Review of Liblik et al.: "Wind-driven stratification patterns and dissolved oxygen depletion off the Changjiang (Yangtze) Estuary"

First, I'd like to express that I appreciate that the authors addressed most of my comments on the previous version of the manuscript. In my opinion, the manuscript has improved quite a bit, although I still see some room for improvement.
In particular, I suggest some re-ordering (and shortening) of parts of the Results section and some clarification and streamlining in the Discussion. In addition, some of the key results that are described in the text could be supported by matching figures to make them easier understandable.
Therefore, I recommend reconsidering the manuscript for publication after moderate revisions. For specific comments and suggestions please see below.

General comments
The entire section 3.1 is very long and provides a lot of details, which at least partly seem not useful to me. In fact, it rather complicates getting the key information relevant for the story from that section. The back and forth jumping between Figs. 3-7 in the text block from line 190 to line 261 further complicates that. I strongly recommend rearranging that section and possibly the corresponding figures, ideally by introducing a "chronological" order of figures and figure referencing (e.g. all maps first, then the section plots). This would make this part much easier to follow.

I really like the analysis described on lines 411-434 as it establishes the strong link between wind forcing and hydrodynamics, and thus the potential for hypoxia in the northern and southern regions. However, as it's only described by text, it is not delivered as strikingly as it could be. A figure could really help improve that. Possibly, a figure showing exactly these grouped velocities (described in the text) plotted against each other could do the trick?

The discussion could be tightened a bit more by more clearly emphasizing the main aspects found in the study. Similar to the aforementioned Results section, it seems to contain a lot of details among which the key links get a bit buried (especially lines 465-530). In consequence, I still have troubles lining up the essential cause-effect chain from the Discussion. Based on existing literature and the present study, it appears clear to me that stratification and primary production together with the bottom depth (which controls the initial subsurface DO inventory) are the essential factors for the formation of hypoxia off the Changjiang. Stratification is controlled by vertical density gradients, which can form either due to fresh CDW near the surface or onshore transport of cold and saline KSSW or a combination of both. Production is controlled by nutrient availability (during spring to fall), which can be provided either from the CDW or the KSSW via upwelling, whose distribution and occurrence are controlled primarily by the wind forcing. Both is nicely shown in this study, but it's not clearly discussed in this causal context The latter point is confirmed by the discussion of the wind-current link in the context of literature, which could provide the basis for a relatively simple hypoxia forecast.
The discussion part on years with wind conditions supporting hypoxia in the northern and southern regions, respectively, based on the analysis of wind forcing and upwelling/downwelling conditions could also be summarized in a table to make it more convincing (and easier to follow). Instead of listing all the years with their different down-

/upwelling characteristics and the potentially matching hypoxia observations (lines 489-504), a table could be provided including, e.g. year, down-/upwelling favorable, hypoxia in north/south, matching observations (with references).

The text descriptions are sometimes a bit complicated and stilted (but generally understandable). Perhaps you can have a colleague (ideally an English native speaker) have a look at the manuscript to make it more concise and accessible?

Specific comments

L78/79: Isn't it DO consumption and vertical supply (via mixing), which are determined by CDW? DO depletion is a result of the two factors.

L97-99: I think the hypothesis is a bit weakly phrased. "Sensitive" suggests that the wind will do something to hypoxia, but your study shows that wind is a key control for the physical environment supporting hypoxia formation. Please rephrase.

L177: Please specify why you chose the 2 mg/L AOU isoline otherwise it appears somewhat arbitrary.

L194/195: Wouldn't AOU (which you also show) be a better indicator for DO consumption? Chl *a* is acceptable as a proxy for production (although it comes with some limitations).

L204/205: This sentence seems to contradict line 194

L196-261: Please describe figures (and panels) chronologically.

L311: River discharge is a buoyancy flux, yes. But is it that buoyancy flux that causes the southward flow or is it the barotropic pressure gradient (imposed by the river discharge) that causes the geostrophic southward flow?

L315-334: I would start this paragraph by stating that two factors can be considered to affect the distribution of CDW in the region: Changjiang River discharge and wind forcing.

L333/334: Given the different survey timing, it is very likely that seasonality/annual cycle of the wind forcing is important. I wouldn't phrase that as a hypothesis.

L364: Perhaps you can start a subsection here as the following parts provide an analysis on larger time scales (i.e. not only based on the two surveys).

L364-370: My understanding of this paragraph is that you calculate cross-correlations for different time lags (shifted by one day each) and wind in the different directions? Is that correct? Please clarify in the text what you did exactly and which lags you tested.

L398: You previously highlight the role of wind direction, while you refer to wind stress here. How do the periods of low wind stress relate to wind direction? Based on Eq. (1), my understanding is that low positive stress means weak northward winds; it could be useful stating that here to make the description less abstract.

L412: I don't think "quantify" is the appropriate term here. Isn't it rather an extrapolation of your survey findings to a longer time period? Or in other words, you're putting the survey observations in a broader context.

L420: Technically, the current is already "altered" if it's slowed down (or sped up). Maybe rephrase it such that you 'define' v_m = 0 as current 'alteration' and that you use this term accordingly hereafter.

L433/434: This is a key message that should be made more clearly, like: "This supports our hypothesis that the wind field is a key control for the direction of the Chinese Coastal Current controlling the CDW distribution off the Changjiang River Estuary, and thus hypoxia formation."

L439-444: Perhaps you could start the discussion with a statement that the balance between oxygen consumption in and supply to the subsurface layer controls the formation of hypoxia, and that consumption is controlled primarily by primary production and subsequent organic matter degradation, while supply depends on vertical stratification and lateral transport. Then you could state that your study focuses on the effect of the wind field these supply pathways.
L445-504: I think the general logic of this part works well, however, I recommend tightening it a little bit to make it more concise and easier to follow. State what the key patterns from the two cruises are and what they suggest with respect to the role of the physical environment for hypoxia formation (and to some level productivity, but you don't need to put as much emphasis on that as it's not the main focus). Then discuss the findings from your wind analysis and how the wind field controls the physical environment in the northern and southern regions (and thus the likelihood of hypoxia in both regions). Then you put it in context with observed hypoxia to support your findings.
L456-458: I do not agree with this statement. Yes, the wind conditions were different during the two summers, nevertheless, your observations were at too very different stages of the summer season. Thus, they cannot be compared in this way and they do not reflect inter-annual differences but seasonal differences.
L463: I think the role of the surface current in distributing the CDW (Fig. 11) is the essential ingredient, and should be stated here. The strong haline stratification limits the vertical DO supply in the first place. In case of a deep KSSW intrusion, the additional thermal stratification then defines the potential vertical extent of hypoxia.
L509-510: I don't understand this statement. If river discharge accounts for 80-90% of the coastal current, why does that mean that most discharge does <u>not</u> remain in the river plume? Shouldn't it be the exact opposite as the coastal current determines the distribution of the river plume?
L561-562: I am not fully convinced that it is KSSW along the N17 section (Fig. 5) as temperature and salinity of the subsurface waters along this section (Fig. 5) differ from the other sections. Could it also be water originating from the Taiwan Warm Current? Maybe you could state earlier in the manuscript (e.g. on pages 3 and 5) what the physical properties (T and S) of both water masses are to make clear that it can only be KSSW (if that's the case).
L578-580: Same as the previous comment with respect to KSSW.
L604/605: Why does this mixture promote organic matter settling? Or do you mean primary production (since you mention nutrient consumption) and subsequent settling?

Technical corrections
L24: the pycnocline
L26: the manuscript already uses a bunch of acronyms, I would avoid using CCC (throughout the manuscript)
L28/29: a well ventilated area in the north and a hypoxic area; the CCC; reversal of the
L31: offshore transport; a subsurface intrusion
L32: shallow areas (<10 m depth) at the continental slope
L33: in the north
L64: In the literature, hypoxia off …
L65: divided into; state where the division between northern and southern region is usually done (around 30°N); features a shallow
L66: bottom, while

L69: The region is strongly
L74: by a shallow
L77: in consequence
L81: compared to that of wind stirring
L86: are further influenced by
L87: no need for CCC and TWC (rarely used); n comma after (CCC); , which originates
L88: at the surface, at the bottom
L92: remove “formation”
L98: to the hypothesis that hypoxic
L100: Zhang et al. (2018) recently demonstrated that; and, as a result, location
L101: are variable
L102: do not
L104: in a more general
L106/107: remove “It is clear”; result in higher DO consumption rates
L114/115: in the observed spatial patterns of temperature, salinity, chlorophyll *a* (Chl *a*) and DO
L128: inside the river estuary
L129: At all stations, vertical
L134: cruise, Chl *a*
L140: state that Chl *a* was not sampled in 2017, which is why satellite-derived Chl *a* is used
L143-145: remove this sentence and only state how sigma was calculated in the figure caption where sigma is used (Fig. 6a, b).
L147: for the estimation of the spatial extent of CDW
L148: remove “well”
L151: Could you provide a link to the Copernicus Marine Service website?
L152: Large and Pond (1981).
L153: What are rho_air and U?
L164: during the reference period
L177: Avoid using UBD. Instead refer to the isoline in the text later on
L178: add references for the different criteria; originates
L179: Avoid using UBK. Instead refer to the isoline in the text later on
L182: in Xu et al. (2018); used in this study
L183: The width
L186-187: define “g” and the alpha in the equation looks odd
L192: no abbreviation in title
L193: observations, we analyse
L194: distributions observed in the summers of 2015 and 2017
L196: Here you discuss the 25 (psu) isoline, but in the figures you show the 30 isoline. Please show the one you need for the description
L198: In 2017, the
L199: Water fresher than 25 was …
L202: The sea surface temperature; in 2015 (Fig. 3c) than in 2017 (Fig. 3d).
L204: remove “(Fig. 3c-d)”
L212/213: The calculation of the density difference is bottom minus surface value.
L214: in the north
L215: south of the
L216: and a cold and salty

L218: (Fig. 6a) as the dense bottom layer water
L222: of the coastal slope; Downwelling had pushed
L223: Probably, onshore transport
L311: causing the current
L314: in the further discussion
L315: Comparing the two years' discharge
L319: remove "we"; remove "(wind forcing)"
L322: According to Eq. (4), the width …
L324: 55 km for the inflow
L325: remove "were considered"; "partly" instead of "somewhat"; explains
L329: could have caused the differences between
L335: As described previously
L336: before the sampling
L339: toward
L340: did not prevail
L342: "Probably" instead of "Likely"
L348: Maybe "subsurface" instead of "deep layer"?
L353: This is the combined
L355: such phenomenon did not occur/prevail
L358: An opposed gradient; coastline, was
L362: currents toward
L365: You write "First", but there's no "Second"
L370: correlates well
L379: occur on shorter time scales, probably cause
L381/382: move "also" after "likely"
L384: remove "well"; flatter
L385-391: remove sentences about eddy, it's too speculative and distracts from the main story
L394: We further analysed
L396: and a northward; should it be "t_c>>0" to distinguish from weak northward winds on L398?
L402: The CDW was mainly transported
L405: flow
L407: include reference to figure (panels) that show this northeastward spreading
L425: explains why
L426: is required
L430: refer chronologically to panels of Fig. 12 (currently a, c are referred to after b, d), maybe just rearrange the panels accordingly
L442: accompanied by; the spreading; nutrient-rich
L449: remove the text in the parentheses, it's a bit confusing
L451: Southeasterly winds and higher discharge during summer monsoon both favor …
L454: late August/early September; and are reflected in
L455/456: only use "On the other hand" if you used "On the one hand" in an opposing statement just before; Wind speed and discharge were close to average during summer 2017, while wind speed was much weaker in 2015
L457: remove "concurrently"
L462: remove "paper"

L463: also related; the corresponding surface
L465: The monthly occurrence […] September 2018 (Fig. 11) clearly shows that CDW …
L467: one can see that 2015 differs with low values
L468: maps
L469: do not; in the north
L470: compared to; occurred in the south in October; include a reference for this deterioration; This demonstrates that the wind
L474: close to the long-term; will more likely occur east of the
L478: than the long-term
L480: "extent" instead of "spreading area"
L492: 2018, stratification
L502: and, according to the latter factors, estimate
L506: fate; separated by regions; What do you mean with bulge?
L507: downstream the coastal current
L509: of the coastal current
L519/520: remove statement on eddy
L524: was also observed by Yang et al.
L528: nitrogen-to-phosphorus (N and P have not been introduced)
L536: by a typhoon
L539: a valuable time series of DO in the near-bottom layer
L543: of the KSSW as DO declined
L550: The importance
L551: is also revealed when near-bottom; maps of vertically integrated AOU
L552: compared to
L572: holds according to our analyses
L577: In the latter area, KSSW was present but there …
L584: effect of the thermocline
L585: add reference for previous oxygen depletion of subsurface water
L588: compared to
L590: is an important
L591: lateral DO supply
L592: nutrient-rich
L593: sinking causes DO consumption
L598: only use "On the other hand" if you used "On the one hand" in an opposing statement just before
L601: not as pronounced
L602: The continuous; was demonstrated by; "monthly" is not really " continuous"
L604: Second
L605: on its way south
L606: compared to the northern region
L607: "penetrate" or "diminish" instead of "destroy"?
L608: remove "In short"
L617: by KSSW intrusion or CDW spreading
L619: below the pycnocline
L620: Both distribution of CDW and KSSW and the occurrence of KSSW upwelling are controlled by the wind field.
L621: alters the Chinese Coastal Current by creating an Ekman surface flow

L623: on the coastal slope, resulting in upwelling
L625: In consequence, the
L633: remove sentence starting with “Concepts”
L634/635: Our results further suggest that a combination of wind field data, remotely sensed sea-surface salinity and sea level could be used to forecast the hydrographic conditions and potential hypoxic area prior to field work. (NOTE: You don’t really need river discharge if you have sea surface salinity information.)
L648: led
L647: during the 2015 cruise

Fig. 1: add “depth (m)” label to color scale; the cross with the wind location, the large circle with the mooring and the sections N15/N17 are barely visible in gray-scale
Fig. 3: Is it on purpose that excluded stations (in the estuary) are shown on the maps for 2017 but not for 2015? Change order of isolines in caption as you first show salinity
Figs. 4/5: Maybe change order of first and second row as you first describe S and then T in the manuscript. I suggest adding a few more increments to the color scales (e.g. 20, 21, 22, 23, 24, 25, 26 for temperature), perhaps you can then match the number of colors in the panels to the increments (then you could also remove the green lines, which I find a bit confusing). Maybe use longitude for the x axes to allow for easier match-up with the locations in Fig. 1. I am a bit confused by the fact that the number of station locations (small crosses) in the first row doesn’t match the number of stations along each transect in Fig. 1
Fig. 8: The red line for the current measurements is barely visible in gray-scale. Positive and negative values in the last two panels are indistinguishable in gray-scale. Add units to the color scales. I suggest removing the longitude/latitude information for all panels except the bottom-left to save some space between panels (that would allow for larger panels). Use white space between “cm” and “s” in the units in the caption.
Fig. 9: at section S1 (see Fig. 8); use white space between “m” and “s” in units of axes
Fig. 11: I suggest removing the longitude/latitude information for all panels except the bottom-left to save some space between panels (that would allow for larger panels). Why do you use g/kg as salinity unit in the caption? In the methods you wrote it’s practical salinity scale. Better be consistent. If the satellite product is in g/kg, you should convert to PSU..

Table 1: I don’t find this table very useful. I think you could remove it.