclusion of chromophoric dissolved organic matter (CDOM), implemented due to limited observational constraints in the PRE, introduces potential uncertainties, though its influence is likely subordinate in this sediment-dominated system where mineral particles govern light attenuation. Additionally, our bulk attenuation approach inherently differs from spectral radiative transfer models by neglecting wavelength-dependent processes, a simplification justified by the overwhelming control of suspended sediments on turbidity dynamics. We contend that these methodological choices provide a pragmatically balanced representation for hypoxia studies, prioritizing computational efficiency while maintaining essential physical realism in high-turbidity environments.*

Apart from anthropogenic activities, alterations in regional physical conditions aligning with climate changes could also regulate the long-term deoxygenation in coastal regions ..."

Minor edits:

L107: "Use" should be "used", assuming past tense.

Response: We will correct it as advised.

L118: Should the annual runoff estimate have a time unit? If it is indeed per year, you could indicate that in the unit.

Response: We will correct it as advised.

L122: "has" should be "have"

Response: We will correct it as advised.

Figure 1: The graph in this figure is really tiny. Please make it larger so the axis labels are easier to read

Response: We will correct it as advised.

Line 137: "Until the late 1990s…" is confusing as written. Can you specify the period in which the reservoirs were built? This is important because you say that there were no hydraulic facilities in the 1990s. How are these two periods different?

Response: Zhang et al. (2008) summarized the timeline of dam construction in the Pearl River Basin: '…In the Xijiang main channel, most reservoirs/dams were constructed in the 1990s…' 'In the Beijiang, the Feilaixia Reservoir was constructed in 1999 for flood regulation and farmland irrigation…'

Most large reservoirs were completed before 2000, and no large reservoirs have been constructed in the Pearl River Basin since 2010.

In this section, to eliminate ambiguity in the original text, we will revise the wording as follows:

"In the 1990s, the PRE displayed relatively low eutrophication levels, reflecting limited upstream urbanization at that time. This period also witnessed extensive construction of water infrastructure projects, mostly completed around 2000 (Zhang et al., 2008), which led to a dramatic reduction in the riverine suspended sediment concentration (SSC). By the late 1990s, the Pearl River basin contained at least 8636 reservoirs (Wu et al., 2016). After the 2000s…"

Figure 2: This figure is also far too small and difficult to read

Response: We will revise the figure.

Line 188: "dissolve" should be "dissolved"

Response: We will correct it as advised.

Line 205: "instant" should be "instantaneous"

Response: We will correct it as advised.

Line 272: "consecutive sinking" doesn't make sense. You could replace "consecutive sinking along with water transport," to "particle sinking as waters advected downstream,"

Response: We will correct it as advised.

Line 331: "has" should be "was"

Response: We will correct it as advised.

Figure 5 caption: Please add a unit for the hypoxia frequency

Response: We will correct it as advised. The revision now reads:

"Hypoxia frequency is calculated as the number of hypoxic days divided by the total number of days in the study period, yielding a dimensionless ratio (range: 0–1)."

Line 354-355: you could probably mention in this first sentence that surface DO went down in the landward region before you mention the increase in the ocean region. Fig 5b clearly shows a decline in the landward section.

Response: We will modify it as advised. The revision now reads:

"While in the 2010s, the surface DO has undergone an evident increase by 0.2-0.3 mg/L (Fig. 5b-c), with an oxygen-enriched zone in the lower PRE, which was closely

coupled to the surface high Chl a value (Fig. 3k). However, low-oxygen events (DO < 4 mg/L) have appeared in the surface waters adjacent to the river outlets in the 2010s (Fig. 5b) due to the low DO influx from the Pearl River along with the freshwater discharge."

Line 409: "enhanced" should be "increased"
Response: We will correct it as advised.

Line 561: "predominated" should be "predominant" (see also line 597)
Response: We will correct it as advised.

Reference

Chen, Z., Yu, L., and Hu, J.: Disentangling the contributions of anthropogenic nutrient input and physical forcing to long-term deoxygenation off the Pearl River Estuary, China, Water Research, 265, 122258, https://doi.org/10.1016/j.watres.2024.122258, 2024.

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