Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-421-AC3, 2019
© Author(s) 2019. This work is distributed under
the Creative Commons Attribution 4.0 License.

Biogeosciences Discussions

Interactive comment on "Wind-driven stratification patterns and dissolved oxygen depletion in the area off the Changjiang (Yangtze) Estuary" *by* Taavi Liblik et al.

Taavi Liblik et al.

taavi.liblik@taltech.ee

Received and published: 30 December 2019

It seems that supplement appears as dead link. I add specific responses to Dr. Große here.

Reply: Thank you for your careful reading and comprehensive review! Manuscript has been improved thanks for your suggestions/comments. We have taken into account most of your comments in revising the manuscript. Please notice that in the replies we provide texts from the draft of revised paper. There might be minor changes (related to language) in the final revised paper submission.

Comment: 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.

Reply: We agree with the critics on referencing and description of figures and we have dealt with the issue. We also agree conclusions needed rewriting. Yes, this is true, we focus on physical processes impacting hypoxia. We believe, the physical environment and its sensitivity to forcing is extremely important for hypoxia formation here. Stratification is just one, but not only aspect. Advection of the diluted water is driven by physical processes, as well is deep water intrusion. Both are required for the existence of hypoxia and both depending on wind forcing. Of course oxygen consumption is required to have hypoxia, but advection and diffusion are important as well in oxygen budget. Likewise, it is important, where all this oxygen consumption driven by primary production happens. Latter is driven by physical forcing. We agree DO consumption and related processes must be more highlighted in this paper. But the focus of the paper stays as it was.

Action: In the revised manuscript citing and description of figures is much more com-

Printer-friendly version

Discussion paper

prehensive. Fixing has been done in the entire manuscript, particularly in the results section. Figures were cited 53 times in the original manuscript, and now reaching 152 times in the revised document. We have rewritten conclusions according to your specific comments.

We have improved introduction to show the focus of the paper more clearly. Primary production and consequent organic matter in the context of consumption is now much more highlighted in the manuscript, particularly in introduction and discussion. Likewise we mention this now in results and conclusions. We have added chl-a maps to give background information about the initial causes of consumption, but we keep our focus on wind forcing and related effects on oxygen fields. Otherwise we rely on existing papers, which have dealt with biogeochemical processes and have shown well the importance of the CDW and upwelling on oxygen consumption in this region.

General comments Comment: 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.

Reply: We agree the critics about description of figures and results. Sea surface salinity was used both in results and discussion part of the manuscript (Fig. 11). We agree, figures 3-5 were not described well enough in detail in the original manuscript. All

Printer-friendly version

Discussion paper

figures except figure 13 are mentioned in the results.

Action: We have extended the manuscript to give much more details about these figures, and each subplots have been separately cited in the context. We also considered the suggestion to include figure 13 in results part, but its major content is very related to earlier studies. Thus, we think it is better to start using it in discussion. Otherwise citing to figures is more extensive in the revised manuscript.

Comment: 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 nearshore 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.

Reply: We agree, the background of the do consumption was not well enough mentioned in the previous version of manuscript.

Action: Features in vertical sections are described much more detailed in the revised manuscript. Likewise, information related to consumption is more highlighted in introduction, results, discussion and conclusions. We have added chl-a to the manuscript (to fig. 6) and described it. However, we believe that consumption related processes

Printer-friendly version

Discussion paper

are quite intensively studied and going deeper into that topic in this paper is out of focus (as wind forcing has been out of focus in many other studies, which have dealt with link between primary production and hypoxia). The preconditioning of offshore water aspect is now included in the discussion.

Comment: 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.

Reply: Thank you for the good suggestion. Action: the first point about stratification, this is much more specific in the revised manuscript (mainly because of reviewer 2 comment). It says now "Pycnocline created by Kuroshio subsurface water is precondition and determines where hypoxia could develop". There is much more analysis dealing with this issue thorough results and discussion section in the revised manuscript. Your recommendation for conclusion is included now: "There is a strong connection between the upper boundaries of Kuroshio intrusion and oxygen depletion. The sensibility of the boundaries to wind forcing shapes oxygen conditions considerably in the area. Autonomous measurement campaigns by mooring arrays and underwater gliders could considerably improve the knowledge about related processes. Concepts suggested in the present work can be utilized, when planning in-situ experiments. Wind, river discharge, remotely sensed salinity and altimetry data can be used to forecast hydrographic situation and potentially hypoxic areas prior field works."

Comment: 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 connection between wind field, SSS (or Changjiang

CC BY

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.

Reply: Thank you for good suggestions. Action: We have added more literature examples to discussion. And we have added a subsection to the discussion about survey planning. "Thus, when planning hypoxia related measurement campaigns in future, it is worthwhile to take into account wind-driven transport, river discharge, remotely sensed salinity and altimetry to forecast spreading of the CDW, upwelling occurrence and deep water intrusion and according to latter factors estimate potential hypoxic area prior to field works. This could allow more efficient use of ship time and more detailed sampling of the hypoxic area. "

Yes, timing is important. In the original manuscript, the first section of discussion was initiated by the timing question. But somehow the most obvious fact (which you mention) we did not say out. It has been made in the revised manuscript. In short, timing matters, but the wind conditions in 2015 were not only related to the beginning of winter monsoon (you have mixed years in your comment), but the whole summer stands out times-series (Fig. 12) as with weak summer monsoon. This reveals also in Fig. 11, where we show the occurrence of CDW (by remotely sensed salinity). As a result hypoxia developed in south. We have added an additional reference, where the surveys were also from July and October in 2015. There was more oxygen depletion in north in July, but not hypoxia and very strong hypoxia in south in October. We have added "Thus, our observations conducted in late August - early September 2015 and late July 2017 illustrate the annual cycle of forcing and latter reflect in oxygen and stratification

CC BY

patterns. On the other hand summer monsoon and river discharge were close to average in the whole summer of 2017 while the summer monsoon was clearly weaker in 2015 (Fig. 13a-b). Thus, our observations reflect also the differences in forcing and concurrently the DO distributions between two summers." And "It is clear from fig. 11 that CDW transport offshore (to northeast) has occurred in all years, including 2015. However, one can see how year 2015 differs with low value in the inter-annual time series of wind stress of τc $\geq$ 0.02 N m-2 (Fig. 13a-b) and with considerable southward advection of the CDW (Fig. 11). Monthly mappings of bottom oxygen in 2015 (Li et al., 2018b) does not show significant oxygen depletion in north in any month while deteriorated hypoxia (comparing to our observations) occurred in October."

Comment: 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/cmocean). 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.)

Reply: Thank you for bringing this up.

Action: We changed figures according to your suggestions.

Specific comments Comment: Title: I suggest removing "in the area"

Reply: Good idea.

Action: Done.

Comment: 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" (line25) 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,

Printer-friendly version

Discussion paper

which are likely to be already undersaturated in DO. Discuss the findings chronologically (first 2015, then 2017).

Reply: We agree.

Action: We have re-written the abstract. We realized interaction as such is indeed not analysed and is not under focus of the present study. We removed it from abstract. The abstract has now been written in chronological order.

Comment: 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).

Reply: We agree this is important aspect.

Action: We have emphasized this topic more comprehensively in introduction of the revised version of the manuscript. Likewise, we have extended the topic in the discussion. We have included Chl a data to the results section. We also mention primary production in conclusions in the revised paper.

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

Reply: I am not 100% sure, if I understand your concern about lines 92-94 (there is no upwelling mentioned). Nevertheless, I understand your general concern.

Action: In the revised manuscript, we have described the role of productivity in the CDW and due to upwelling and the consequences to oxygen depletion more thoroughly. Also we have made more clear, what is the focus of our work. We believe after these changes it is understandable for a reader why we write lines 92-94 like this.

Comment: Lines 99-101: I would remove this paragraph.

Printer-friendly version

Discussion paper

Reply: We removed.

Comment: 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.

Reply: Yes, we agree. This topic was dealt in the first section of the discussion (original manuscript) but clear statement about the matter was missing.

Action: We extended there: "Both, maximum frequency of southerly wind and river discharge in the annual cycle occur in July-August (Figs. 13a,c). Thus, our observations conducted in late August - early September 2015 and late July 2017 illustrate the annual cycle of forcing and latter reflect in oxygen and stratification patterns." But we also need add: "On the other hand summer monsoon and river discharge were close to average in the whole summer of 2017 while the summer monsoon was clearly weaker in 2015 (Fig. 13a-b). Thus, our observations reflect also the differences in forcing and concurrently the DO distributions between two summers. "

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

Reply and action: We added (65 in 2015 and 49 in 2017).

Comment: 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?

Reply: We checked again and found that we have described and cited Fig. 11 under the results section, please see 295-302 (original submission). Resolution is 0.25°.

Action: We need to show and discuss figure 11. We considered merging of the Fig. 11 related text from discussion to results. But we realized this text (though have some results) has many discussion elements. So we prefer to keep it there. However, we added

Printer-friendly version

Discussion paper

text after the section we just mentioned (295-302, original submission). We introduce there also 2016/18 sea surface salinity observations (CDW spreading). We added information about resolution to data and methods section. Match between satellite and in-situ salinity is quite good. We don't want to add another figure just for comparison. Time-series comparison (Fig. 2) shows well that remotely sensed salinity is useful product in this region. We think fig. 2 is a good indicator of remotely sensed salinity quality in this area also for further studies. There are very limited time-series available in this region.

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

Reply and action: Done

Comment: 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.

Reply: It is not exactly the same. GLORYS uses ERA-Interim wind. For the wind forcing we use the dedicated CMEMS Wind product, which uses remote sensing observations from several satellite missions. However, potential temporal gaps in the observational time-series are filled by ERA-interim. The product has been validated and the quality is well proven the according to the public documents provided by CMEMS.

Action: We prefer to keep wind forcing as it is now.

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

Reply: We agree, it was repeating of results.

Action: We removed it from here. We state this also in results section and cite to figure 8 (simulated currents) and 3 (observed salinity).

Comment: Lines 134-142: Please state why you are analyzing AOU. E.g. to illustrate

Printer-friendly version

Discussion paper

cc BY

the role of DO consumption.

Reply and action: We added suggested sentence.

Comment: 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.

Reply: We agree.

Action: We have increased number of references to figures considerably. We cite to panels.

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

Reply: We agree.

Action: We added to the very beginning: "In order to link the thermohaline structure and DO observations we next analyze temperature.."

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

Reply: We fixed.

Action: There are 30 both in the figure and text of the revised manuscript.

Comment: 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.

Reply: We agree with critics.

Action: We have solved this issue. Figures are now cited by subplots. Also more text is added to describe the results more comprehensively.

CC BY

Comment: 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, . . .)

Reply: We changed.

Action: We changed colorscale and changed panel labels according to your suggestion.

Comment: 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.

Reply: We made changes accordingly.

Action: We have added more text on that topic. We have added chl-a to illustrate the process. Revised text is: "This indicates that coupling between coastal upwelling, which bring subsurface water to shallower depths (euphotic zone) and fresher riverine surface water, created favorable conditions for the primary production in the upper layer and concurrent DO consumption and depletion in the deep layer (Figs. 5h,i,k,l). Higher Chl a concentrations in the upwelling areas revealed from the satellite image, although not as high as in north (Fig. 6d)"

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

Reply: We specified.

Action: We added: ", e.g. to a temperature-, salinity- or density isoline."

Comment: 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

Printer-friendly version

Discussion paper

CC BY

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).

Reply: Yes we agree in that, thermocline and oxycline are not the best terms. The reason why the gradient approach (what you suggest) was not used and why we still don't want to use it, is the fact that if there is upwelling event (like in 2017), then the gradient approach fails. We would like to show surfacing of the both isolines. Gradient approach does not allow that. Likewise, gradient approach has some other problems (e.g. small intrusions, which are not interests of the present study, cause jumps in space). I (T. Liblik) have dealt with this issue on 35 sections measured across the Gulf of Finland (http://www.borenv.net/BER/pdfs/ber22/ber22-027-047-Liblik.pdf). For similar reasons we did not use finally the gradient approach, but used isolines (which varied for every section though) in that study.

Action: We rephrased the terms accordingly, so we don't call the isolines anymore as thermocline and oxycline. We have changed that in the whole manuscript. 24.5° describes the upper boundary of the colder water mass (Kuroshio intrusion) while the 2 mg/l AOU describes the upper boundary of oxygen depletion. This is done in the whole manuscript. In relation to the comment of other reviewer we now put much more attention to these boundaries in the whole manuscript. It also means that we don't talk about stratification in general way anymore (as in the original manuscript). We have also added altimetry data (mean sea level anomaly) to confirm the upwelling/downwelling

Printer-friendly version

Discussion paper

pattern and related changes with isolines. We copy here section from the draft of the revised paper to illustrate the approach/conclusion in the revised paper: " Two features must be present for hypoxia formation: 1) KSSW, 2) CDW and/or subsurface water upwelling. We can conclude that colder KSSW determines where (including in what depths) hypoxia could develop. Thus, latter provides necessary precondition for hypoxia. The CDW spreading and/or surbsurface water upwelling (and related biogeochemical, biological processes) determine the magnitude, exact location and timing of oxygen depletion."

Comment: 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).

Reply: We think figure 7 is necessary. In figures 4-5 we have selected 3 sections (not all the stations) while here we use all the available stations. It gives the better view about lateral isoline distributions, which is hard to visualize just by selected sections. After revision this figure became even more important. It describes the role of Kuroshio subsurface water as precondition for the hypoxia (as suggested by reviewer 2). The statement is related to the fact that if we compare 2015/2017 south/north bottom oxygen, we get that northern areas was much more oxygen depleted in 2017. However if we compare the total AOU (which describes the oxygen depletion in the whole water column) the two areas/years have similar values. This means bottom oxygen maps, which is the most common way to illustrate oxygen problem in this region, not necessarily describe the oxygen problem (depletion) solely. Other stratification characteristics matter also,

CC BY

not only the strength of the stratification. We think it is needed to highlight this point for further studies, because oxygen depletion (Oxygen Dept) is one of the indicator of eutrophication (e.g. https://www.frontiersin.org/articles/10.3389/fmars.2019.00054/full). Yes, that is good point about AOU-layer thickness statement. We have made error with the statement.

Action: We have added sentence about this matter to discussion, where we talk about importance of vertical location and movements of pycnocline (line 409, original submission). We rephrased the statement: "It means that high total AOU there (comparing to neighboring areas) was related both to the thick subsurface DO depleted layer and to the higher (comparing to surrounding area) AOU." The point we wanted to make is that thickness also matters. It is often missed, because not full resolution CTD-profiles are used, but bottle values (surface and bottom) instead. PS. This is another reason why want to show fig. 7.

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

Reply: We added.

Action: We have added the equation to data and methods.

Comment: 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.

Reply: Yes, this can be already mentioned here.

Action: Added "Since our surveys are conducted annually one month apart, the differences between the two surveys might be associated with the annual cycle in wind climate and river discharge. Our hypothesis is that the dominant factors behind the discrepancy of the two surveys are wind forcing and river discharge, and possibly their seasonality."

CC BY

Comment: 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?

Reply: We use 7-day averages.

Action: We changed the sentence "Mean wind, surface currents and bottom currents during 7 days prior to the surveys are presented in Fig. 8." and figure caption accordingly.

Comment: 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.

Reply: We agree, indeed the sentence can be omitted.

Action: We removed it.

Comment: 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.

Reply: Not in the same location. For all the work one location was used for the wind. We calculated all directions by 10 degree step. We believe giving correlation values of other directions is not necessary. It is higher closer to the best direction (near SE-NW) and it is weakest across the best direction (NE-SW) as expected. We have tried also to use wind over larger area when preparing manuscript, but it did not give any advantage.

Action: We added to the text. "(see the location in Fig. 1)". We added to the text "…from different directions (full circle by 10° steps)."

Printer-friendly version

Discussion paper

Comment: 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?

Reply: Yes, it is done as you wrote. We do it for the location marked in Fig. 8.

Action: We tried to rephrase "We grouped the simulated vm to the wind velocity wc classes with step of 0.25 ms-1 and took average of the each group. By doing this, we found relation between mean wc and vm." We hope it is clearer now. We added information to the previous sentence to make it clear: Next we make an attempt to quantify the two CDW spreading cases caused by wind forcing through analyzing meridional currents vm at section S1 (see Fig. 8) and wind data from the period June–September in 1993-2018."

Discussion: Comment: 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.

Reply: We have made mistake there, what we meant is Ekman surface current.

Action: We fixed the error.

Comment: 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

CC BY

conditions in 2016 and 2018 (e.g. from other studies if available).

Reply: This is not true. Content at section 295-302 (original submission file) is based on Fig. 11 and the figure is mentioned there as well. Same is with the figure 12. It was included in the last section of the results. You are correct about figure 13 – it appears in the discussion first. However, this figure is so much linked to the discussion that it is difficult to handle it before without repeating, which would be annoying for a reader. The annual cycle aspect is now mentioned (as reaction to your other comment) in the results part. So this information is provided now before discussion. We hope discussion is easier to follow after those changes. The last sentence there is speculative indeed. Previously in the results we have linked wind, discharge, CDW spreading and hypoxia in 2015/2017. In the same discussion section we show that wind and discharge qualitatively explain CDW spreading. Naturally, we should discuss/hint at least, how the oxygen picture look like based on this limited information.

We have information about extensive hypoxia in north in 2016, which very well confirm our suggestion. Unfortunately, it is difficult to cite this observation (no publication or report available yet to our knowledge).

Action: We have extended section in results about Fig. 11 in revision and included some sentences about 2016, 2018 observations there (as reaction to your other specific comment).

The figure 12 is important to show that mean current structure a week before 2015/2017 surveys (Fig. 8) holds truth also when we take historical winds-currents and group them according to our criteria. We have added such explaining sentence about this aspect to the very end of the results.

We made the speculative statement softer by replacing “should“ with “could”.

Comment: 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

Printer-friendly version

Discussion paper

(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.

Reply: Thank you for the suggestion.

Action: We have added more examples to the suggested section and also to the section before (where we talk about SSS). We also added sentence about synoptic scale variability until the end of the section. End of the section now looks like this “Such situation has been captured for instance in 1998 (Wang and Wang, 2007) and in 1999 (Li et al., 2002). However, on the top of the inter-annual variability and annual cycle are synoptic scale changes of wind-driven currents and river forcing, which likely influence the distributions on shorter time scales (Fig. 10). Thus, when planning hypoxia related measurement campaigns in future, it is worthwhile to take into account wind-driven transport, river discharge, remotely sensed salinity and altimetry to forecast spreading of the CDW, upwelling occurrence and deep water intrusion and according to latter factors estimate potential hypoxic area prior to field works. This could allow more efficient use of ship time and more detailed sampling of the hypoxic area. ”

Comment: 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”

Reply: We agree.

Action: We rephrased as you suggested.

CC BY

Comment: Lines 377-379: Irrelevant. Remove the whole paragraph

Reply: The point we wanted to make here is that in the case of southerly winds CDW might impact surrounding ocean also out from our study area. We believe statement hinting that should stay in manuscript.

Action: We rephrased the section. We hope its importance comes out better now. "Offshore, east- or northeastward advected CDW caused by southerly wind, as we observed in 2017, might form detached eddies due to interaction of the Ekman flow and density driven frontal currents (Xuan et al., 2012). Those eddies bring CDW further offshore and alter physical, chemical characteristics (including oxygen conditions) and primary production in the water column (Wei et al., 2017). On the other hand we noted ventilating impact of colder cyclonic eddy in north in 2015.

Comment: 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.

Reply: Studies in similar environments show that it is not just displacement, but also mixing between two water masses occur (subsurface water and former surface water). E.g. https://www.sciencedirect.com/science/article/pii/S0278434309002064 found that the share of former surface was 15% in the formed mixture after upwelling. But we agree, some other term than interaction can be used here.

Action: We rephrased"inter-action" with "coupling"and added the reference of Wei et al. 2017 who have described the"coupling" more thoroughly. It now says: "Shoreward, upslope penetration of the sub-thermocline KSSW and hypoxia in the upwelling – CDW coupling zone (Wei et al., 2017a) were observed."

Printer-friendly version

Discussion paper

Comment: 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.

Reply: We agree.

Action: Yes we have expanded the topic and have included chl a for illustration.

Comment: 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.

Reply: Lines can be added, but we would like to keep fig. 7 (we have explained reasons in other answer).

Action: We added the lines to the figures 4-5

Comment: 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.

Reply: We agree

Action: We removed the section.

Comment: The conclusions in general are too weak and rather a summary. Lines 452-454: Why is the statement on the inclination important?

Reply: Inclination is one reason why northern and southern hypoxia are so different. Deeper clines in south provide more stable and long lasting conditions for hypoxia.

Action: We have rewritten conclusions. Inclination as such is not mentioned anymore in conclusions.

CC BY

Technical corrections Thank you again for the careful look! We have made all the changes you suggested. Action related your comments is “done” or “solved”.

Regarding your comment: Line 238: what exactly do you mean with river plume bulge? It’s not clear to me.

Reply: We moved this part to discussion, because the topic is investigated more thoroughly by other papers, no need to repeat.

Action: As reaction to your comment we put new sentence there to the beginning: “The faith of the river plume can be separated to the regions and processes: circulating bulge near the mouth and downstream current along the coast (Fong and Geyer, 2002; Horner-Devine, 2009). The question is how much of riverine water remains in the river plume bulge and how much is advected to the neighboring areas. It has been estimated that about 80-90% of the discharge accounts to freshwater transport of coastal current (Li and Rong, 2012; Wu et al., 2013).”

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-421, 2019.

CC BY