Author Comment (AC2) by Soeren Thomsen to

Interactive comment on "Remote and local drivers of oxygen and nitrate variability in the shallow oxygen minimum zone off Mauritania in June 2014" by Soeren Thomsen et al.

Anonymous Referee #2

The authors present an analysis of the variability of dissolved oxygen (DO) and nitrate (NOx) in an upwelling region off Mauritania (18○S) using a series of glider and ship-based observations including a novel underwater vision-profiler mounted on the rosette to obtain particle size spectra in the water column. In addition to the CTD, the glider was equipped with oxygen and nitrate (Satlantic Deep SUNA) sensors. Based on water- mass analysis and AOU changes (along with a sequence of reasonable hypothesis) the authors separate DO and NOx local variability from a remote signal. The remote signal is mainly associated to changes in ocean transport, while local processes are related to local respiration and remineralization. Particularly, the results based on glider observations showed that an increasing of turbidity is related to negative DO anomalies close to the bottom and the authors hypothesize that resuspended particles increase local respiration. This hypothesis is supported by observations from the vision-profiler and, DOC and DHAA data obtained from the ship. In general, the paper is well structured and the data processing is well done and adequately explained (references are appropriate when additional information is required). Figures and figure texts are clear and main features are well emphasized. I have only few comments:

We thank the reviewer for his/her motivating and supporting review.

1.- I'm not sure if I missed anything, but it's not clear to me how the particle-based oxygen respiration rates were calculated. In the Page 17 lines 8 and 9, the sentence: "the mean particle associated respiration rate estimate along 18 ○ N during June 2014 reveal a similar pattern". On what is this claim based? The spatial distribution of the particles has been described in the previous lines, but the respiration rates are not mentioned. How the values of respiration rate given below (values in lines 10 to 12) are then obtained. Are these values given by OMP / AOU methods (like in Figure 6)?
From the sentence indicated above (lines 8 and 9) should it be understood that both sets of particles (small and large) are associated with a same respiration rate?

We thank the reviewer for this comment which reveals that the text needs more clarity about the way the respiration rates were estimated. Please note that in section 2.2 we give detailed information about this and also the relevant references. However, we changed the sentence accordingly to make more clear that the respiration rates are based on the combined abundance of small and large particles. Additionally we now give reference to the method description detailed in section 2.2. Furthermore we now refer to Fig. 8c at the end of the sentence to make clear that we are not referring to the AOU or OMP method estimates here.

We further decided to merge the initial "Observational datasets and data processing" section with the "method" section as we realised that several methods (e.g. the description of the particle-associated respiration estimation was found in the "Observational datasets and data processing" section. We hope that this is now clearer for the reader. Please note that this led to a change in the labelling and numbering of the sections as we have one section less now.

2.- Regarding the above comment, I understand that values for respiration (and nutrient remineralization) rate are crude estimations, nevertheless it would be valuable if the authors can provide an estimation of the error (or the variability, based on how they were estimated) associated to the given numbers.

We fully agree with the reviewer that it is very important to discuss and mention the limitations of our respiration rate estimates. We now address the main uncertainties of the particle-associated oxygen respiration rate in a quantitative manner as asked by the reviewer. For this we reached out to M. Iversen to receive the original dataset which is used in Kalvelage et al. (2015) to construct the relationship between particle size and oxygen respiration rates. The main uncertainty results from the large variability of respiration rate measurement of individual particles. This variability probably reflects the high variability of particle associated respiration which depends on the energy level, the quality and the state of the particles as well as the status of colonisation by microbes. Additionally it seems to be particularly difficult to analyse very small particles experimentally in the lab. However small particles make up a large amount of the overall particle abundance and thus are potentially also very important for the oxygen respiration rates. We decided to follow the lower-bound approach by Kalvelage et al. (2015) as they give most reasonable results when comparing with various other estimations (e.g. Brandt et al. 2015, diapycnal flux divergence off Mauritania along 18°N). However, to stress the uncertainty we added the information that the PARR could also be up to a factor of 13 to 16 higher, based on the upper bound fit to the individual particle respiration rates as shown in Iversen et al. 2010, Fig. 8c. Despite this large uncertainty we want to note that for our study its already very useful to quantify the relative difference between the onshore (near-bottom) and offshore particle abundance and the associated rate estimates.

Additionally to the in depth error discussion above we give now a quantification of the variability of the calculated particle-associated respiration rate estimates by separating all profiles into onshore and offshore profiles. This is necessary to have enough profiles to estimate a proper standard deviation. Although most stations have been occupied actually several times there are a few stations mainly offshore which were just occupied once. This makes it difficult to give an estimate on the variability there when describing the overall distributions across the whole transect. However, we added ranges of observed values for the specific regions of interest. In general the variability is high and standard deviations of the order of the observed mean values are typical.

We hope that this additional in-depth discussion will result in more future efforts to reduce this uncertainty. Thus we now also stress this in the discussion in relation to a recent study of Machu et al. (2018) which reports for the first time of an anoxic event on the continental shelf off Senegal. Improved local respiration estimates are needed to calibrate regional model simulations to possibly predict these kind of events.

We added the following sentence to the figure caption to make it more clear to the reader how the transect has been constructed. "Most stations along the transect have been occupied multiple times and averaged in depth space prior visualization." We further changed to a non-continuous colorbar in Fig. 8 (reduced numbers of colors) to make it easier to read the values.

3.- I wonder why there is no mention to the third type of oxygen anomalies described in section 4.2.1 in the discussion. I mean the negative (positive) DO (nitrate) anomaly "lens about 110 km from the coast and in 80 to 100 m depth" showed in 4n, m (and also in figure 5, last row).

We added a paragraph of discussion on the third type of DO anomalies into the discussion section. Please also see our answer to a similar comment of Reviewer 1 above.

Minor specific comments

Some extra information about the glider data would contribute to improve the paper.

We agree with the reviewer that some more detailed information regarding the glider data and how the glider was navigated would improve the paper.

Did de glider sample only until 250 m depth?

The glider was programmed to dive and sample only until 300 m depth as we focus on the upper OMZ. We added a sentence to section 2.5.

What is the accuracy and precision of the nitrate data?

The accuracy and precision of the Suna nitrate sensor is given with 2 µmol/l and 0.3 µmol/l in the data sheet. https://www.seabird.com/asset-get.download.jsa?id=54627862138
Our in-situ recalibration using temporally and spatially close measurement of glider-based nitrate measurements and in-situ CTD bottle based data reveal an even better accuracy (RMS) of 1.3 µmol/l. When reducing the fit to values at depth below 50 m (the focus of this study) we even get a better fit due to smaller internal variability. We added this information including these values to section 2.5.

Does the G2 glider have a pumped CTD?

Yes the glider has a pumped CTD. This information was added to section 2.5.

Was the sensor a fast-response Aanderaa optode?

The glider was equipped with a normal Aanderaa optode with a time constant of about 20 - 30s. However, our data processing includes beside the normal calibration (Hahn et al. 2014) a correction of this time delay as described in Bittig et al. (2014). Me mention that we use Aanderaa optodes now. However, as all details of the data processing and calibration are described in very detail in Thomsen et al. (2016), they are not repeated again in this manuscript.

Page 7. L 27: AOU was already defined (in P4 L31)

Changed

P 8. L 20-21: Check redaction.

Removed.

P 13. L 11: OPM should be OMP

Corrected.

P 13. L 25: delete "by"

Deleted.

P 17. L 23: "Of cause" should be "Of course"

Changed.

P18. L16: "waver" should be "water"

Changed.

P23. L9: Define OM or you mean DOM

Thanks for checking the manuscript so carefully! This is very valuable. We changed OM to organic matter. We refer here to both POM and DOM and thus organic matter is used. This was also changed in the figure caption of the final schematic.

Table 1: include units

Units have been included.

Figure 3: Indicate what the white vertical line represents.

The vertical line represent the transition between the glider data and the CTD dataset. We could interpolate over this small data gab but prefer to make clear that two different data sources are used for this figure. We mention the “white vertical line” now explicitly in the figure caption.

Figure 7. Delete one “following”.

One “following” has been deleted.

Figure 9. A depth scale for the study region would help (because the shallow OMZ was studied).

An approximate depth information was added to the schematic and we further added the information (shallow OMZ and approximate depth scale) to the figure caption.

Literature:

Bittig, H. C., B. Fiedler, R. Scholz, G. Krahmann, and A. Koertzinger (2014), Time response of oxygen optodes on profiling platforms and its dependence on flow speed and temperature, Limnol. Oceanogr. Methods, 12(8), 617–636,

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Kalvelage, T., Lavik, G., Jensen, M. M., Revsbech, N. P., Löscher, C., Schunck, H., Desai, D. K., Hauss, H., Kiko, R., Holtappels, M., LaRoche, J., Schmitz, R. A., Graco, M. I., and Kuypers, M. M. M.: Aerobic Microbial Respiration In Oceanic Oxygen Minimum Zones, PLOS ONE, 10, 1–17, 2015.

Machu, E., Capet, X., Estrade, P. A., Ndoye, S., Brajard, J., Baurand, F., Lazar, A., and Brehmer, P.: First evidence of Anoxia and Nitrogen Loss in the Southern Canary Upwelling System (Resubmitted), Geophysical Research Letters, 2018.

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