General comments

Based on the long-term historical data, this paper tried to investigate the underlying mechanisms controlling the episodic hypoxia events in the Hong Kong waters. The authors identified the frequent high wind as the main factor and archived some interesting conclusions. However, there are still some major flaws making people feel that this is not a scientific paper with sufficient discussion.

(1) The authors analyzed and discussed the historical data from three stations in Hong Kong waters and concluded that the hypoxia in this region was episodic and limited to a small area. However, as a reader, I was also very interested to see results from other stations and was wondering whether these three stations were representative in this region. The authors should have some discussions on this. As mentioned in the Materials and Methods section and shown in Figure 1, there were 86 stations in this region. The authors should firstly show the results from these stations, e.g. the spatial distributions of DO and hypoxia frequency, to identify the most typical stations.

(2) Some major conclusions were not well supported or discussed. The authors suggested that the high wind event was a major mechanism to interrupt hypoxia by breaking stratification. However, results from the Table 3 and Figure3 seems not supportive, e.g. $\Delta\sigma$ is not correlated with the preceding wind speed in summer. And more importantly, the authors didn't have an in depth discuss on relations between the long-term trend of **wind frequency** (not wind speed) to stratification and hypoxia events. Since there was a decreasing trend of wind frequency, why there was no such a trend of bottom DO? And how about frequency of hypoxia/low oxygen events? The authors should have more discussion on it.

(3) As mentioned in the Introduction section, unlike the wind speed and variations, the role of frequent wind events was rarely studied and the authors also mentioned it as the novel point in this study. However, they didn't have a sufficient discussion on the frequency of wind events. On the contrary, they often conducted analysis and discussion from the perspective of wind speed. The authors should be clearer about the differences in wind speed and wind frequency.

(4) The discussion section was not well structured and should be reorganized. The authors used a lot of words to describe some contents which had little to do with the subject of this paper (the wind interruptive effects), e.g. the residence time, the freshwater input and tidal forcing, the sewage effluent, the climate change, and etc. The authors didn't use these concepts to better explain the episodic hypoxia or discuss the relative importance of these factors versus wind frequency. This made the discussion distracting.

Based on the above reasons, I don't think this manuscript is ready to be published and would suggest reject but allow for a new submission after thorough revision.

Specific comments:

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.

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

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.

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?

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?

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.

P6 Lines 126-128. Are there any other mechanisms resulting in the ΔDO 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 ΔDO and AOU? This statement seems to be contradicted with the following conclusion that stratification plays regulating roles in bottom DO.

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.

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

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

P7 Lines 152-154. As shown in Figure 5, the most data points with low $\Delta\sigma$ 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.

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.

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.

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.

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?

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.

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?

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?

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.

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.

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.

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.