Summary:

This study examines whether frequent wind events prevented the seasonal formation of bottom hypoxia in Hong Kong coastal waters based on a long time series of monthly dissolved oxygen data at 3 stations and daily wind speed. The analysis shows that strong-wind events (> 6m/s) often correlate with high bottom DO, and that episodic hypoxic events occur slightly more frequently in August than June and July, coincide with the less frequent strong-wind events (> 6m/s) in August than the other two months. The study thus proposes that a decreasing trend of the frequency of strong-wind events in summer, as suggested by the time series of wind speed data, may explain the system being more prone to developing hypoxia in recent years.

Overall, I think this study tackles an interesting topic and is of value to the community. However, I have some major issues regarding the validity of the method and analysis: 1) whether the 3 stations selected out of the 86 and its temporal resolution (i.e., monthly) are sufficient to testify the hypothesis; 2) other factors (e.g., shallow topography, strong tidal mixing, river discharge, wind direction, warming) contributing to the hypoxia characteristics of this region and the long term trend seem neglected.

Therefore, I would request the authors to address the comments, detailed below, and substantially improve the manuscript.

Major comments:

1. The monthly monitoring data from 86 stations (Figure 1) are public available. What's the rationale of selecting the 3 stations in the southern water of Hong Kong? Are you choosing the stations that are affected most by the nutrient-rich Pearl River plume? If so, the station on the west of SM17 and stations in the northwest of Hong Kong water (west of Lantau Island) might be more representative. Or are these stations the most productive one (based on the nutrient and chlorophyll data that are also included in this monitoring program)? Or did you find these stations experience most severe low-DO or hypoxic conditions?

2. Another major concern is whether the spatial and temporal resolutions of the data (monthly DO data from only 3 stations) are sufficient to make the statement that hypoxia is only episodic in this region and to testify the hypothesis of wind events preventing the development of more severe hypoxic conditions.

2.1. The authors stressed that hypoxia is episodic and spatially limited in Hong Kong waters and seem to suggest that this is also true for Pearl River Estuary (PRE). However, it is debatable whether the hypoxia within and off PRE (including Hong Kong waters) is still episodic at present date. Several summer surveys in recent years (since 2010) have

found quite large area of oxygen-deficient water in the lower PRE and the adjacent shelf waters. Please check Qian et al. (2018) (who presented a synthesized figure including multiple data sets), Su et al. (2017), Lu et al. (2018), and Zhao et al. (2020). I noticed that authors have cited some of these references but described them as "hypoxia in the coastal waters south of Macau". This is a bit misleading as the newly reported low-DO and hypoxic zones from different years in these papers actually extend to include the southern waters off Hong Kong.

2.2. Qian et al. (2018) has presented the same oxygen dataset from station SM18 as in this study and they revealed a significantly declining trend in the bottom DO annual minimum values from 1990 to 2014. I'm not sure why in this study the "bottom DO does not show a significantly decreasing trend" (Line 29-30). Have you conducted trend analysis of the bottom minimum DO for the 3 stations to support your statement?

2.3. If the authors aimed to apply the hypothesis tested here to the entire Hong Kong waters and further to the PRE, I think the analysis based on 3 stations is not sufficient and analyzing the time-series bottom DO at more monitoring stations (which are all available) is required. Otherwise, please state explicitly that the conclusions only apply to a limited water area on the south off Hong Kong. But this will apparently reduce the significance and implications of this study.

2.4. The validity of the analysis based on 3 stations is further limited by the fact that the time series of DO is monthly. The hypoxia could be transient in this very dynamic estuarine system that monthly survey data at three stations may have missed the hypoxic events.

3. This manuscript suggests that frequency of strong-wind events can be used to explain: i) why hypoxia events only occasionally occurred at the 3 stations; ii) why August has comparably higher occurrences of hypoxia events; and iii) why hypoxia events increased in recent years. Can wind speed explain all? I think contribution of other factors to the observed hypoxia characteristics in this region could not be excluded and to what extent they affect your "wind-centering" hypothesis should be discussed.

3.1. Regarding i): Earlier studies have suggested that PRE is not prone to develop seasonal hypoxia despite of high riverine nutrient input (e.g., review by Harrison et al. 2008). An important reason attributed to is the shallow water depth of PRE (5~20m), which enables the wind mixing an effective way to replenish the bottom water. This point has rarely been stressed in the manuscript but very important to distinguish PRE from other hypoxic systems such as Northern Gulf of Mexico and Baltic Sea that authors cited.

3.2. Regarding i): Apart from wind, tide is another important agent to induce strong mixing in PRE. There are modelling studies that showed tides greatly enhance vertical mixing and the replenishment of bottom DO (e.g., Huang et al. 2019). I noticed that authors have

mentioned impact of tide on hypoxia. But the descriptions are in a general sense and not specifically linking to the PRE or discuss in the context of how it will affect your hypothesis.

3.3. Regarding ii): The statement is based on the finding that hypoxic events occurred more often in August (4 times) than June and July (2 times) in 29 years, coincide with the less frequent strong-wind events (> 6m/s) in August than June and July. First, I would ask if this is also true for other stations in Hong Kong waters. Second, I wonder if factors other than wind also play a role in making August more prone to develop hypoxia, such as water temperature, river discharge and wind direction. E.g., August is warmer than June and July and thus enhance water column stratification, reduce oxygen solubility, and/or accelerate the remineralization rate of organic matter in bottom water. River discharge has high temporal variability. Does the region receive more riverine inputs in August than other months? Wind direction can affect the eastward plume extension. Is it possible that August has higher frequency of southwesterly (upwelling-favorable) wind that enables more eastward-directed plume propagation to affect the southern water of Hong Kong? These factors have to be considered before attributing wind speed as the cause of the different occurrences of hypoxia events between August and other moths.

3.4. Regarding iii): Again, the increased hypoxic events in recent years could be attributed to factors other than wind speed and they should be discussed. These factors include riverine nutrients (which have been continuously increasing in the past decades), river discharge, warming etc. Please check Niu et al. (2020) on the trend of anthropogenic forcing. In addition, the monitoring program consists of the long time series data of nutrients and chl data that could also help to look into the contribution of these factors.

4. The Discussion section needs improvement. It currently lacks a clear clue and often jumps from one focus to the other. E.g., Line 199-201 the concept of "ecosystem buffering capacity" comes a bit awkward in this first paragraph discussing the results/analysis of wind impact on hypoxia. Lines 206-230 provides very general descriptions on physical and biogeochemical processes affecting hypoxia formation that read like a textbook. Such information should have been given in the Introduction already. In a section under the name "The interruptive role of wind events on hypoxia formation", you have mixed descriptions on the impact of freshwater, tide, wind, freshwater again, and wind.

Minor comments

Title: depends on how you are going to address the comments above, I'm not sure whether you can broadly say "coastal hypoxia". Currently it's only the 3 stations in the southern waters of Hong Kong.

L25: should explicitly state that the time series of DO is monthly data.

L28-30: “Episodic hypoxia events appear to occur more frequently in recent years” – is this statement based on the recent papers reporting larger low-DO or hypoxic zones? “but bottom DO does not show a significantly decreasing trend” – See my earlier comment. Did you conduct trend analysis? I’m asking as this sentence is not clear in terms what is based on others’ observations or views and what is based on the analysis in this work.

L30: the applicability of the wind speed threshold ‘6 m/s’ should be discussed in the manuscript as this value is based on the analysis at 3 stations in Hong Kong waters that are relatively shallow and have strong tidal mixing. In other system with different water depths and/or strength of tidal forcing, this value might not fit.

L46: a bit redundant to say ‘20% in dry season’ and ‘80% in wet season’. Suggest deleting the former and add the months you referred to as ‘wet season’ in bracket.

L49-50: tide should not be neglected.

L51-57: Be careful of the difference of PRE from other river-dominated hypoxic systems. PRE is shallower that is naturally less prone to hypoxia formation.

L57-61: See my earlier comment. This statement is not accurate.

L70: Want et al. 2015 is not about PRE.

L85-86: See my earlier comment. You might be able to come up with a wind speed threshold that works for the 3 stations. But I’m not sure its applicability for other regions.

L99-101: Do you have evidence or reference to show that SM18 can be affected by the sewage effluents from Stonecutter Island Works in the north during summer? Under the prevailing southeasterly wind in summer, the current direction in Lamma Channel is most likely northward directed.

L127: insert ‘in bottom layer’ after AOU.

L148-150: Not true. V7 does not have significant correlation with the surface-bottom density difference in Table 2. Interestingly, among the 4 variables (DO, AOU, delta DO, density difference), density difference has the least significant correlations with wind speeds (Table 2). Shouldn’t the impact of wind speed be most directly reflected in density difference?

L154: ‘density difference is close to 0’ – this statement doesn’t seem true. None is close to 0 in June, and only 2/3 in July and 1/4 in August are close to 0 (Figure 5).

L255-256: Can you give a bit more explanations on why it takes longer for oxygen depletion to hypoxia in bottled bottom water?

L264: Insert ‘from Pearl River’ after large nutrient inputs?

L269: See my earlier comment. Please provide evidence that the sewage effluent could affect SM18.

L271-272: The two factors you stated have contrasting impact on hypoxia formation. “least influenced by estuarine plume and sewage effluent” means it’s less prone to develop hypoxia, while “deepest depth” means wind mixing is less effective and thus favors hypoxia formation.

L299-300: Any reference to support this statement?

L316-321: These sentences all belong to Introduction rather than “Conclusions”.