Reply to comments RC2:

P2 Lines 38-39. The natural hypoxia often occurs in the open ocean, e.g. OMZ (oxygen minimum zone), while the coastal hypoxia is usually caused by human activities in recent centuries. The authors should be clearer about these two types of hypoxia.

Reply 1: This sentence should be revised. Natural hypoxic environments have existed throughout geological time in the open ocean. However, coastal hypoxia occurs in recent centuries due to human activities.

P3 Lines 55-56. In my opinion, this sentence is not easy to follow. Could the authors simplify?

Reply 2: In the Pear River estuarine coastal waters, hypoxia has only occurred as episodic events over small areas.

P3 Lines 57-59. The hypoxia south of Macau is not a new discovery but has been reported and modelled much earlier. In recent years, it received more attention and was studied more extensively. The authors should clarify this.

Reply 3: Yes, the hypoxia south of Macau has been reported much earlier (Yin et al., 2004; Su et al., 2017; Lu et al., 2018) and relatively higher hypoxia frequency (> 50%)was observed in the western hypoxia center (Li et al., 2020). We should modify the statement.

P5 Lines 105-107 It is not clear why grouping 29 years into 5 groups is needed. The authors only used this to show the decadal trend of wind frequency in Figure 8. Actually, in my opinion, the decadal trend shown in Figure 8 was not obvious. Why not just plot the wind frequency every year and do the regression to show its trend as they did in Figure 7?

Reply 4: Yes, maybe it is a better choice to plot the wind frequency every year and then do the regression to show its decadal trend rather than divide 29 years into 6 groups (5 years per group). We will redraw Figure 8.

Section 3.1 What's the depth of the surface and bottom oxygen measurements? Is the bottom oxygen measured near the seafloor? The authors should clarify this. In addition, could the authors show the complete vertical profiles of measured temperature, salinity, and oxygen?

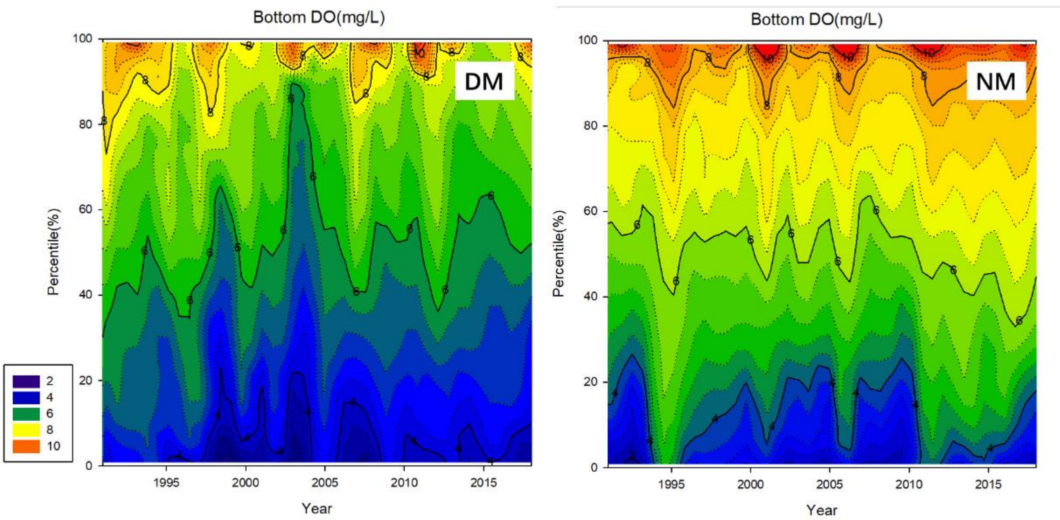
Reply 5: The surface DO is measured at 1 m below water surface, and bottom DO at 1 m above seabed. The vertical profiles of temperature, salinity and oxygen are not accurate because the water sampling is only taken at 3 depths and continuous measurements in the whole water column are not processed.

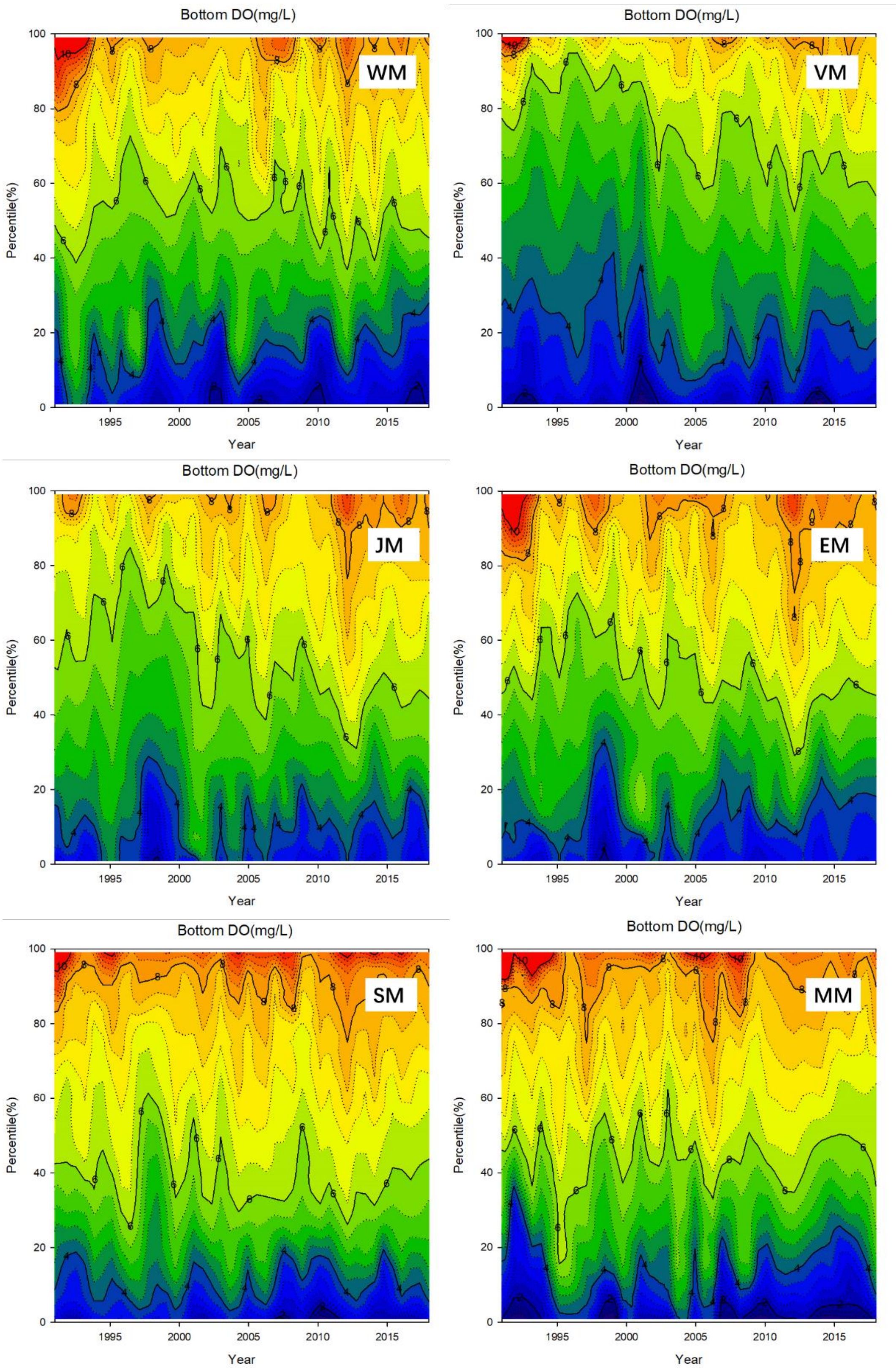
P6 Lines 114-117. Considering that there is only one oxygen measurement per month in 3 stations, the conclusion seems not representative. The authors should have more justifications on this.

Reply 6: The newly added Fig. S2 shows the sufficient coverage of DO in the 10 water control zones in Hong Kong (Fig. S1). We also plot the time series of bottom DO at SM20, SM6 and MM8 (Fig. S3).

About temporal scales, if a hypoxic event at one location can not occur every year or can not last for 2 months long, we do not consider it to be a seasonal phenomenon or a seasonal scale event. Our data show that a hypoxic event rarely occurs at one station in two consecutive months, and hardly occurs across the 3 stations in the same month, which means that the hypoxic event is only site events, not over the coast-wide scale of the southern waters.

Fig. S1 The ten water control zones in the Hong Kong waters (EPD report, 2017). The number 1-10 denotes water control zones as follows: 1-Tolo Harbour and Channel WCZ (TM), 2-Southern WCZ (SM), 3-Port Shelter WCZ (PM), 4-Junk Bay WCZ (JM), 5-Deep Bay WCZ (DM), 6-Mirs Bay WCZ (MM), 7-North Western WCZ (NM), 8-Western Buffer WCZ (WM), 9-Eastern Buffer WCZ (EM), 10-Victoria Harbour WCZ (VM).




Bottom DO(mg/L)
WM
VM
JM
EM
SM
MM
Percentile(%)
Year
0
20
40
60
80
100
1995
2000
2005
2010
2015

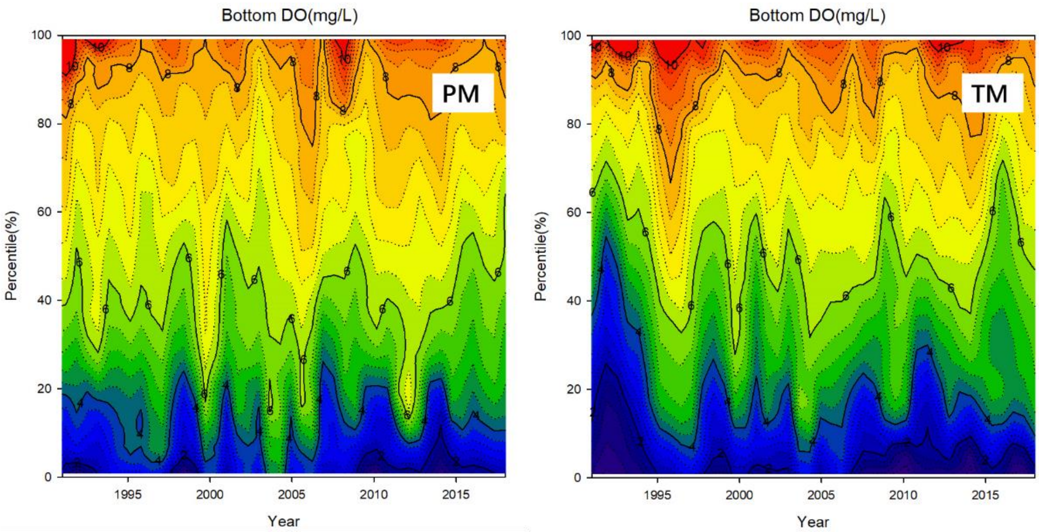


Fig. S2 The percentile of bottom DO during 1986 to 2018 in 10 water control zones in the Hong Kong waters.

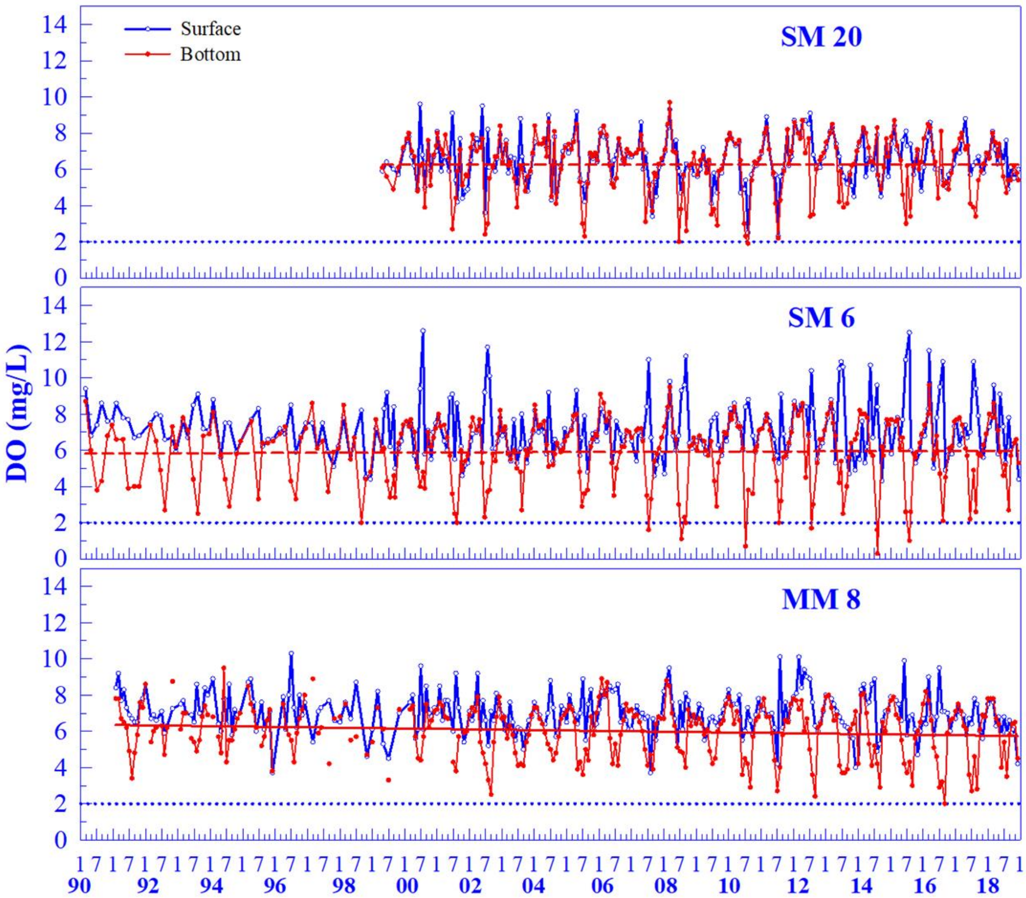


Fig. S3 The time series of DO at SM20, SM6 and MM8 during 1990-2018.

P6 Lines 126-128. Are there any other mechanisms resulting in the △DO and AOU? Since these three stations receive sewage from the CEPT of

Stonecutter's Island, is it possible that the AOU is caused by nutrient inputs from sewage? Is stratification responsible for the △DO and AOU? This statement seems to be contradicted with the following conclusion that stratification plays regulating roles in bottom DO.

Reply 7: Instead of saying AOU is caused by nutrient inputs, we agree that AOU indicates oxygen consumption in the sinking process of water mass due to decomposition of organic matter which is related to nutrient inputs. Water column stratification affects the bottom DO supply rather than oxygen consumption. In sum, organic matter decomposition is the dominant mechanism that results in the △DO and AOU, and stratification plays a regulating role in bottom DO supply which affects the bottom DO concentration as well.

P6 Lines 128-133. The authors seem to use the data in all seasons to do the regression analysis. In this time scales, the major forcing to control the stratification and oxygen should be river discharges instead, rather than the wind forcing. Since this study is to focus on the wind interruptive effects on summer hypoxia, the authors should also do the regression analysis only for summer data as they did in Figure 7.

Reply 8: Agreed. We should revise the regression analysis by only using summer data.

P6 Lines 129. Should the correlation coefficient r be -0.70?

Reply 9: Yes, bottom DO is correlated to $\triangle\sigma$ at the 3 stations with correlation coefficient, r, being -0.70 at $p < 0.01$.

P7 Lines 147-150. The $\triangle\sigma$ seems not correlated to wind speed.

Reply 10: Yes, we should revise the statement. The correlation between surface-bottom density difference $\triangle\sigma$ and wind speed V7 is not significant mainly because water column stratification is synthetically determined by many physical factors such as tides and river discharge, not just wind speed. In addition, V7 may not necessarily be the best forceful period.

P7 Lines 152-154. As shown in Figure 5, the most data points with low $\triangle\sigma$ and high bottom DO seems from September. Since the river discharges in September are much lower than that in summer (June - August), the dominant mechanism may be different. The authors should have more discussion here, e.g. about effects of river discharges, or remove the September data from analysis.

Reply 11: Thanks for your advice, we need to add river discharge data and discuss its effects on $\triangle\sigma$ especially in September.

Section 4.1. The whole section is basic knowledge of hypoxia and not tightly related to the subject of this paper (wind interruptive effects). I would suggest to move this part into introduction and shorten this section, which would be helpful for readers who are not familiar with hypoxia.

Reply 12: Thanks for your suggestion. We will move this part into introduction.

P9 Lines 199-200. Could the authors explain more about the ecosystem buffering capacity. Plus, the reference of Yin et al. (2013) was missing. Please check it.

Reply 13: The ecosystem buffering capacity has been discussed in section 4.3., and the reference of Yin et al. (2013) will be added.

Yin, K.*, J. Xu, Z. Lai, P. J. Harrison, 2013. Dynamics of phytoplankton blooms and nutrient limitation in the Pearl River (Zhujiang) estuarine coastal waters. pp. 274-295. In Thomas S. Bianchi, Mead A. Allison, and Wei-Jun Cai (eds), Biogeochemical Dynamics at Major River-Coastal Interfaces: Linkages with Global Change, Cambridge University Press, 658 pages. ISBN 978-1-107-02257-7 (hardback).

Section 4.2 I like the idea to compare the interval of wind events and timescales of oxygen being consumed to hypoxic level. Could the authors make this section more precise and concise by removing those unrelated contents, e.g. P10-11 Lines 219-230 and the 3rd paragraph in this section.

Otherwise, the authors should relate these contents to the idea of this section (wind interruptive effects) more clearly.

Reply 14: We cited some previous studies (Lines 219-239) to illustrate the role of physical processes especially wind in hypoxia formation, which is helpful to our discussion on wind interruptive effects. We will revise this section as you suggest. In Lines 244-264, we compared the interval of wind events and timescales of oxygen being consumed to hypoxic level, and explained its rationale in detail.

As I understand, a strong wind event will interrupt stratification and hypoxia formation. After that, there requires several days, for example at least 7 days in this study, for the reformation of hypoxia. Following this logic, the bottom DO concentrations should be related to more recent wind speed, rather than V7. Could the authors have some explanation about it?

Reply 15: A strong wind event will interrupt hypoxia formation and reset the bottom to a higher initial DO value for consumption. The reformation of hypoxia may takes at least 7 days, but we stress the effects of strong winds on DO happen in the DO raising process, not decreasing process. That means the higher initial DO after wind should be related to the wind speed in preceding days and in this study we choose V7 (7 days averaged wind speed before sampling) to represent the preceding wind speed after the correlation analysis in Table 2.

P11 Lines 247-248. Considering that there is a few records of hypoxia (e.g. 2, 4, and 2 times at three stations, respectively), could the authors also use the low oxygen events (DO<3) in the discussion. This metric only appeared in Table 3 and was never used in other sections, but would be more representative than the hypoxia events. In addition, since this paper is to focus on the wind frequency, I would suggest the authors to explain the hypoxia or low oxygen events from a perspective of wind frequency, rather than the wind speed.

Reply 16: Thanks for your advice. We should use the low oxygen events (DO <3 mg/L) to further verify this conclusion. As for the perspective of wind frequency or wind speed, our objective is to confirm the wind interruptive role, so we focus on the frequency of strong winds, which means we have to set a wind speed threshold and define strong wind events before discussing the frequent wind effects and explaining hypoxia from a perspective of wind frequency.

P12-13 Lines 271-273. *"SM19 appears to be least influenced by the estuarine plume and sewage effluent, and by a wind event due to its deepest depth (24 m). This explains low occurrences of hypoxia at SM19 at wind speeds >5 m/s (Table 3)".* As I understand, the station SM19 is least influenced by wind, the hypoxia events should be more frequent than other two stations. Why the authors attributed the low occurrence of hypoxia in SM19 to the wind? If the reasons are estuarine plume and sewage effluent, what's their relative importance versus wind frequency? Since the authors didn't consider these factors when analyzing the historical data, does this matter for the results and conclusions in this study?

Reply 17: SM19 is least influenced by the estuarine plume and sewage effluent, which means hypoxia events would be less frequent. On the other hand, its deeper water depth makes wind mixing less interruptive to the hypoxia formation. However, as the stratification at SM19 is weaker than SM17 and SM18 with less estuarine plume, the same wind speed may exert more wind mixing at SM19 than at SM18. This can cause the low occurrence of hypoxia at SM19.

A following-up question: Will the threshold of wind speed vary with the bathymetry depth? For example, in deeper waters, the threshold of wind speed will be higher. Since the depth of SM19 is 2-folds larger than in the SM17, is there any significant differences in the threshold of wind speed between these two stations? Furthermore, can this threshold of 6m/s be applied in other stations in Hong Kong waters, in Pearl River Estuary, or even in other hypoxic systems?

Reply 18: The threshold of wind speed is determined by water column stratification, rather than bathymetry depth. As the stratification at SM19 is weaker than SM17 and SM18 with less estuarine plume, the same wind speed may exert more wind mixing at SM19 than at SM17 and SM18. According to Table 3(a), the threshold of wind speed at SM17 and SM18 is between 6 m/s and 7 m/s, while at SM19 is between 5 m/s and 6 m/s despite of its deeper depth.

As these 3 stations are wide open, the wind effects are probably the strongest among other sheltered or semi-enclosed bays. This threshold of 6 m/s may not necessarily be applied to other sheltered waters, but it should be generally applicable to other open waters as the wind-induced mixing is subject to the relevant physical laws.

P14 Lines 299-300. Although the frequency of summer wind events is decreasing, there is no decreasing trends in bottom DO. Could the authors have some explanation and discussion about this? Is there any trend in hypoxia and low oxygen events? Since the authors grouping 29 years into 5 groups, why not calculate the frequency of hypoxia and low oxygen events? Or the authors can count how many stations with hypoxia and low oxygen every year by using all measurements from 86 stations.

Reply 19: The summer wind events showed a decreasing trend from 1990 to 2018, but the averaged monthly frequency is still above 10 days per month in June, July and Septemper, and above 7 days per month in August, which means the wind events occur every 3 or 4 days and are frequent enough to mix the water column and raise the bottom DO since it takes at least 7 days to consume the botom DO to hypoxic level. Based on time series of DO at SM17, SM18 and SM19, hypoxia and low oxygen events are relatively rare, so we did not calculate the frequency of them in 29 years. We can count how many stations with hypoxia and low oxygen every year by using all measurements from 86 stations as you suggest.

P13 Lines 275-298. The authors discussed the effects of ecosystem buffering capacity including physical and biological processes, i.e. monsoons, river outflow, tidal cycles, algal blooms, P limitation, zooplankton grazing, and photosynthesis in bottom waters, on the hypoxia. However, these factor are not discussed in depth. Are these factors important in the Hong Kong waters? If so, the authors should discussed the relative importance of these factors versus wind interruptive effects. And does these factors influence the conclusions if they are considered in the historical data analysis? If they are not important, I would suggest to remove this part as it would be distracting.

Reply 20: This part mainly emphasizes the importance of ecosystem buffering capacity and these physical and biological processes other than strong winds also play a role in hypoxia formation in Hong Kong waters. However, we focus on wind interruptive effects in this study, so we would remove this distracting part as you suggest.

P14 Lines 299-300. The relation between wind events frequency and climate changes are not evident. The authors should provide more discussion or references here.

Reply 21: Yes, we should cite more papers to support this statement as follows. Climate change can induce alterations in wind patterns and storm regimes such as the timing, frequency and intensity of winds, thus modifying hypoxic conditions by affecting the water column stratification, especially in lower latitudes where hurricanes and typhoons are common (Altieri et al., 2014; Conley et al., 2007; Meier et al., 2011; Rabalais et al., 2009).

Figure 1. Could the authors increase the fontsize in Figure 1b so that it can be seen more clearly. In addition, the coastal line of the Pearl River Estuary seems a little different form the google map, e.g. the islands in the south of Macau. Could the authors check it. Finally, a map with longitude/latitude or showing the location of the study area in a larger domain, e.g. south China sea, will be better for readers that are not familiar with this region.

Reply 22: Thanks for your advice. We should check the map and add longitude/latitude and increase the fontsize in figure 1 to make it clear.