

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

The authors present an analysis of dissolved oxygen (DO) concentrations and hydrography off the Changjiang Estuary observed during two summer surveys in 2015 and 2017. The analysis provides valuable insight in the role of freshwater discharge from the Changjiang and more importantly makes a strong case for the effect of wind forcing on (a) water mass transport/distribution and stratification and (b) coastal upwelling, both affecting the formation of hypoxia in different parts of the region.

As such, this study adds to the evolving knowledge base on hypoxia formation in the East China Sea and is worth considering publication.

However, my major issue with the manuscript at hand is that it is often hard to get what findings are based on what results (due to a lack of figure description and cross-referencing). This leaves me with the impression some of the figures are unnecessary and should be removed. In consequence, the conclusions appear a bit limited and more of a summary of what has been done despite this relatively large number of figures.

Apart from that, the manuscript almost exclusively considers and discusses the physical controls of hypoxia, although it is shown by earlier studies and the one at hand that the physical environment (i.e. stratification, which limits DO supply from oxygenated surface waters to deeper layers) only provides the frame for hypoxia. DO consumption (represented by AOU in this manuscript), driven by primary production and subsequent organic matter degradation, is required for its formation. The described upwelling plays an essential role for enhancing primary production (and subsequent DO consumption), yet this is barely mentioned.

Therefore, I recommend reconsidering the manuscript for publication after major revisions. For specific comments and suggestions please see below.

General comments

A large fraction of the figures are not (explicitly) used/described in the Results section, which in parts makes it difficult to follow. In most cases, it is not stated why a specific figure is shown. For instance, what is the purpose of comparing satellite sea surface salinity (SSS) with the mooring time series (Fig. 2), while the mooring data is not used in the rest of the manuscript, nor is satellite SSS? Figures 4 and 5 (transects) are not mentioned explicitly, although they clearly show some important vertical patterns, like the subsurface hypoxic layer reaching almost up to 5m depth (Fig 5). However, this is only mentioned in the discussion. Figures 11-13 are also only mentioned in the Discussion, which is too late. All figures that are relevant for the manuscript need to be described in the Results section in order of their appearance. Irrelevant figures should be removed. It's hard to tell for me, what figures are really important because of the partial lack of description; possibly some panels could be removed from Figs. 3-5.

The AOU figures (although only very briefly discussed) clearly show the important role of DO consumption driven by organic matter degradation for hypoxia formation. However, this factor is only briefly mentioned in both introduction and discussion. Figure 5 shows a distinct increase in AOU in the subsurface from offshore toward the coast. This strong increase indicates that the DO minimum in the near-shore subsurface area is formed via local organic matter remineralisation, although the reduced DO concentrations in the offshore subsurface

waters suggest that the water is preconditioned for hypoxia formation. These things should be discussed as well, as the physics alone cannot explain hypoxia formation. If the survey data contain information on variables that can be used as indicators for organic matter production (e.g. chlorophyll-a/fluorescence observations or nutrient concentrations (as an indicator for enhanced nutrient supply via upwelling), I strongly recommend showing results for the indicator variable(s), e.g. as plots of surface concentrations or vertically resolved transects. This would strengthen the results and discussion, and provide a good connection between physical environment, productivity and DO depletion.

The conclusion reads as if the key findings are (1) that stratification must be present for hypoxia to form (which is nothing new) and (2) that there are two modes of salinity (i.e. stratification) and DO distribution patterns, which are controlled by the prevailing wind field (which is new). The latter is a very interesting finding, however, in terms of conclusions it would be absolutely worthwhile to raise the implications of this finding, e.g. for survey planning, or even for the potential development of a hypoxia forecasting system. Considering that SSS and wind information can be obtained with relatively high spatio-temporal resolution, a combination of both with the findings of the study could be used to make spatially resolved forecasts on likely occurrences of hypoxia, which in turn could be used to optimise hypoxia survey design. The only limitation I see here at the moment is that the connection between wind field, SSS (or Changjiang Diluted Water) and hypoxia is based on only two years, with some additional support from the literature (see Discussion). However, if the authors were able to match up a few more years of high wind stress (according to their study) with corresponding hypoxia patterns, this could at least point into the right direction and provide directions for useful future research. The authors should furthermore discuss what role the different survey timing played for the differences in the observed features as the 2017 survey took place at the beginning of the winter monsoon, while the 2015 one was done in the middle of the summer monsoon. This obviously has a strong effect on the wind field.

Figures: Many figures are not legible in grey-scale, i.e. for colorblind people. I strongly recommend using perceptually uniform color scales, which are available for R, MatLab and Python (and other languages; e.g. <https://github.com/matplotlib/cmocolor>). Color references in figure captions should be avoided for the exact same reason. All figure captions must state clearly what data is shown (e.g. over what period they were averaged etc.)

Specific comments

Title: I suggest removing "in the area"

Abstract: The abstract should be rewritten, such that the key messages can be understood without reading the entire manuscript. At this point, it is unclear what the "interaction zone" (line 25) between upwelling and surface freshwater is meant to be. High AOU furthermore does not necessarily mean "high DO utilization there" (i.e. local consumption; line 24), especially in the case of advected/upwelled subsurface waters, which are likely to be already undersaturated in DO. Discuss the findings chronologically (first 2015, then 2017).

Introduction:

Lines 50/51: nutrients lead to production, which in turn leads to sinking/sedimentation of organic matter. This is an important aspect, which should also be more emphasized in the discussion (and the results, if possible with the survey data; see my general comment).

Lines 92-94: Again, what about the influence on productivity? Upwelling brings nutrients into the euphotic zone, significantly enhancing organic matter production.

Lines 99-101: I would remove this paragraph.

Data and Methods:

Line 109: There is a 1-month difference in the survey timing between the two years. It would be nice to include a statement in the discussion to what extent relates to the seasonality in the monsoon cycle and if/how it affected the differences between the observations in both years.

Line 110: state the number of stations you used for both years

Lines 119-121: Since you do not describe Fig. 11 (until the discussion), do you need to show/discuss it at all? If not, remove the description of the satellite product and Figs. 2 and 11. If you need it, what is the spatial resolution of the satellite product and how does it match in-situ spatial patterns observed during the survey?

Figure 1: The labels of transects S15/S17 are barely visible. Remove color references in caption.

Lines 122-128: Is the used wind forcing the same that the GLORYS model uses? Please specify. If it's not, I advise using the same one.

Lines 132/133: This is a result and should be stated in the Results section. Where is that shown? Include reference to corresponding figure.

Lines 134-142: Please state why you are analyzing AOU. E.g. to illustrate the role of DO consumption.

Results:

For the entire section: please refer to figures and figure panels where appropriate. Right now it's really hard to know what figure (panel) to look at due to minimal cross-referencing.

Lines 151/152: Please state why you analyse the spatial patterns (e.g. to put the DO observations in context with the physical environment).

Line 154 and Fig. 3: In the figure, you show the 31 isohaline, here you refer to the 30 isohaline. Please be consistent. I suggest using the 30 isohaline in the figure.

Lines 164-168: You "describe" 3 figures on 5 lines. It is not possible to understand what finding is based on what figure. It also gives the impression that 1-2 of the figures are superfluous.

Figures 3-5, 6a-2/b-2: Don't use the jet color scale, dark red and dark blue are indistinguishable in grey scale (i.e. for colorblind people). Use standard panel labels, i.e. a, b, c, d etc. (not a-1, a-2, b-1, b-2, ...)

Lines 190-192: Upwelling creates favourable conditions for organic matter production (primary production), which then drives DO consumption. You should not skip this process. If you have chlorophyll (or nutrient) data, you should show them to illustrate this.

Line 195: What do you mean with "a certain physical property of water"? Do you mean temperature or salinity or else? Please specify.

Lines 196-199: How did you determine the 2 mg/L AOU-cline to be the "oxycline"? And why do you use the AOU isoline to define your oxycline and not oxygen in the first place? An oxycline is defined by a strong gradient in DO, not by AOU and not by DO depletion (and you are not using a gradient either), so the term cannot be used here. The 2 mg/L AOU isoline further doesn't seem to match the oxycline nor the upper boundary of DO depletion in 2015 (Fig. 4). And what is your basis for using the 24.5°C isotherm to represent the thermocline? A thermocline is also defined by a gradient in temperature, not by a fixed temperature; it doesn't seem to represent the thermocline at transect N15 (Fig. 4). I suggest calculating the pycnocline/oxycline using a gradient approach (e.g. strongest vertical density/DO gradients), which would be much more objective than just picking some isolines. If they still match (which I expect being the case), this would strengthen your statement that the pycnocline determines the upper limit of DO depletion (although this is not really new).

Lines 200-208: I wonder if Fig. 7 adds a lot of information that cannot be obtained from Figs. 4 and 5? The spatial gradients of thermocline depth indicate the strength of upwelling, which can be described using the transects. Same applies to the statement on AOU and the effect of the thickness of the hypoxic layer vs. the DO concentrations. Although I am not sure why this statement is important? In addition, both factors determine AOU and, at transect C15, a thick DO depleted layer coincides with very low DO concentrations, which makes it difficult to quantify what factor is more relevant. I suggest removing this last statement, otherwise the contributions need to be quantified (which does not add to the story).

Lines 243-246: Please provide the equation you use to do this calculation.

Lines 249-254: Here, you possibly could mention the monsoon cycle (in relation to the differences in survey timing) as the 2015 survey took place at the end (beginning) of the summer (winter) monsoon. Then you could also be a more specific with respect to your hypothesis on the main cause for the 2015 vs. 2017 differences.

Lines 256: Does Fig. 8 show wind and currents averaged over the 7-day periods before the surveys or the only averaged over the single day 7 days before the surveys? Please clarify and also clarify in the figure caption. If it's averaged over the single day only, why do you use that one and not the 7-day averages?

Lines 268-270: Is this sentence relevant? Does the negligible bottom current have an important effect on the observed patterns? If yes, clarify. By talking about a buoyant current you also imply that it's baroclinic.

Lines 271-277: Did you calculate the mean winds at the same location as the currents? Please clarify. Further state which different wind directions you used to calculate correlations and to determine the best one. Possibly state the highest values of the other correlations, too, in order to illustrate the difference between the SE-NW direction and the others.

Lines 303-305: I am not sure I understand this statement. You define wind velocity intervals of 0.25 m/s width and average the corresponding current velocities simulated by the model, right? Perhaps you can explain this more clearly. Also, do you do this for every model grid cell in the area of interest or just for the location marked in Fig. 8?

Discussion:

Lines 335-338: You refer to Ekman transport, yet you do not show any analysis of it. You do show near-surface currents (0-5 m depth; Fig. 8), however, the Ekman depth (over which you would need to average/integrate to get Ekman transport) is likely deeper than 5 m and will vary depending on the wind speed. It would indeed be nice if you showed the Ekman velocities in order to illustrate the upwelling conditions.

Lines 339-355: Figures 11-13 have not been mentioned before here; you need to do this in the Results section. It is not possible to follow this discussion without prior description of these results. I also do not see the benefit of discussing 2016 since you do not provide information on the DO conditions in that year. The last sentence in this paragraph is entirely speculative, unless you provide information on the DO conditions in 2016 and 2018 (e.g. from other studies if available).

Lines 360-368: This qualitative comparison of the findings of this study with existing literature is very useful and it suggests that the wind stress (in combination with river discharge) could possibly be used for the development of a simple forecast of hypoxia occurrence using remotely sensed SSS and wind information. This could be discussed, although it may need support by more examples than only the few years mentioned. There is some more literature on hypoxia observations in the East China Sea, which the authors may want to check, e.g.:

Zhu et al. (2011) <https://doi.org/10.1016/j.marchem.2011.03.005>

Zhu et al. (2017) <https://doi.org/10.1016/j.marpolbul.2017.07.029>

Li et al. (2002) <https://link.springer.com/article/10.1360/02yd9110>

If the patterns described in these papers match the “wind-based likelihood” of hypoxia as suggested by this study, the authors could make a case here, which could provide a good direction for future work.

Line 363: “probably occurred” is too speculative. Either you have support for this statement or you should rephrase it, e.g., to “could have occurred”

Lines 377-379: Irrelevant. Remove the whole paragraph

Lines 381/382: The wind-driven near-surface transport offshore is the cause for coastal upwelling of subsurface waters. Please rephrase. The term “upwelling-CDW interaction zone” is not very clear. I understand what you mean, but I would suggest not using this term as it is a bit misleading. The two water masses do not really *interact* with each other, it is rather a displacement of CDW and its replacement by upwelled water.

Lines 386-390: This is one of the few occasions where the role of primary production and nutrient supply is mentioned. This should be expanded and, if possible, strengthened with observations of chlorophyll or phosphate in the Results section.

Lines 396-397: This could be shown more clearly by drawing oxycline and thermocline in Figs. 4 and 5. I think Fig. 7 is unnecessary.

Lines 440-444: None of this analysis is shown/described, so it cannot be discussed. If you want to make a statement on potential future changes (or no changes) due to wind, you need to show a figure comparing future wind projections with current winds. Using only one projection from a single model further doesn't allow for such a strong statement. Please provide references for projected increases in SST and eutrophication if you want to keep the statement.

Conclusions:

The conclusions in general are too weak and rather a summary.

Lines 452-454: Why is the statement on the inclination important?

Technical corrections

Line 21: Changjiang Diluted Water (CDW)

Line 26: patterns; are prevailing; summer

Line 29: while the

Line 32: likely the main determinants of the

Line 47: “DO” needs to be introduced here (first time used in main text)

Line 53: below the

Line 55: Hypoxia is a

Line 57: related to eutrophication

Line 62: is characterized by; a steep slop

Line 63: occupy the southern

Line 70: below the

Line 71: the pycnocline

Line 77: are enhanced

Line 79: the Chinese Coastal Current; remove “(TWC)”, you don't use it in the manuscript

Line 83: East China Sea

Line 90: hydrographical; in a particular summer

Line 91: in more general context

Line 92: The hypothesis

Line 94: also cause

Line 96: describe

Line 97: during two cruises in the summers of 2015 and 2017; explore

Line 98: investigate

Line 100: related variables
Line 114: for 2015 and 2017 were analyzed
Line 122: “resolution” instead of “grid size”
Line 125, Equation 1: It looks like capital “i” letters in the equation? Should it be the norm of U (i.e. $|U|$)? Please correct.
Line 126: add “above sea level” at end of sentence
Line 128: with approximately 8 km ($1/12^\circ$) resolution
Line 135: present; apparent oxygen utilization
Lines 135-142: Make sure the font style is used consistently throughout the manuscript for AOU (either italics or normal)
Line 150: Provide a more descriptive title for the subsection, e.g. “Spatial patterns of hydrography and oxygen”
Line 154: The practical salinity scale is a unitless scale, i.e. don’t write “psu” here and throughout the manuscript.
Line 156: the 25 isohaline
Line 158: has been prevailing
Line 160: The sea surface temperature (SST)
Line 161: avoid the reference to the Zhoushan Islands or mark them in Fig. 1, otherwise the reader not familiar with the region does not know where to look
Lines 162/163: “... in both years, but colder waters covered smaller areas in 2017.” The other part of the sentence is a repetition.
Line 169: Spatial patterns in stratification are described (you don’t show changes). What equation of state did you use to calculate density?
Line 172: in areas of 20-30 m bottom depth; no blank after 124
Line 180: in areas of 25-60 m bottom depth
Line 187: less saline OR low-salinity
Line 194: in regions of different depths
Lines 198/199: State the exact number of profiles used for the calculation (i.e. “n = xyz” and not “n < 44/34”)
Line 200: and below DO depletion can develop.
Line 201: of both clines
Line 210: summarized in Table 1.
Line 212: What do you mean with “quantified forcing”? Please clarify.
Line 236: during the week
Line 238: what exactly do you mean with river plume **bulge**? It’s not clear to me.
Line 239: B should be italicized in the text
Line 240: Add the unit of f (1/s)
Line 241: in 2015 and 2017, respectively.
Line 243: of the buoyant
Line 249: Several indicators (remove “While”)
Line 257: The mean wind direction
Line 259: is clearly visible
Line 279: the near-surface offshore flow
Line 283: according to Eq. (1), your τ can be negative. In this case, your u^* would be a complex number. Please use $|\tau|$ in the equation for u^* ; use ρ for density not q
Line 286: which occur on shorter time scales probably causing deeper mixing.
Line 305: introduce w_c

Line 308: What/where is S1?
Line 327: In the upwelling region
Line 329: remove "As we have argued"
Line 339: In Fig. 11, we ...
Line 340: 2015 differs
Line 384: one blank too many before Yang et al
Line 395: hypoxia was terminated
Line 398: published a valuable
Line 399: time series; please change throughout the manuscript
Line 406: Fig. 2; In these cases, DO
Line 409: at the coastal
Line 412: such as
Line 413: spatio-temporal resolution

Figure 1: use different colors for transects S15 and S17

Figs. 3-5 use different colors

Figures 4/5: State in the caption what the crosses in the top panels are. Use same color scale for oxygen as in Fig. 3 (0 to 8). Draw thermocline (or pycnocline) in oxygen and AOU panels. Remove remnants of scale labels on left side of top right panel in Fig. 5.

Fig. 6: add labels to panels stating the shown quantity; change units for density difference to kg/m^3 ; remove last sentence in caption and state that AOU was calculated according to Eq. (3) instead; plot hypoxia outline (i.e. 3 mg DO L^{-1}) in all panels; use different colors for DO

Fig. 8: Clarify the averaging period and include the depth ranges for the currents in the caption. Remove last sentence in caption

Fig. 9: remove sentence with histogram; add label to color scale

Fig. 10: use colors that are better distinguishable in grey scale; only show legend in first panel and only use "2015" and "2017" in the legend

Fig. 11: use different colors; red areas are not identifiable as high-frequency areas in grey scale; increase size of color scale and add label

Fig. 12: use thicker arrow lines (like in Fig. 8); include the depth ranges for the currents in the caption.

Fig. 13: Use different line styles and for easy distinguishing of means in grey scale