Re: Destruction and reinstatement of coastal hypoxia in the South China Sea off the Pearl River Estuary" by Yangyang Zhao et al.

January 31, 2021

Dear Editor,

Thank you for your time in handling our paper. Enclosed please find our revised MS entitled "Destruction and reinstatement of coastal hypoxia in the South China Sea off the Pearl River Estuary" by Yangyang Zhao et al.

During the revisions, we have fully considered the comments and suggestions from the reviewers. Briefly, we elaborated the dynamics of the time-series observations. We followed the reviewer's suggestion and calculated the biochemical-induced DIC changes from Leg 2 to Leg 3 and their relationship to the biochemical-induced oxygen changes and the vertical diffusion for oxygen based on the published diffusivity from Cui et al. (2019), to support our estimates of oxygen consumption rates (OCR). We also compared our estimates of the OCR and hypoxia formation timescale with previous studies in this study area and other large river-dominated shelf systems. Taken together, this study provides a robust approach to estimate the *in situ* OCR for hypoxia formation at large-scales.

Following suggestions, we have restructured our MS by combining results and discussion into a clear narrative. Moreover, we have expanded our discussion and included the tidal effects on hypoxia, the impacts of tropical cyclones-induced processes on hypoxia restoration, and the response of the hypoxia's evolution to the changes in frequency and intensity of tropical cyclones. Additionally, we have improved the quality of the figures for better illustrations. More detailed revisions are explained in the enclosure.

Finally, we would like to take this opportunity to thank the reviewers for their constructive comments and suggestions, which significantly improved the quality of the paper. We sincerely hope that our revision will meet the standards of *Biogeosciences*.

Sincerely,

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Anonymous Referee #1

General comments:

In their manuscript Zhao et al investigate the effects of a typhoon on subsurface water reoxygenation following hypoxic condition off the Pearl River Estuary and the subsequent time scale of hypoxia formation. The author calculate the oxygen consumption rate in bottom waters using a three end-member mixing model. The subject of the paper is not novel but is an interesting approach and relevant for the region and the journal. The use of the model is interesting although the assumption about the "quasi-static bottom waters" is questionable and do not seem to fit with the observations (e.g. Figure 3, see specific comments below). The observations and analysis are fine but too limited to fully support the conclusions. The authors rely heavily on the literature, whereas they could provide analysis or observations to support their interpretations. For example it would be interesting to see a sequence of oxygen vertical profiles, at multiple locations, when they look into the time-scale of hypoxia development after the typhoon. Volume might be an important factor at this time, although it is not mentioned because, as I understand, OCR is assumed to be uniform in subsurface waters. Does that imply that sediment oxygen demand is negligible? DIC observations are not used presently, although their analysis could strengthen the conclusions by relating OCR with respiration. Finally, part of the Discussion is a review of the literature (e.g. section 4.3) that is interesting but somewhat disconnected from the results presented. The discussion about hypoxia under future climate is highly hypothetical. Overall, the manuscript provides interesting results but could be significantly improved with more in-depth analyses of the observations. The quality of some figures could also be improved. Detailed comments are listed below.

[Response]: We appreciate the critical and constructive comments from the reviewer. The reviewer is correct that bottom waters may be dynamic upon disturbance by typhoons as reflected notably in vertical mixing. Here our "quasi-static bottom waters" assumption was referring to the limited exchanges between the oxygen-depleted bottom water mass under study with the surrounding environment. More specifically, we contend that this assumption was reasonable for calculating the oxygen consumption rate (OCR) from Leg 2 to Leg 3 because hypoxic zones typically developed in strong convergent zones (Lu et al., 2018; Li et al., 2020) with thus longer residence time that facilitates oxygen consumption. Indeed, the bottom water residence time in the sampling area was ~ 15 days (Li et al., 2020), significantly longer than the time lag from Leg 2 to Leg 3 (~ 6 days). We thus contend that the disturbance by typhoon mainly led to vertical mixing instead of horizontal water mass exchanges. The upward-intruded bottom waters were indeed visible in the time-series observations, and such vertical mixing was gradually suppressed by the surface plume-induced stratification (Fig. R1). The water column below the pycnocline became almost vertically well-mixed towards the end of the time-series observations before Leg 2. Based on the above notions and the reviewer's comments, we have revised our assumption as "the bottom water masses where biochemical oxygen-consumption prevailed were constrained by strong convergence and their outflow from the sampling area is insignificant on the time scale of the water residence time" in our revised manuscript (Page 6, Lines 142-144).

The reviewer is also correct that the OCR was assumed to be uniform in the subsurface waters, because (1) we only collected samples at three depth layers during Leg 2 and Leg 3, with usually only one depth layer below the pycnocline; (2) if the middle layer was below the pycnocline, the concentration of dissolved oxygen (DO) almost equaled to that at the bottom layer (e.g., Station F304; Fig. R3k). Sediment oxygen demand might be significant near the seabed or in its overlying water column (Kemp et al., 1992; Zhang and Li 2010). Here in our sampling area, oxygen losses by sediment oxygen demand (i.e., benthic respiration) were found much smaller than the bacterial respiration in the water column based on both incubation experiments and oxygen budget analysis (Cui et al., 2019). We thus assumed that the sediment oxygen demand was negligible and the microbial respiration in the water column dominated the estimated OCR (Page 19, Lines 434-442 of our revised MS).

We agree with the reviewer that we should fully use our data to support our conclusions. Dissolved inorganic carbon (DIC) and total alkalinity (TA) were used to validate our three-endmember mixing model. We have further calculated the biochemical-induced DIC changes from Leg 2 to Leg 3 and showed their relationship to the biochemical-induced DO changes, with a slope of -0.92±0.17 (Fig. R2), implying aerobic respiration dominated the OCR (Page 18, Lines 409-411 of our revised MS). We must point out that we did not directly estimate the DIC production rate. This is because uncertainties in DIC predications from the conservative mixing among three water masses reached ~ 30 μmol kg^{-1}, comparable to changes in DIC between two legs for nearly half of the sampling stations during Leg 3. We have however further added the oxygen profiles at multiple stations to further illustrate their temporal variations (Fig. R3): the DO concentration below the pycnocline significantly decreased from Leg 2 to Leg 3 and was almost homogeneous vertically (Page 14, Lines 302-305 of our revised MS).

Following comments, we have also reorganized our paper by combining results and discussion into a clear narrative, with an outline as below:

3 Evolution of intermittent hypoxia off the PRE

- 3.1 Extensive hypoxia before the typhoon
- 3.2 Destruction of hypoxia by the typhoon
- 3.3 Reinstatement of hypoxia after the typhoon

4 Maintenance, destruction and reinstatement of coastal hypoxia

- 4.1 Water column stability
- 4.2 Oxygen sinks and hypoxia formation timescale
  - 4.2.1 Mixing-induced oxygen sinks
  - 4.2.2 Biochemical-induced oxygen sinks
  - 4.2.3 Hypoxia formation timescale
- 4.3 Imprint of tropical cyclones on the evolution of coastal hypoxia

In particular, we have further modified the discussion, including the tidal effects on hypoxia, the impacts of tropical cyclones-induced processes on hypoxia restoration, and the response of the hypoxia's evolution to the changes in frequency and intensity of tropical cyclones. We have calculated the potential maximum hypoxic area as spring-to-neap tidal oscillations lead to variations in the DO concentration with a maximum neighboring oxygen range of 0.5 mg L^{-1} (Cui et al., 2019) (Page 16, Lines 345-360 of our revised MS). We have correlated post-storm precipitation and river discharge to our observations showing post-storm stronger blooms (Pages 21-22, Lines 512-529 of our revised MS). We have also added statistics on annual mean number and wind direction of tropical cyclones and the time interval between two successive tropical clones (Fig. R4 and Table R1) (Pages 23, Lines 541-551 of our revised MS).

Finally, we have improved the quality of the figures to better illustrate spatial distributions of temperature, salinity, DO and chlorophyll *a* (Chl *a*) concentrations (Pages 10, Figure 3 and Page 11, Figure 4 of our revised MS). We will further address these concerns from the reviewer in our responses as of below.

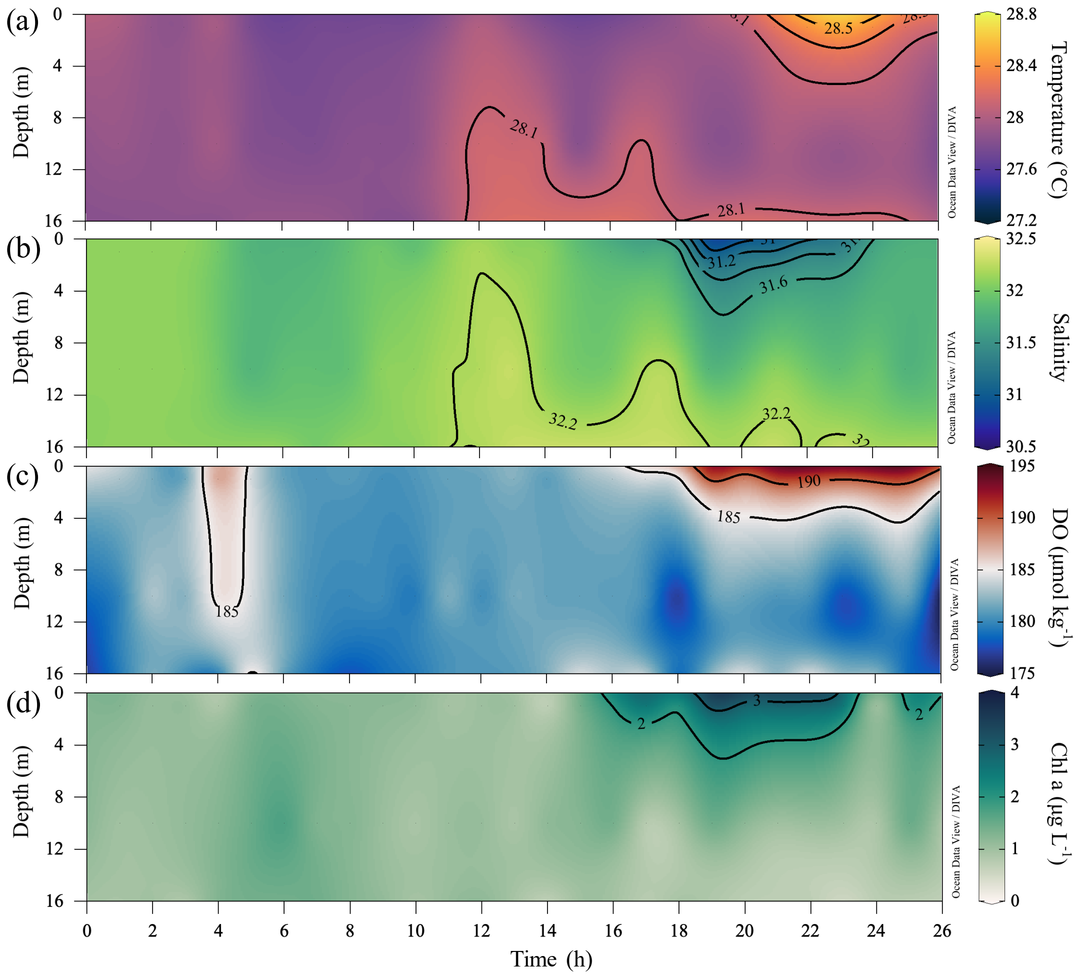

Figure R1 : Time-series observations of (a) temperature (°C), (b) salinity, (c) DO (μmol kg^{-1}) and (d) Chl *a* concentrations (μg L^{-1}) at Station F303 (see Fig. 1b) from July 19-20, 2018 after the typhoon passage, showing the complete destruction and the subsequent rapid development of stratification.

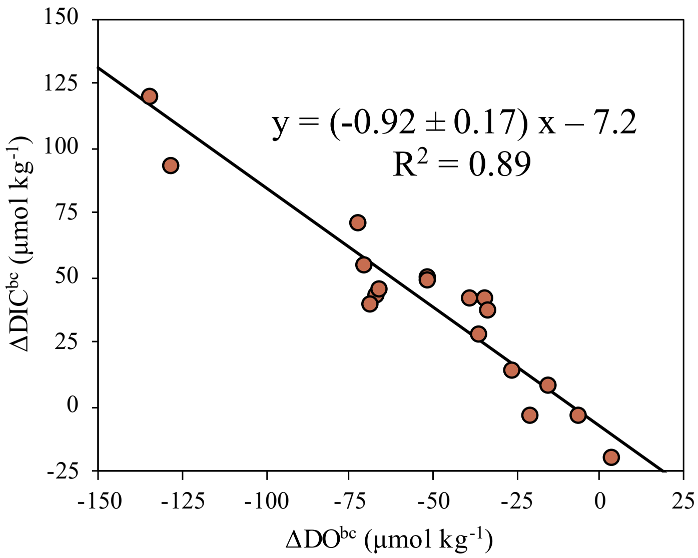

Figure R2: Biochemical-induced changes in DIC (ΔDIC^{bc}, μmol kg^{-1}) vs. DO (ΔDO^{bc}, μmol kg^{-1}) in bottom waters with depths > 10 m from Leg 2 to Leg 3. The black line denotes the slope of ΔDIC^{bc} plotted against ΔDO^{bc} derived from the Model II regression.

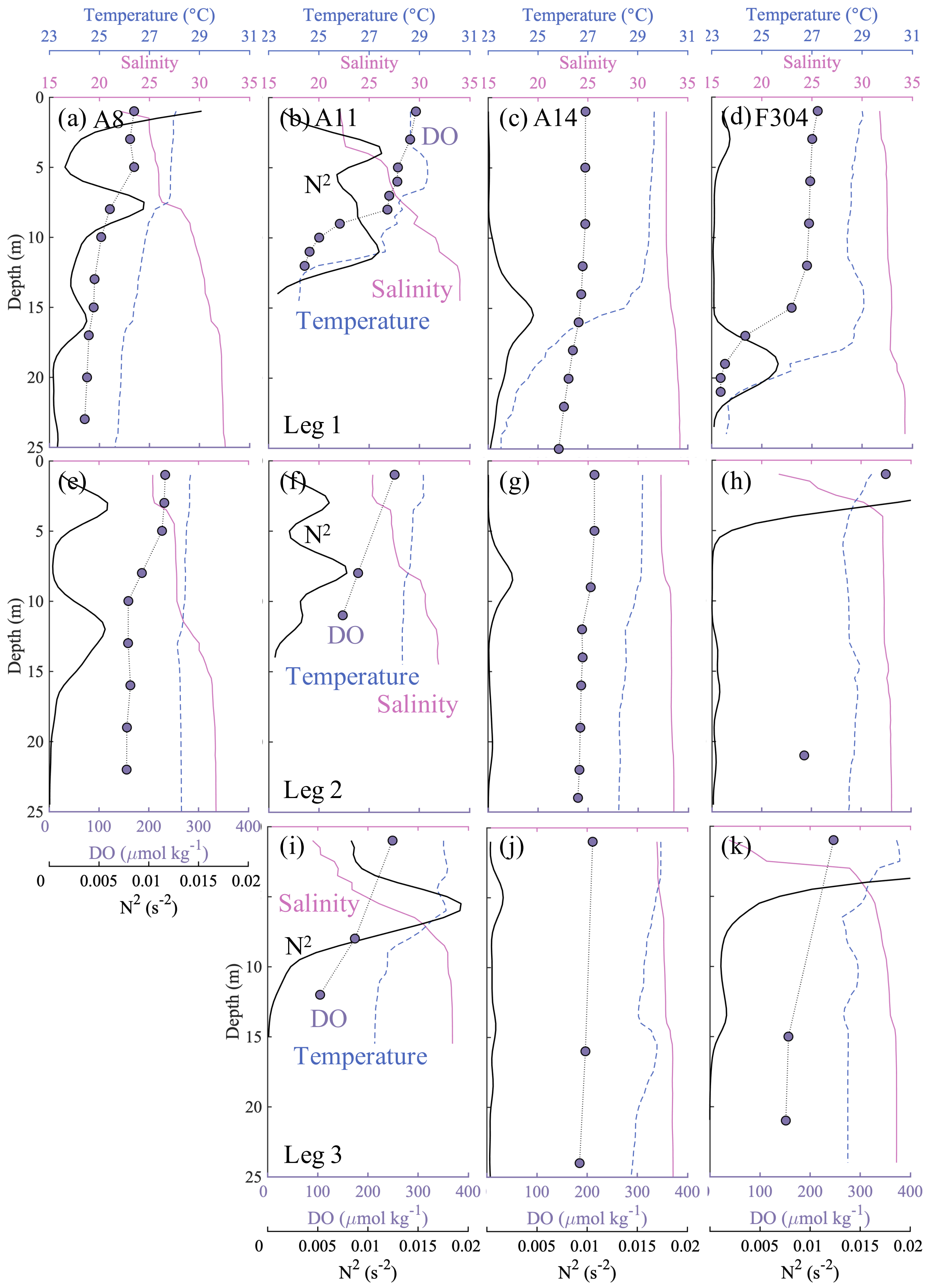

Figure R3: Profiles of temperature (°C) (green dashed lines), salinity (pink solid lines), dissolved oxygen (DO, μmol kg^{-1}) (purple dots) and buoyancy frequency N^2 (s^{-2}) (bold black solid lines) at stations A8, A11, A14 and F304 (see Fig. 1b), with visits both pre-typhoon (Leg 1) and post-typhoon (Legs 2 and 3). The vertical distributions of N^2 have been smoothed by the Gaussian method.

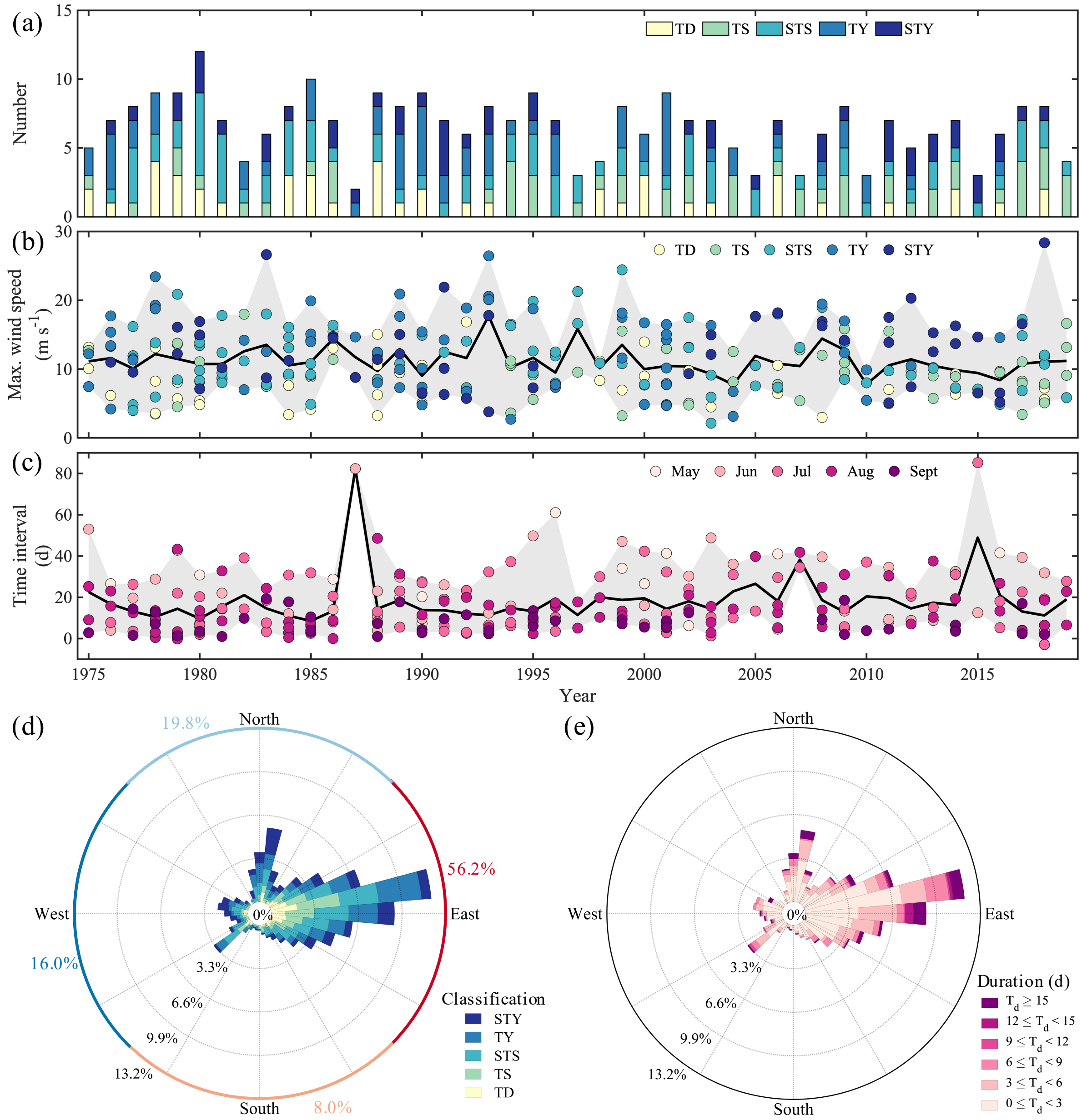

Figure R4: Statistics of tropical cyclones passing the northern South China Sea (NSCS) from May to September over 1975-2019. (a) Numbers of tropical cyclones. TD, TS, STS, TY and STY represent tropical depressions (the maximum wind speed near the centre is between 10.8-17.1 m s^{-1} over its lifetime), tropical storms (17.2-24.4 m s^{-1}), strong tropical storms (24.5-32.6 m s^{-1}), typhoons (32.7-41.4 m s^{-1}) and strong typhoons (41.5-50.9 m s^{-1}), respectively. (b) The maximum wind speed of each tropical cyclone. The black line and grey shadow denote the annual average and range of the maximum wind speeds. (c) The time interval between two successive tropical cyclones. The black line and grey shadow denote the annual average and range of the time intervals. (d) The wind rose of the intensity of tropical cyclones. (e) The wind rose of the duration of tropical cyclones. The wind speed in (b) and wind direction in (d, e) were recorded at the Waglan Island station.

Table R1: Summary of average frenquency of tropical cyclones in each decade from 1950-2019. TD, TS, STS, TY and STY represent tropical depressions (the maximum wind speed near the centre is between 10.8-17.1 m s^{-1} over its lifetime), tropical storms (17.2-24.4 m s^{-1}), strong tropical storms (24.5-32.6 m s^{-1}), typhoons (32.7-41.4 m s^{-1}) and strong typhoons (41.5-50.9 m s^{-1}), respectively.

Years	TD	TS	STS	TY	STY	SUM
1950-1959	3.5	1.1	1.2	1.1	1.5	8.4
1960-1969	1.7	0.6	1.5	2.1	2.7	8.6
1970-1979	1.8	0.7	2.1	2.1	1.2	7.9
1980-1989	1.5	0.7	2.5	1.3	1.3	7.3
1990-1999	0.7	1.2	1.8	2	1.1	6.8
2000-2009	0.9	1.4	1.5	1.5	0.8	6.1
2010-2019	0.6	1.8	1.4	0.4	1.5	5.7

Specific comments

L89: What are DIC and TA used for? How come you didn't use your DIC data to validate your estimate of OCR and to support your conclusions?

[Response]: DIC and TA were used for validating the three-endmember mixing model. TA is a quasi-conservative parameter due to its small changes during biological processes. Comparing our predicted values with measured TA, we found they were consistent with a subtle difference of 8±8 μmol kg^{-1}, which is asscoiated with measurement errors, propagation of uncertainty through the mixing scheme and/or biological processes (Fig. R5b). DIC is produced with the oxygen depletion. We calculated the biochemical-induced DIC and DO changes for each leg and found that they had a good relationship with a slope of -0.93±0.07 (Fig. R5c), similar to that reported by Zhao et al. (2020) for the same study area (Page 7, Lines 174-177 of our revised MS).

Following suggestions, we calculated the biochemical-induced DIC changes (ΔDIC^{bc}) from Leg 2 to Leg 3. We must point out that uncertainties in DIC predictions from the conservative mixing among three water masses reached ~ 30 μmol kg^{-1}, mainly due to a large variability in the DIC concentration of the brackish plume endmember (Table R2). These uncertainties were comparable to changes in DIC between two legs for nearly half of the sampling stations during Leg 3 (uncertainties in DO were only ~ 5 μmol kg^{-1}, much smaller than changes in the DO concentration from Leg 2 to Leg 3). We thus plotted the biochemical-induced DIC changes versus DO changes from Leg 2 to Leg 3, also showing a good relationship with a slope of -0.92±0.17 (Fig. R2), to indirectly validate our estimates of the OCR. This slope was consistent throughout the sampling legs, implying aerobic respiration of organic matters indeed dominated the OCR (Page 18, Lines 409-411 of our revised MS).

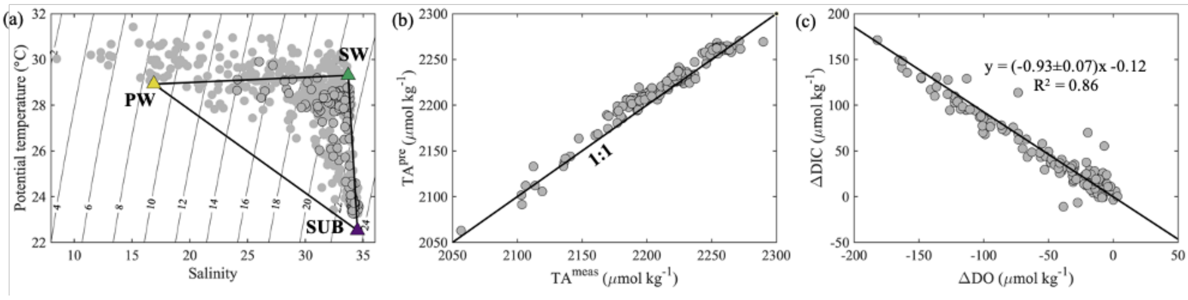

Figure R5: (a) Potential temperature (°C) vs. salinity, (b) predicted TA (TA^{pre}, μmol kg^{-1}) vs. measured TA (TA^{meas}, μmol kg^{-1}), and (c) ΔDIC (μmol kg^{-1}) vs. ΔDO (μmol kg^{-1}) on the NSCS shelf off the PRE. The black-edged circles represent bottom water samples with depths > 10 m. The yellow, green and purple triangles in (a) represent the endmember values of Brackish Plume Water (PW), offshore surface water (SW) and upwelled subsurface water (SUB), respectively. The black line in (c) denotes the slope of ΔDIC plotted against ΔDO derived from the Model II regression.

Table R2: Summary of the end-member values adopted in the three-endmember mixing model

Water mass	θ (°C)	Salinity	DIC (μmol kg^{-1})	DO (μmol kg^{-1})
Brackish plume water	28.9±0.4[b]	16.9	1776±29[b]	217.3±1.4[c]
Offshore surface water[a]	29.3±0.1	33.7±0.1	1922±5	194.4±0.3[c]
Upwelled subsurface water[a]	22.5±0.1	34.5±0.0	2022±3	180.9

[a]Adopted from Zhao et al., (2020)

[b]Uncertainties were derived from mutiple samples collected at the entrance of the PRE

[c]Uncertainties were calculated by propagating errors associated with the estimation of oxygen solubility using Benson and Krause Jr (1984)

L101: did you collect chlorophyll samples? at what time of the day did you collect your chlorophyll profiles? did you notice non photochemical quenching near the surface and if so how did you correct the profiles?

[Response]: We collected samples for chlorophyll *a* (Chl *a*) concentrations along with the cruise track during both day and night. We noticed the non-photochemical quenching near the surface from the fluorescence sensor, which was however not used to derive Chl *a*. Instead, we obtained the Chl *a* concentrations from discrete water samples. These samples were filtered onto GF/F (Whatman, USA) and stored in foil bags in liquid nitrogen until they were measured on a Trilogy laboratory fluorometer (Welschmeyer 1994) after extracted with 90% acetone for 14 h at -20 °C (Page 3, Lines 88-91 and Page 4, Lines 99-101 of our revised MS).

Figure 1c: can you put shaded areas at the time of the cruises to be clear about the conditions during the sampling? Figure 1d: can you provide units and y-axis tick values? The length of *the vectors don't seem to match the wind speed in the panel above. Also most vectors are oriented north-south and easterly winds vectors (associated with high wind speed) are small. Can you verify that the wind vectors are plotted properly?*

[Response]: We appreciate the suggestions. Accordingly, we have added shaded areas at the time of the cruises for each leg and re-plotted Figure 1d to match the arrow lengths with the wind speed (Fig. R6; Page 5, Figure 1 of our revised MS). During the cruise legs, easterly winds dominated with relatively larger east-west components than south-north components of wind velocities.

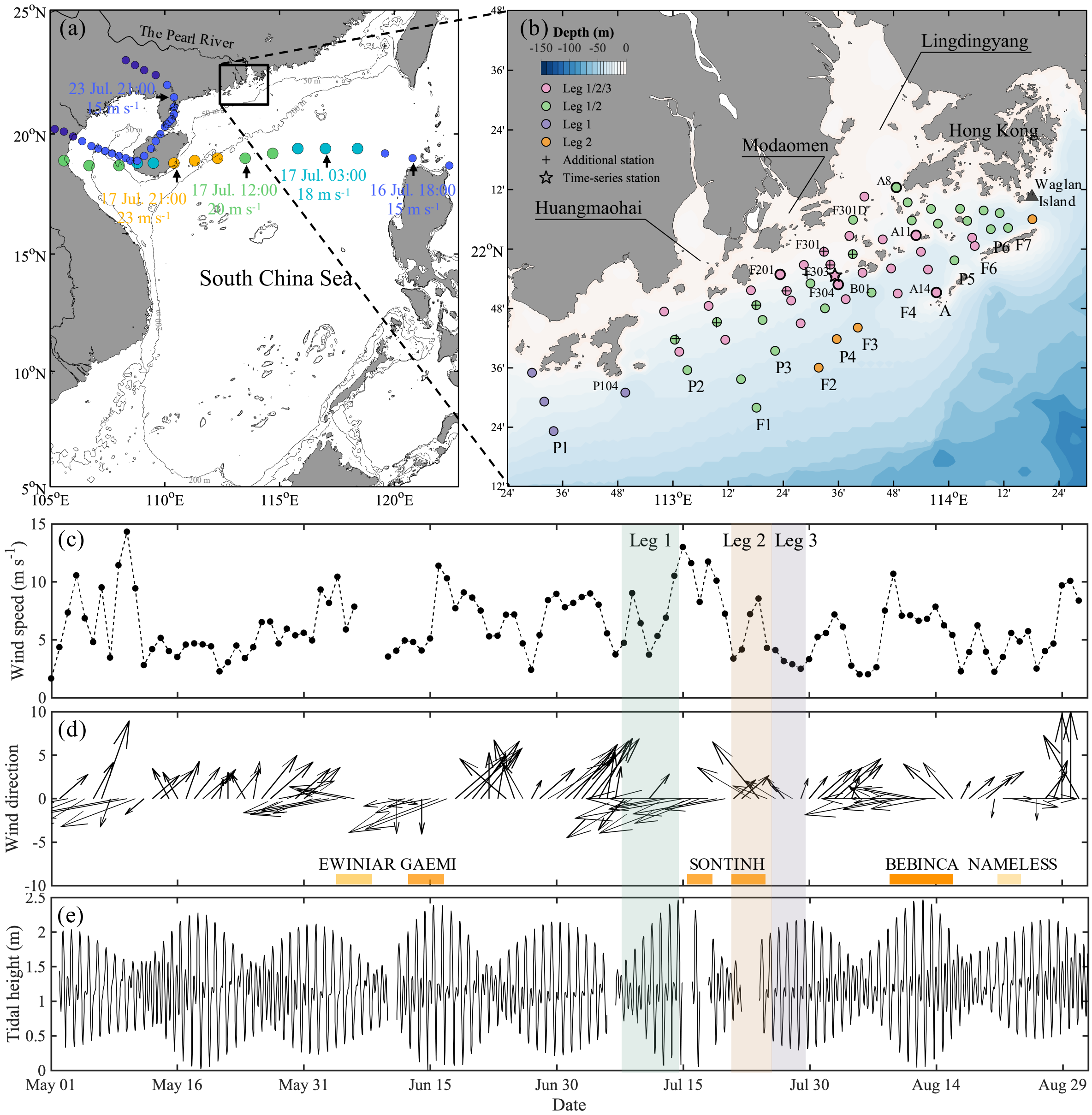

Figure R6: (a) Map of the study area on the shelf of the northern South China Sea (NSCS), showing the track of Typhoon SONTIHN (circles) across the NSCS during July 16-24, 2018. The color of the circles represents the magnitude of wind speed. Additionally, the smaller circles denote tropical depression (wind speeds ≤ 17.1 m s^{-1}) and the larger circles denote tropical storm (wind speeds within 17.2-32.6 m s^{-1}). The arrows denote the locations of the typhoon as marked with time and wind speed. The grey lines are the depth contours at 50 and 200 m. (b)

Sampling stations on the NSCS shelf off the Pearl River Estuary in summer 2018. The pink, green, purple and orange circles denote the stations surveyed in all three legs, only both Leg 1 and Leg 2, only Leg 1 and only Leg 2, respectively. Time-series observations were conducted at Station F303 as marked by the star, and vertically high-resolution samplings were conducted at stations marked with bold circles. (c) The wind speed and (d) wind direction at Waglan Island (triangle in (b)) from May to August, 2018. Bars at the bottom of (d) mark times when tropical cyclones impacted the NSCS. (e) The tidal height at the Dawanshan gauge station near Station F303 from May to August, 2018. The shaded area indicates the cruise periods for Leg 1 (grey), Leg 2 (pink) and Leg 3 (blue), respectively.

L132: I have a hard time believing this assumption. Can you provide support to your claim? In Figure 3 bottom water conditions vary rapidly

[Response]: We appreciate the reviewer's comment, which is indeed critical to this study. Please refer to our response to the general comment (Pages 1-2 of this response).

Figure 3: This indicates the intrusion of warm/salty warters that participate to the re-stratification of the water column. What is the O_2 and OM content of these waters? is OCR driven by sediment O_2 consumption or just water column respiration? this figure indicates dynamic surface and bottom layers, not what it is described in the Results section (quiescent bottom layer). Do you know why O_2 increases near the bottom? is there an advective source of O_2 that is not taken into account?

[Response]: We appreciate the critical comments. This figure indeed showed the intrusion of warm/salty waters, with the DO concentrations of ~ 180-184 μmol kg^{-1} and the POC concentrations of 0.12-0.16 mg L^{-1}, which differed by < 4 μmol kg^{-1} and < 0.04 mg L^{-1}, respectively, from the upper waters. The estimated OCR here was mainly driven by water column respiration detailed in our response above (Page 2 of this response).

We agree with the reviewer that the surface and bottom layers were dynamic over time, but with quite small variations (Fig. R1). As described in the Results section, DO in the bottom layer was homogeneous and temperature and salinity showed smaller cross-shore gradients than those during Leg 1, rather than quiescent. The slightly higher DO near the bottom layer likely resulted from previous strong vertical mixing upon disturbance by the typhoon, which mixed high-DO surface waters into the depth. As the upward-intruded bottom waters in our time-series observations were warm but salty, and the DO distributions were almost homogeneous at the bottom layer during Leg 2 (Fig. R7), these bottom waters unlikely sourced from nearshore warm, less-salty waters or offshore cooler, saline waters via lateral advection. We thus contend that we have taken into account all advective sources of oxygen from Leg 2 to Leg 3.

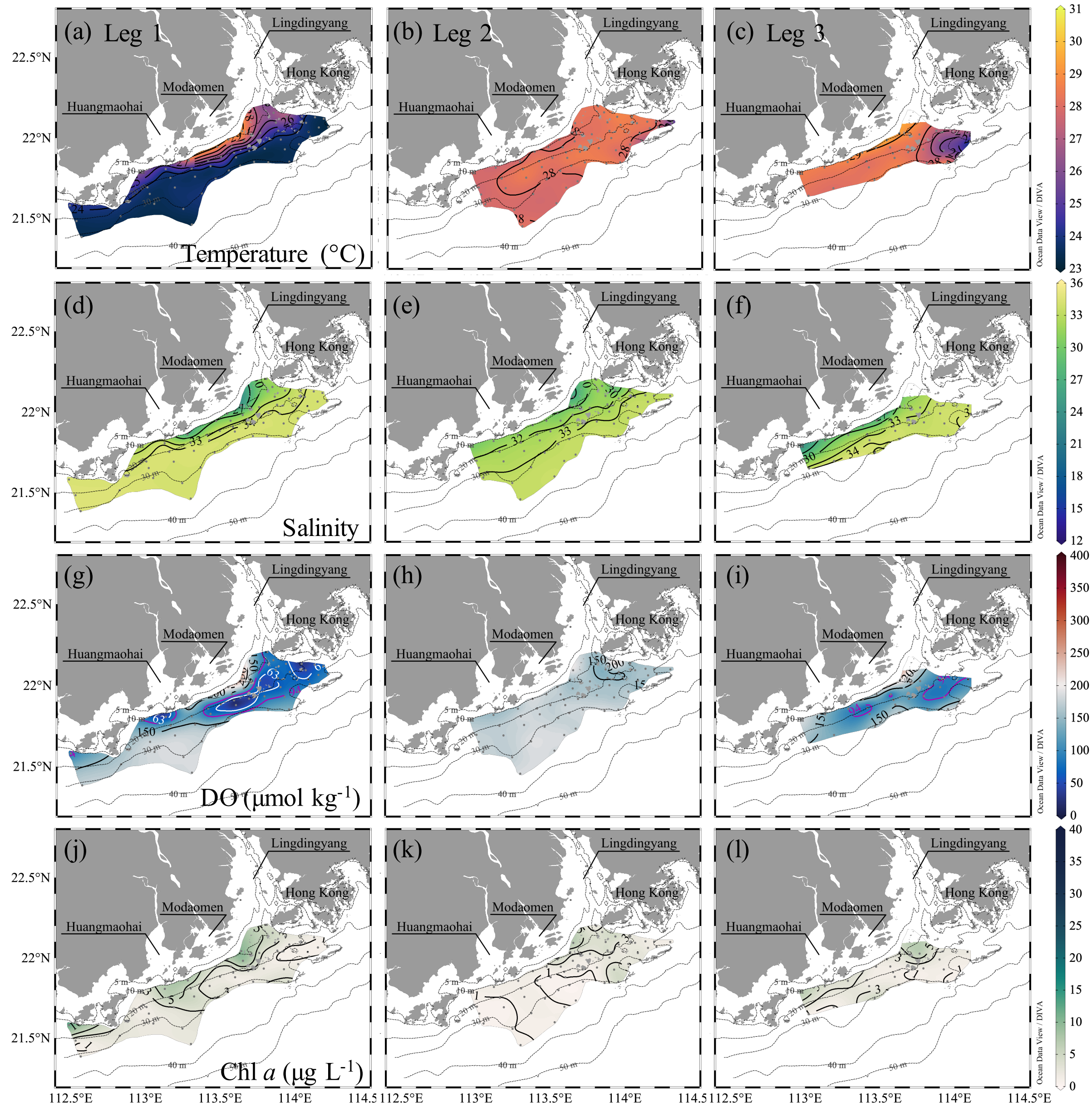

Figure R7: Distributions of temperature (°C), salinity, DO (μmol kg^{-1}) and Chl *a* concentrations (μg L^{-1}) at the bottom water layer off the PRE during Leg 1 pre-typhoon, and during Legs 2 and 3 post-typhoon. The white and magenta contours in (g) and (w) show the hypoxic (DO < 63 μmol kg^{-1}) and oxygen-deficit (DO < 94 μmol kg^{-1}) zones.

L199: You should calculate stratification rather than citing others

[Response]: We appreciate the suggestion. We calculated stratification using the buoyancy frequency (Page 4, Lines 102-109 of our revised MS), but we cannot partition the contributions from the vertical gradients in temperature or salinity. We cited the reference here to support that vertical temperature gradients could intensify stratification in addition to vertical salinity gradients. We therefore would like to keep this citation as we have reorganized the paper by combining results and discussion into a clear narrative with an outline listed above in our response to the general comment (Page 3 of this response).

L200: it didn't shift westward but was advected offshore indeed

[Response]: Accepted. By carefully comparing the surface salinity distributions during three legs, we have corrected the statement as "The freshwater bulge of lower salinity (< 15) advected offshore around the Modaomen sub-estuary" in our revised manuscript (Page 9, Lines 250-251).

L201: you are mixing discussion here

[Response]: Accepted. We have reorganized the paper by combining results and discussion to a clear narrative with an outline as listed above in our response to the general comment, and also revised the statement as "… likely driven by the interaction between the seaward buoyant current and northeastward shelf current (Pan et al., 2014; Li et al., 2020)" in our revised manuscript (Page 9, Lines 252-253).

L241: but salinity (i.e. plume waters) will also control the wind intensity that is required to mix the water column

[Response]: We agree with the reviewer that the wide-spreading plume also influences the wind-driven turbulent mixing. We discussed in sequence the effects of freshwater inputs (i.e., river plume), wind stress and direction, tidal fluctuations and spring-to-neap tidal oscillations on the water column stability. We therefore have corrected the statement as "Water column stability also largely depends on wind stress in coastal waters" and discussed the effects of river plume vs. wind stress on the water column stability in our revised manuscript (Pages 14-15, Lines 317-340).

L246: Figure 2j,n shows stratification during leg 2, i.e. surface plume water, so the vertically homogeneous water column occurs before leg 2 (July 14-19)

[Response]: Accepted. We have corrected the statement as "The strong winds facilitated mixing between high-temperature, low-salinity surface waters and cold, saline bottom waters, resulting in a vertically-homogeneous temperature and salinity, as observed in the first half of the time-series observations before Leg 2" in our revised manuscript (Page 14, Lines 321-323).

L248: Figure 2l indicates a strong post-storm bloom that is not mentioned

[Response]: We described the strong post-storm bloom in Section 3.2 — Destruction of hypoxia by typhoon — "Stronger blooms than that during Leg 1 were identified in the surface plume, widely spreading from the mouth of the Lingdingyang sub-estuary to near the Huangmaohai sub-estuary, potentially fueled by nutrients mixed upward from the deep in addition to riverine inputs (Wang et al., 2017; Qiu et al., 2019). The maximum Chl *a* concentration was > 40 μg L^{-1} off the Modaomen sub-estuary, accompanied by an extraordinarily high DO concentration of > 350 μmol kg^{-1}" (Page 9, Lines 228-232 of our revised MS).

L253: This should be true along the coast where the plume is trapped but not offshore
[Response]: We agree with the reviewer. Indeed, here we discussed the water column stability along the coast mostly within the 30-m isobaths where the plume was trapped.

L254: This is discussed in a recent paper of the Changjiang estuary, you could have a look, I assume similar mechanisms occur in the PRE. https://doi.org/10.5194/bg-2019- 341
[Response]: We appreciate the suggestion. Zhang et al., (2020) discussed the effect of wind direction on the plume spreading: the strong northward wind redistributes and advects the river plume towards the Yangtze Bank through Ekman transport, while the weakened northward or westward wind allows the location of the bottom hypoxia to migrate to the Submarine Canyon (Zhang et al., 2019). Similarly, off the PRE, upwelling-favorable winds (i.e., southwesterly) drive the river plume offshore and eastward (Gan et al., 2009), increasing the hypoxia area. However, downwelling-favorable easterly winds tend to constrain the river plume near the coast and drive surface waters to penetrate into the depth (Fig. R7), leading to an offshore shift of the hypoxic zone within a limited area beneath the surface plume. If the easterly winds last for a longer time than the hypoxia timescale, stronger blooms in the surface plume would enhance the bottom hypoxia with an abundant supply of fresh, labile organic matters. Based on the above notions, we have revised the statement in our revised manuscript (Pages 14-15, Lines 331-340).

L259: This is an interesting discussion but not supported by your observations so it feels a bit off topic
[Response]: Accepted. We have revised this discussion by calculating the potential maximum area of ~ 990 km^2 and ~ 1930 km^2 for the hypoxic and oxygen-deficient zones. The calculation is based on that spring-to-neap tidal oscillations lead to variations in the DO concentration with a maximum neighboring oxygen range of 0.5 mg L^{-1} (Cui et al., 2019) and our cruise legs were all conducted during the transformation from a neap tide to a spring tide (Fig. R6e). The hypoxia and oxygen-deficient areas therefore might be underestimated by at most of 34-50% in our sampling area due to tidal fluctuations (Page 16, Lines 345-360 of our revised MS).

L291: what does that mean? that salinity was >xx in your samples?

[Response]: The statement "we selected samples with water depths > 10 m, approximately below the pycnocline and where the surface plume was rarely involved" means that samples used for calculating the OCR were below the pycnocline and less affected by the upper plume waters. The selected samples with water depths > 10 m have salinity of > 31. We have revised the statement as "In estimating the OCR, we excluded the above-pycnocline samples collected at depths < 10 m affected by the upper plume waters that are subject to strong air-sea exchanges and/or photosynthetic production of oxygen" in our revised manuscript (Page 7, Lines 171-172).

L308: it is not a lower limit but an average estimate

[Response]: The OCR was spatially averaged, but it was also a lower limit. This is because we might overestimate the actual time for the significant oxygen consumption since its initiation between Leg 2 and Leg 3 and underestimate the diffusion of oxygen from the surface layer (assumed negligible during our estimates of the OCR). Both factors likely lead to an underestimation of the OCR (Pages 18-19, Lines 429-434 of our revised MS).

L309: I am surprised that there was no advective sources/sinks of O_2 given the observations in Figure 3

[Response]: We admit that there were vertically advective sources/sinks of O_2 in the time-series observations (Fig. R1). However, the upward intrusion of slightly warm/salty waters was progressively suppressed by the surface plume towards the end of the time-series observations. We also observed the wide-spreading river plume at the surface layer during Leg 2 (Fig. R8), restoring the vertical stratification and restricting the oxygen supply from the surface layer. In addition, the slightly higher DO near the bottom layer likely resulted from previous strong vertical mixing upon disturbance by typhoon, which mixed high-DO surface waters into the depth. We found that the upward-intruded bottom waters in our time-series observations were warm but salty, and the DO distributions were almost homogeneous at the bottom layer during Leg 2 (Fig. R7). These bottom waters were thus unlikely sourced from nearshore warm, less-salty waters or offshore cooler, saline waters via lateral advection. We contend that we have taken into account all advective sources of oxygen from Leg 2 to Leg 3.

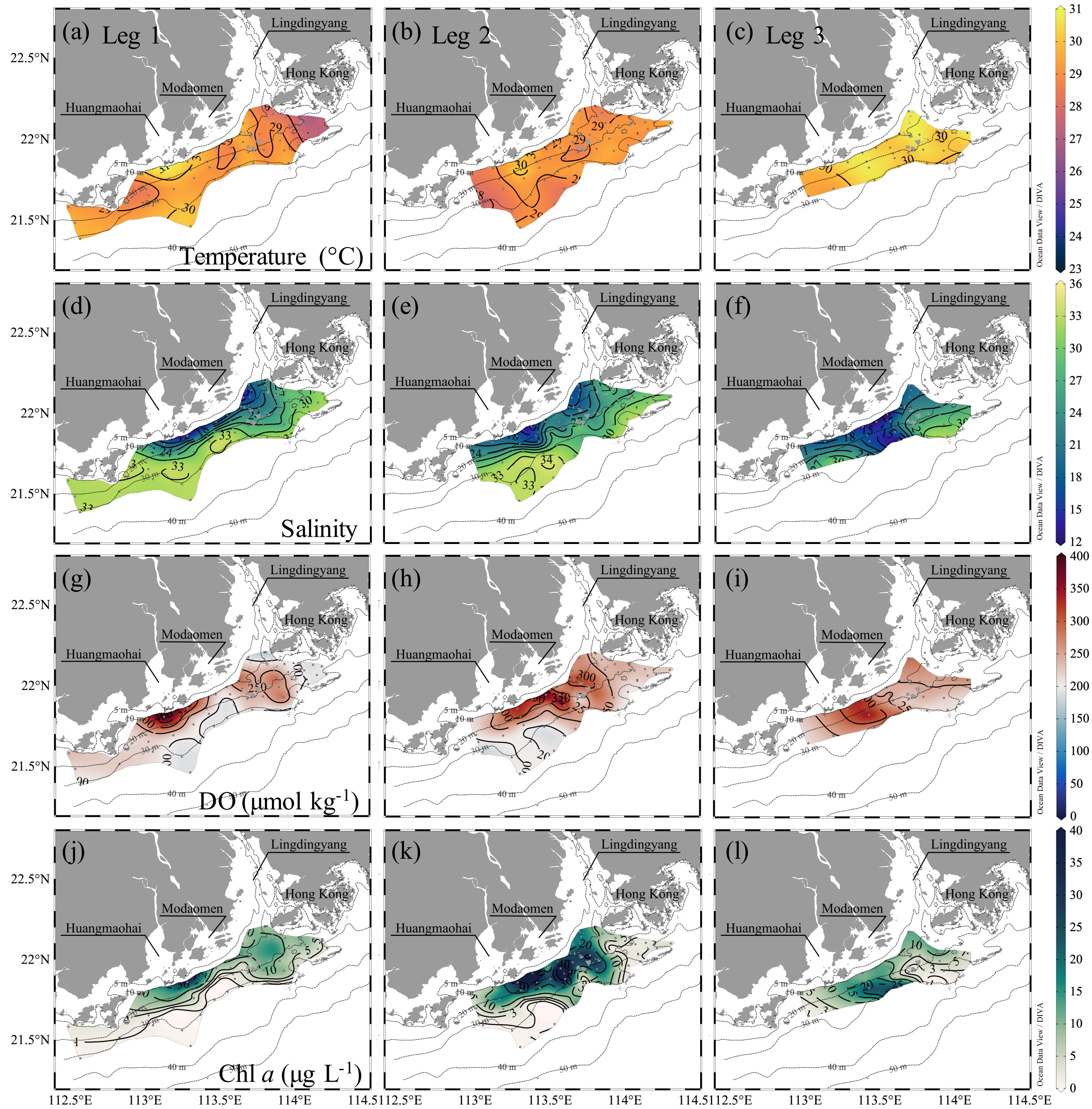

Figure R8: Distributions of temperature (°C), salinity, DO (μmol kg^{-1}) and Chl *a* concentrations (μg L^{-1}) at the surface water layer off the PRE during Leg 1 pre-typhoon, and during Legs 2 and 3 post-typhoon. The white and magenta contours in (g) and (w) show the hypoxic (DO < 63 μmol kg^{-1}) and oxygen-deficit (DO < 94 μmol kg^{-1}) zones. Figures were produced using Ocean Data View v. 5.3.0 (http://odv.awi.de, last access: 08 June 2020)

L313: You have to be clear how you use the terminology OCR. An increase in the OCR value (more positive) indicate a reoxygenetion.

[Response]: We appreciate the critical comment. Changes in the OCR of a negative value were confusing. We have revised the definition for the OCR as the biochemical-induced DO consumption with time. A higher OCR value indicates stronger oxygen consumption and a negative value indicates oxygen production due to biochemical processes (e.g., photosynthesis) (Page 4, Lines 113-115 of our revised MS). Accordingly, we have corrected all the statements associated with changes in the OCR (Page 1, Line 18; Page 18, Lines 412-415; Page 19, Lines 452-454; Page 20, Lines 468-471 of our revised MS).

L314: where is this shown?

[Response]: The data were not shown in time series for explicit comparisons. We have plotted the figure to show the data (Fig. R9) and have added this figure to our revised supplementary material (Page 4, Figure S3 of our revised Supplement).

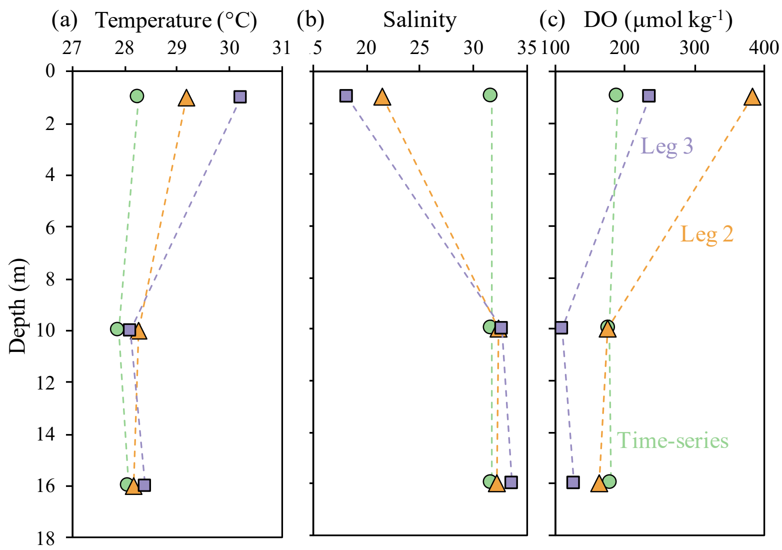

Figure R9: Profiles of temperature (°C), salinity and DO (μmol kg^{-1}) at station F303 at the end of the time-series observations and during Leg 2 and Leg 3.

L315: "decreased": so you mean less negative?

[Response]: The reviewer is correct that the OCR was less negative as it varied from ~ -9 μmol O_2 kg d^{-1} (July 20-22) to ~ -5.5 μmol O_2 kg d^{-1} (July 22-29). To avoid misleading, we have revised the definition of the OCR as the biochemically-induced oxygen consumption with time. A higher OCR value indicates stronger oxygen consumption and a negative value indicates oxygen production due to biochemical processes (e.g., photosynthesis) (Page 4, Lines 112-114 of our revised MS). We have also corrected the statement as "DO declined at a rate of ~ 9 μmol O_2 kg^{-1} d^{-1} from July 20-22, when the winds remained strong, and the OCR decreased to ~ 5.5 μmol O_2 kg^{-1} d^{-1} from Leg 2 to Leg 3 (from July 22-29)" in our revised manuscript (Page 19, Lines 452-454).

L317: you mean horizontal diffusion?

[Response]: The reviewer is correct, but we found that the horizontal diffusion for oxygen between the hypoxic zone and its surrounding environment might be much smaller than that induced by lateral advection. We thus have revised it to solely discuss the lateral advection (Page 19, Lines 457-462 of our revised MS).

L316-320: your analysis is not well supported, can you discuss your results and be more quantitative? If not, please do not extrapolate

[Response]: We agree with the reviewer that the horizontal diffusion for oxygen was hypothetical and we have removed this discussion in our revised manuscript.

L324: Figure 3 indicates the intrusion of warm and salty subsurface waters at station F303 after the typhoon, can you comment on that and how it fits with your analysis?

[Response]: We appreciate the critical comment. Indeed, the time-series observations at station F303 indicated an upward intrusion of warm and salty subsurface waters (Fig. R1). However, this upward intrusion was progressively suppressed by the surface plume. The water column below the pycnocline became almost vertically well-mixed towards the end of the time-series observations before Leg 2, showing small vertical variabilities in profiles of temperature, salinity and DO concentrations. Therefore, this upward-intrusion before Leg 2 will not compromise our assumption for estimating the OCR from Leg 2 to Leg 3.

Specifically, we attributed the upward intrusion to the subdued vertical mixing due to weakened winds (Fig. R6c), resulting in a less well-mixed water column. The upward-intruded warm and salty waters (temperature > 28.1 °C and salinity > 32.2) resulted from the antecedent strong winds-induced vertical mixing upon disturbance by typhoon, which mixed the warm, brackish surface waters (> 29 °C; Fig. R8a) downward to increase the temperature of bottom waters. Indeed, the bottom waters were > 28.1 °C with water depths > 16 m (the depth of the bottom layer at station F303) during Leg 2 (Fig. R7). The relatively cooler surface waters might result from the heat loss at the air-sea interface due to the reduction in air temperature by ~ 2-3 °C during the typhoon period and afterwards (Fig. R10).

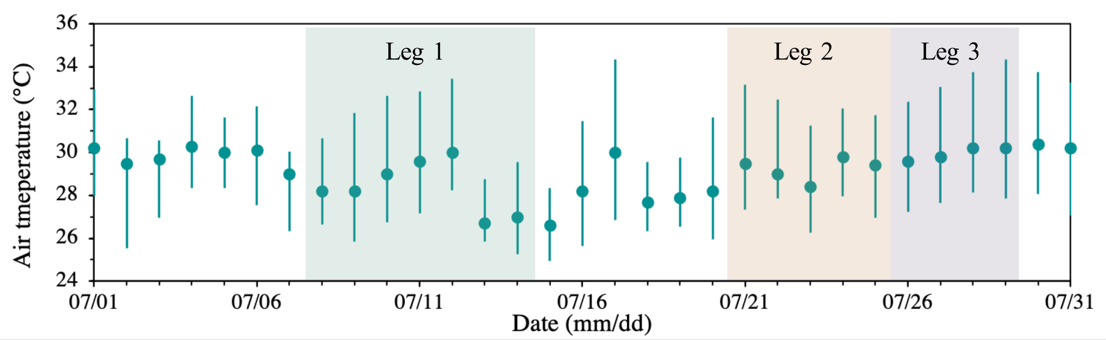

Figure R10: Air temperature at the Hong Kong Observatory in July 2018. The shaded area indicates the cruise periods for Leg 1 (grey), Leg 2 (pink) and Leg 3 (blue), respectively.

L336: end of sentence: (Figure 5)

[Response]: Accepted. We have added the reference of the figure at the end of the sentence (Page 20, Lines 468-470 of our revised MS).

L338: those are really rough estimates. It is impossible to see what are the bottom O_2 values in Figure 2 so it is difficult to judge your reasoning. Is the OCR=-15 value an average over the sampling area? bottom O_2 values seem rather low over the entire area sampled during Leg 3

[Response]: We agree with the reviewer that it was a first order estimate for upscaling to a larger area. This larger area implied the oxygen-deficient zone which likely developed into the hypoxic zone off the PRE. Therefore, we used the OCR of ~ 15 μmol O_2 kg^{-1} d^{-1} averaged over the oxygen-deficient zone (DO < 94 μmol kg^{-1}) during Leg 3, not over the whole sampling area. Despite low bottom oxygen values over the entire area during Leg 3, we showed relatively large spatial gradients of the total DO changes in bottom waters from Leg 2 to Leg 3 in Fig. R11 (Page 17, Figure 8 of our revised MS). The pattern of the OCR was almost consistent with the total DO changes, which is aligned well with the notion that mixing-induced DO changes were much smaller than the total DO changes. We have also improved the quality of figures by enlarging labels and boldening lines (Fig. R7; Page 11, Figure 4 of our revised MS) for better presentations.

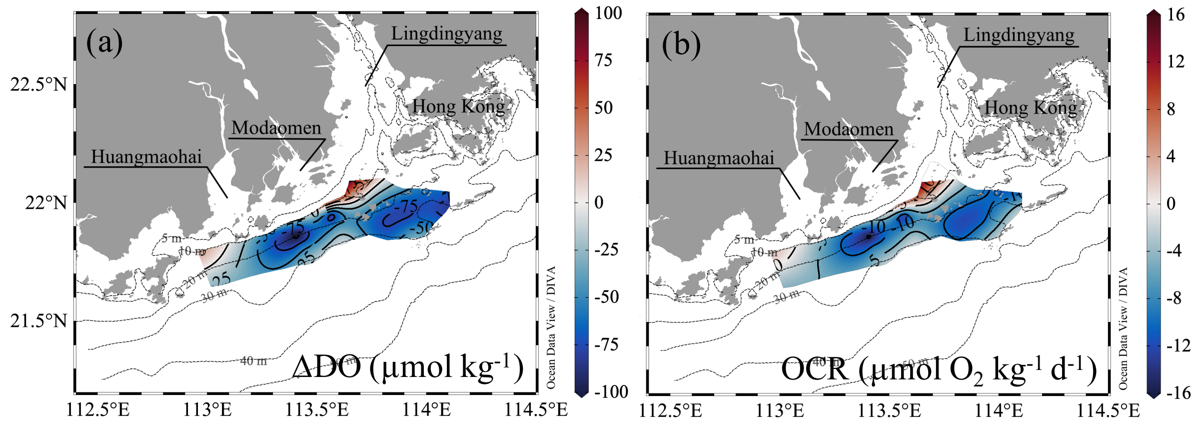

Figure R11: Distributions of (a) total DO changes (ΔDO, μmol kg^{-1}) and (b) the biochemical-induced oxygen consumption rate (OCR, μmol O_2 kg^{-1} d^{-1}) between Leg 3 and Leg 2 on the inner NSCS shelf off the PRE.

L341: Figure 3 shows that this is more variable than assumed here

[Response]: We agree with the reviewer that the time-series observations showed slightly more variable. This is because the time-series observations were conducted after the passage of the typhoon but still with relatively strong winds decreasing from ~ 10 m s^{-1} to ~ 7 m s^{-1} (Fig. R6c). The upward intrusion of warm and salty bottom waters was also progressively suppressed by the freshwater input-induced stratification towards the end of the time-series observations (Fig. R1). This would not conflict with our assumption here for a common scenario during the late spring, typically with a monthly average wind speed of < 6 m s^{-1}. The subsurface waters below the pycnocline therefore could be assumed almost well-mixed.

L345: how did you come up to the value 183 from your assumption? do you assume mixing rather than water replacement during intrusion?

[Response]: Thanks for the critical comment. Based on our assumption, the well-oxygenated offshore surface waters have an DO concentration of ~ 194 μmol kg^{-1}. We also estimated that shoreward-intruded subsurface waters reduced the initial DO level by 8.6±1.7 % of the oxygen decline for hypoxia formation —— ~ 11 μmol kg^{-1} when the oxygen level decreased from ~ 194 μmol kg^{-1} to the threshold of hypoxia (63 μmol kg^{-1}). The initial DO level for the biochemical-induced oxygen consumption for hypoxia formation was therefore ~ 183 μmol kg^{-1}. Actually, we assumed water mass mixing instead of water replacement during the intrusion (Page 20, Lines 475-479 of our revised MS).

L346: This does not match your calculation above with the average OCR, why are you assuming the maximum OCR during spring?

[Response]: We appreciate the critical comment. We used the maximum OCR during late spring to estimate a minimum time for the occurrence of hypoxia hotspots after the initiation of significant oxygen depletion. If the hypoxic zone developed to a larger spatial coverage, the time would be longer than the minimum time, which was consistent with our above estimates for the reinstatement of hypoxia in the summer of 2018.

L347: you should compare your values with similar systems, i.e. river-dominated estuaries (8-89 days). Also you could discuss your estimates in comparison of the PRE values provided in the reference.

[Response]: Accepted. We have compared our OCR estimates with previous studies in this study area and other large river-dominated shelf systems: "This result is larger than that estimated by Fennel and Testa (2019) — 4 days — using the modelled OCR of ~ 34 μmol O_2 kg^{-1} d^{-1} in the water column with a sediment oxygen demand of ~ 2.1 g m^{-2} d^{-1}, which were only applicable to the hypoxia formation in the Lingdingyang sub-estuary with shallower waters (~ 5 m) (Zhang and Li 2010). This result is still at the lower end of the hypoxia formation timescale in large river-dominated shelves globally (e.g., East China Sea off the Changjiang estuary, Northern Gulf of Mexico and Northwestern Black Sea), which varies from 8 to 89 days for hypoxia to develop once initiated (Fennel and Testa 2019). This short hypoxia formation timescale likely owes to a high OCR in relatively warm subsurface waters fuelled by abundant labile organic matters (Su et al., 2017; Zhao et al., 2020)" in our revised manuscript (Page 20, Lines 481-488).

L368-403: The last 2 paragraphs are not related to your results

[Response]: Accepted. We have revised these paragraphs by (1) checking the precipitation and river discharge after the typhoon, (2) correlating our discussion closely with our observtions of stronger post-storm blooms and their offshore advection along with the river plume, and (3) estimating changes in the frequency and intensity of tropical cyclones and the time interval between two successive tropical cyclones during the wet seasons. These revisions aim to discuss about the evolution of hypoxia upon disturbance by tropical cylones and its response to changes in the frequency and intensity of tropical cyclone activities (Pages 21-23, Lines 512-551 of our revised MS).

L368: You did not mention/discussed the phytoplankton bloom in leg 2

[Response]: As responded above, we described the strong post-storm blooms in Section 3.2 — Destruction of hypoxia by typhoon. We have further discussed the response of the post-storm hypoxia development to tropical cyclone activities, which might largely depend on the dynamics of the strong post-storm blooms: "Enhanced vertical mixing and/or freshwater discharge supplied large amounts of nutrients to the surface layer to fuel phytoplankton blooms following large storms (Zhao et al. 2009, Ni et al. 2016, Wang et al. 2017), as shown in Fig. 3 that strong blooms occurred in the surface plume along the coast with much higher Chl *a* concentrations during Leg 2 than that during Leg 1. The fresh autochthonous organic matter, together with the resuspended sedimentary organic carbon, provides sufficient substrates for microbial respiration in a re-stratified water column, leading to renewed or even exacerbated bottom water oxygen depletion (Zhou et al. 2012, Song et al. 2020)" (Page 21, Lines 518-523 of our revised MS).

L379: why?

[Response]: The statement was confusing and hypothetical. We have revised the statement based on our observations — "Whether it can develop into more severe hypoxia compared to that found initially during Leg 1 depends on the net OCR and water column stability, up until the passage of the next storm, Typhoon BEBINCA (Fig. 1d)" (Pages 21-22, Lines 527-529 of our revised MS) — bacause the post-storm blooms advected offshore might reduce the downward transport of labile organic matters to fuel the oxygen depletion in the subsurface waters.

Figure 6: I am not sure that annual averages are very pertinent, an average value per event might be more useful. For wind direction this could be presented as a pie chart. For wind speed, the time series is not very informative, may be think about an other way of presenting the results. This is somewhat included in Figure 6a but it would be interesting to know what is the maximum time between wind events for each year (or have some statistics based on your estimate of OCR). Also remember that hypoxia did not occur for most of the period shown here. panel c: please see comment regarding wind vectors in Figure 1, make sure those are right

[Response]: We appreciate the critical comments and constructive suggestions. The annual average was actually the annual average of the local maximum wind velocity for each tropical cyclone in the NSCS. We have revised the figure to additionally show the maximum wind speed of each tropical cyclone (Fig. R4b; Page 22, Figure 9 of our revised MS). For wind direction, we re-plotted it as roses of winds with the classifications of storm intensities and the duration times of tropical cyclones in the northern South China Sea (NSCS) (Fig. R4d, e). We also have calculated the time interval between two successive tropical cyclones for each year (Fig. R4c). In our calculations and statistics on historical tropical cyclones, only tropical cyclones that impacted the NSCS from May to September were taken into account because hypoxia often occurred from late spring to summer and disappeared in autumn in this study area.

L398: it depends on the direction, offshore intrusions would presumably bring lower O_2 waters

[Response]: We agree with the reviewer that offshore intrusions would bring lower-oxygen waters due to the deoxygenation of oceanic waters and it might contribute to the oxygen loss and the formation of hypoxia more significantly, but we have removed this statement as it was less related to our topic.

L409: "lowest ever recorded"

[Response]: Accepted. We have corrected the statement as "Eutrophication-induced hypoxia off the PRE was exacerbated with an enlarged area of ~ 660 km^2 and the lowest ever recorded regional DO concentration of 3.5 μmol kg^{-1} (~ 0.1 mg L^{-1})" in our revised manuscript (Page 23, Lines 558-559).

L417: This is speculation, higher discharge may lead to lower nutrient concentrations in river waters, more export to deep areas where hypoxia does not occur

[Response]: We agree with the reviewer that the higher discharge may decrease nutrient concentrations in river waters, but the total discharge of nutrients may increase due to strong flushing (Guo et al., 2008). In this study, stronger blooms occurred during Leg 2 than that during Leg 1, significantly utilizing the nutrients and producing organic matters to fuel the oxygen depletion in the subsurface waters. However, we observed an offshore shift of the blooms along with the river plume during Leg 3. Therefore, whether more nutrients will be exported to deep areas also depends on the phytoplankton uptake, the wind direction and alongshore currents that drive the river plume to spread offshore and their dominance on the water residence time. We have revised this statement without nutrient loading in our revised manuscript (Page 23, Lines 548-550).

Minor comments/edits:

L80: can you provide the number of stations in parenthesis for each leg, it is difficult to estimate it from Figure 1b

[Response]: Accepted. We have added the number of stations for each leg as "Almost all stations in Leg 1 (56 stations) were revisited during Leg 2 (56 stations, including 4 stations differing from Leg 1), and nearly half again during Leg 3 (27 stations)" in our revised manuscript (Page 3, Lines 79-80).

L81: suggestion: "on the way back to port"

[Response]: Accepted. We have revised the statement as "Eight stations were additionally revisited on the way back to the port on July 31" in our revised manuscript (Page 3, Lines 80-81).

L155-164: You are mixing results and discussion

[Response]: Accepted. We have re-organized the paper by combining results and discussions to a clear narrative with an outline listed above in our response to the general comment. Therefore, we kept them and will further compare the results with that in the summer of 2014 (Su et al., 2017) (Page 8, Lines 209-214 of our revised MS).

Figure 2: there is no point showing the river labels, they are way too small. Also the contour labels cannot be seen and the color bars are way too small. I suggest you move the colorbar to the top of each column and make it thicker with larger fonts An alternative suggestion is to

split the figure into surface and bottom figures and flip the rows to columns to make larger panels

[Response]: Accepted. Accordingly, we have added the river label in Fig. R6a (Page 5, Figure 1 of our revised MS) and separated this figure into two figures showing the surface and bottom distributions, respectively (Fig. R7 and Fig. R8; Page 10, Figure 3 and Page 11, Figure 4 of our revised MS). We also have enlarged the panels and contour labels for all figures.

Figure 4: The labels and lines are very small

[Response]: Accepted. We have enlarged the labels and boldened the lines (Fig. R3; Page 15, Figure 7 of our revised MS).

L312: not clear, the sentence should be rephrased

[Response]: Accepted. We rephrased the sentence as "During the hypoxia formation, the DO concentrations are in a non-steady state as oxygen sinks exceed sources. To shift towards a balance between oxygen consumption and replenishment for the maintenance of hypoxia, the OCR might decrease or the physical-induced oxygen supply increases" (Page 19, Lines 444-448 of our revised MS).

Anonymous Referee #2

I found this manuscript to be a useful contribution to our understanding of the spatial and temporal nature of oxygen depletion as a large coastal system responds to large events. The narrative is relatively easy to follow and the results are clearly communicated with figures. I think the analysis could benefit from a small amount of additional computations, but I also think that the results and discussion section needs to be reorganized. There is substantial mixing of results and discussions between the two sections, and I think it would be best and easiest to simply combine the two sections into one "Results and Discussion" section that is reorganized into a clear narrative.

[Response]: We are grateful that the reviewer valued our study. We also appreciate the critical comments and constructive suggestions from the reviewer, which will be fully considered in our revised manuscript. In brief, we have followed the suggestions to reorganize the paper by combining results and discussion into a clear narrative (Pages 8-23 of our revised MS), with an outline as below:

3 Evolution of intermittent hypoxia off the PRE

- 3.1 Extensive hypoxia before the typhoon
- 3.2 Destruction of the hypoxia by the typhoon
- 3.3 Reinstatement of the hypoxia after the typhoon

4 Maintenance, destruction and reinstatement of coastal hypoxia

- 4.1 Water column stability
- 4.2 Oxygen sinks and hypoxia formation timescale
  - 4.2.1 Mixing-induced oxygen sinks
  - 4.2.2 Biochemical-induced oxygen sinks
  - 4.2.3 Hypoxia formation timescale
- 4.3 Imprint of tropical cyclones on the evolution of coastal hypoxia

We additionally have (1) calculated the vertical diffusion for oxygen using the density-based eddy diffusivity from Cui et al. (2019) with our observed DO concentrations and estimated buoyancy frequency (Page 14, Lines 306-308 of our revised MS); (2) calculated and plotted the surface-to-bottom salinity and temperature differences to show the spatial distribution of stratification (Page 13, Lines 292-296 of our revised MS); (3) calculated the potential maximum surface area of the hypoxia associated with tidal fluctuations. The spring-to-neap tidal oscillations lead to variations in the DO concentration off the Lingdingyang sub-estuary with a maximum neighboring oxygen range of 0.5 mg L^{-1} (Cui et al., 2019). Assuming the observed DO concentration in Leg 1 (from a neap tide to a spring tide; Fig. R6e) was overestimated by 0.5 mg L^{-1} (i.e., ~ 15 μmol kg^{-1}), the total area of the hypoxic and oxygen-deficient zone would be at most ~ 990 km^2 and ~ 1930 km^2, respectively, 34-50% larger than our observed areas (Page 16, Lines 348-356 of our revised MS); and (4) discussed our results and estimates by comparing with previous studies in this study area and other large river-dominated shelf systems (Page 18, Lines 427-429 and Page 20, Lines 481-488 of our revised MS). We will address these concerns from the reviewer in our responses as of below.

Below are some specific and more general comments for the authors to consider:

(1) Line 49: "typhoons" should be plural

[Response]: Accepted. We have corrected it in our revisions (Page 2, Line 48 of our revised MS).

(2) Figure 1: It is a little difficult to discern the station locations of the different legs, given the overlapping in the circles. One suggestion could be to use different symbols to present (1) stations visited on all legs, (2) stations visited on legs 1+2, (3) stations visited on legs 2+3, and (4) stations visited on 1+3.

[Response]: We appreciate the suggestion. We have modified Figure 1b to show the stations more clearly: (1) stations visited in all legs, (2) stations visited only in Leg 1 and Leg 2, (3) stations visited only in Leg 1, and (4) stations visited only in Leg 2 (Fig. R1b) (Page 5, Figure 1 of our revised MS).

(3) Although Figure 3 nicely illustrates how stratification returned after the cyclone, it does not capture any patterns over space and it does not capture the entire coverage of the study in time. Figure 2 provides a nice, qualitative picture of the changes in water properties over time and space, but I think it might be helpful to also generate maps of the stratification changes, perhaps by plotting max N2 over space or the difference in temperature and salinity (or density) between surface and bottom waters. This would have the benefit of showing if stratification was weaker after it was reinstated than before the typhoon, where stratification was strongest, and how it related in space to hypoxia.

[Response]: We agree with the reviewer that changes in spatial distribution of stratification would help better understand how it is related to hypoxia in space. Following suggestions, we have plotted the difference in temperature and salinity between surface and bottom layers (Fig. R12; Page 14, Figure 6 of our revised MS). We also have revised our discussion on the effect of stratification on the formation and maintenance of hypoxia: "The surface-to-bottom salinity difference showed large values within the surface plume area, which almost covered the bottom hypoxic zones. Exceptions only occurred to the hypoxia zone off the Modaomen sub-estuary, where the surface-to-bottom salinity differences were relatively small but the temperature differences were large (i.e., ΔT_{b-s} < - 4 °C) due to the shoreward intrusion of cold offshore subsurface waters. The regions occupied by the surface plume and the shoreward-intruded shelf bottom waters therefore overlapped, resulting in a more stable water column where a patchy hypoxic zone could persist for more than 5 days (Cui et al., 2019)" (Page 13, Lines 292-298 of our revised MS).

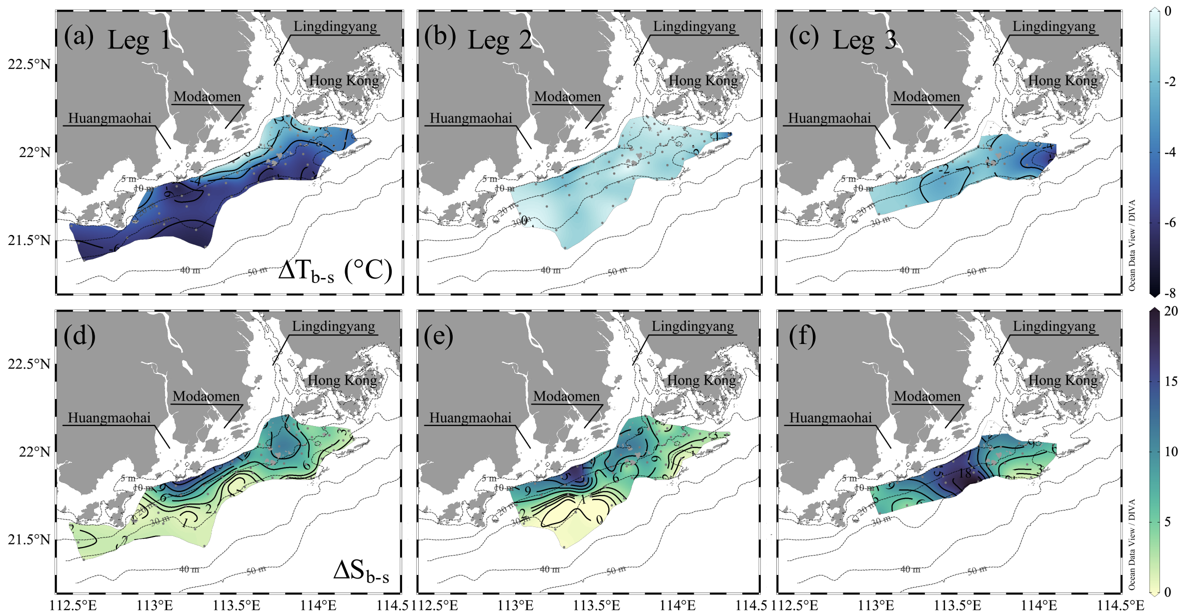

Figure R12: Surface-to-bottom temperature (a-c) and salinity (d-e) distributions off the PRE during Leg 1 pre-typhoon, and during Legs 2 and 3 post-typhoon. ΔT_{b-s} and ΔS_{b-s} represent the difference in temperature and salinity between the bottom and surface layer, respectively.

(4) Figure 4 – I think it would be more interesting to also show vertical oxygen distributions on figure 4, to show where hypoxia exists relative to the vertical structure and stratification.

[Response]: Accepted. We have added oxygen profiles at multiple stations accompanying the vertical distributions of temperature, salinity and buoyancy frequency (Fig. R3; Page 15, Figure 7 of our revised MS). The DO concentrations decreased sharply at the base of the surface plume (salinity ~ 30).

(5) You report on the decline in oxygen concentration in the water column after the typhoon passing as a metric of oxygen consumption rate. It would make the paper more compelling,

and help the discussion, to compare these rates of oxygen depletion to similar rates published in other systems (e.g., Testa and Kemp 2014, others?)

[Response]: We have added such comparisons in our revisions: "Our estimated OCR is comparable in magnitude to the community/bacterial respiration rate from previous studies in this study area (9.6 μmol O_2 kg^{-1} d^{-1}, Su et al., (2017); 7.9 to 19.0 μmol O_2 kg^{-1} d^{-1}, Cui et al., (2019); 16.8±8.9 μmol O_2 kg^{-1} d^{-1}, Li et al., (2019)) and within the range in other estuaries and coastal systems (Dortch et al., 1994; Robinson 2008)" (Page 18, Lines 427-429 of our revised MS).

(6) Line 315-319: Can you estimate the oxygen diffusivity rate from your data, based on any published estimates of diffusivity for the region, or estimated from your density profiles? This would allow you to be more quantitative in your comparison of OCR and diffusivity as eventually balancing. I think you could also speculate, perhaps with data, why OCR could have possibly declined, either as the post-bloom organic material was exhausted or due to oxygen limitation of respiratory uptake?

[Response]: Accepted. We have estimated the vertical diffusion for oxygen based on the published diffusivity from Cui et al., (2019) along with our observed DO concentrations and estimated buoyant frequency: "Using the density-based eddy diffusivity (K_z) of < 5×10^{-6} m^2 s^{-1} for N^2 larger than 1×10^{-3} (Cui et al., 2019), we estimated the vertical DO diffusivity (VDIF = $K_z\times(\partial DO/\partial z)$) of ~ 0.25 g m^{-2} d^{-1} with a maximum of 0.54 g m^{-2} d^{-1} in the top 10 m at stations A8 and A11, which was comparable to the results from Cui et al. (2019). It therefore acted as a barrier layer, with weak dissipation of oxygen into the subsurface waters" (Page 14, Lines 306-309 of our revised MS). As inhibited by freshwater input-induced stratification, the oxygen supply by the vertical diffusion was much smaller than the biochemical consumption, leading to a relatively strong net oxygen consumption.

The decline in OCR could result from a reduced supply of labile organic matter or oxygen limitation of respiratory uptake. Oxygen enriched incubations of unfiltered water samples revealed that the OCR could be significantly enhanced when the initial *in situ* DO concentration was low (e.g., ~ 30 μmol kg^{-1}), but changed little when the *in situ* DO concentration was higher than ~ 90 μmol kg^{-1} (He et al., 2014). Here, the bottom DO concentrations were ~ 180 μmol kg^{-1} at station F303 in the time-series observations, much higher than the oxygen threshold for respiratory uptake. However, the strong post-storm blooms shifted offshore with the river plume and the Chl *a* concentrations over the hypoxic zone also decreased in Leg 3 as comapred to Leg 2 (Fig. R7), reducing the downward transport of labile organic matter to the hypoxic zone. The decline in the OCR in this study thus very likely owed to a reduced supply of labile organic matter (Page 19, Lines 448-457 of our revised MS).

(7) Paragraph on Line 389: This paragraph reads more like an essay on the factors driving hypoxia and vulnerable to climate change, and does not really discuss the specific details of this study. I suggest deleting it, perhaps keeping the cyclone points for the prior paragraph on cyclone effects.

[Response]: Accepted. We have removed the discussion on the exacerbation of hypoxia under a changing climate, but keep discussions on the response of coastal hypoxia to changes in the frequency and intensity of tropical cyclone activities. Statistics on the local maximum wind speed and wind direction of tropical cyclones and the time interval between two successive tropical cyclones have been added based on the historical dataset of tropical cyclones that impacted the northern South China Sea from May to September during the period of 1975-2019 (Table R1; Fig. R4) (Page 23, Lines 541-551 of our revised MS).

(8) I think you should combine the Results and Discussion Sections into one, well-organized narrative. As it stands, there are multiple places where results are reported in the discussion, or there are even methods in the discussion. This would allow you to more clearly and sequentially tell the story of your study. Below are some specific examples to guide this effort: (a) Line 225-239 is largely results and even methods, but is included in the discussion without substantial discussion of the results in the context of the study. (b) Line 285-290. Here, you are describing the method you already described. Move to methods and remove redundancy. (c) Paragraphs beginning on lines 332 and 343 can be combined

[Response]: Following suggestions, we have moved all method descriptions to the section of Materials and methods (Page 4, Lines 102-109 and Pages 6-7, Lines 155-177 of our revised MS) and reorganized the paper by combining results and discussion into a clear narrative, with an outline as listed above in our response to the general comment.

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