Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-103-AC2, 2018
© Author(s) 2018. This work is distributed under
the Creative Commons Attribution 4.0 License.

Biogeosciences
Discussions

Interactive comment on "Modulation of the vertical particles transfer efficiency in the Oxygen Minimum Zone off Peru" *by* Marine Bretagnon et al.

Marine Bretagnon et al.

marine.bretagnon@legos.obs-mip.fr

Received and published: 18 June 2018

1) Comments from Referee

General comments:

The study presented by Bretagnon et al. targets the very interesting and highly relevant relation of organic matter remineralisation associated with sinking particles and ambient oxygen concentrations. Regarding the important role of EBUS in organic matter export in combination with globally intensifying and expanding OMZs, this relationship may have important implications for future scenario modelling. The data presented

clearly indicate an important role of oxygen availability for transport efficiency of sinking particles which is likely related to oxygen demands of zooplankton and inhibition of aerobic organic matter remineralisation by prokaryotes. The authors do not investigate any of these effects directly but infer them from particle fluxes measured between the two sediment trap depths and further, more comprehensive studies are needed to investigate these relationships more precisely, nevertheless the data provide interesting and valuable insights. The authors furthermore investigate the impact of organic matter flux as well as organic matter composition on transport efficiency. There are however several drawbacks associated with the presented dataset, which need to be pointed out and made clear within results and discussion. Only one station equipped with two sediment traps has been investigated, and data were only collected over the course of one year, which results in a relatively limited dataset. The mooring was located on the shallow shelf and especially when looking at OM modifications of sinking particles while sinking through the water column, 115 m between two traps is very little given the often very high sinking velocities and only very limited conclusions on OM modifications occurring over this depth interval can be made. The authors over interpret their dataset specially in paragraph C by attempting to infer OM quality changes based on elemental ratios over a depth interval of 115 m. Overall, the study presents some interesting data and I would recommend it for publication, given that the authors revisit parts of the discussion and point out the limitations of the setup more clearly.

Specific comments: Style:

The manuscript is generally well structured into paragraphs focusing on different aspects. Many sentences are however stretching over several lines and are difficult to follow. It would also make the reader's life easier if the authors would refer to the different sampling seasons instead of using expedition acronyms. While there are no major grammatical errors, a native speaker could improve readability.

Introduction:

I am missing a more global view on the importance of the presented findings, i.e. the relevance of the investigated relation between oxygen and OM remineralization in a warming, deoxygenating ocean.

Page 5 line 14:

Transport efficiency only describes a ratio and the relative amount of carbon export and thus there is no direct relation between Teff and the actual amount of carbon exported, please rephrase.

Page 7 line 2:

Aggregation and disaggregation will impact total mass flux, previously suspended particles or microgels can aggregate and sink while sinking particles may disaggregate to form suspended particles, which represent the largest pool of particles in the water column but do not contribute to mass fluxes..

Page 12, paragraph C:

This paragraph is difficult to follow and would require restructuring. Interpretation of organic matter quality changes over the short depth interval of 115 m can only provide limited insights, especially as there are no information on processes occurring over this depth interval at any stage of the deployment. I would like to ask the authors to include this in their discussion.

Lines 20-26:

The paragraph on CaCO3 content is very interesting and shows the importance of ballasting material. The speculation on pH changes however appears relatively far-fetched without and actual data to support it. The findings might simply be related to phytoplankton community composition differences between seasons.

Page 13, lines 35-36:

Conclusions on OM quality and Teff can hardly be drawn from the presented dataset,

please rephrase to a more moderate statement.

Figure 3: Could you please add some axes labels and change the colour scale to better depict differences in low O2 levels?

2) Authors' response

General comments:

We thank Referee #2 for her/his highly relevant remarks and suggestions which helped us to improve the quality of the manuscript. Especially, we reconsider the paragraph C of the discussion part in order to provide a more cautious interpretation of our data. We agree with Referee #2 that the distance between both traps is short. However, in order to study vertical export over the shelf, the bathymetry constrained the deployment depth. Also, the fixed mooring deployed in this study is one of the first moorings ever deployed in the OMZ off Peru. While other OMZ studies based on sediment traps used drifting lines which are usually deployed for $\approx$0.5-5 days, our study is based on a fixed mooring line and was able to document a seven months period.

Specific comments: Style:

We thank referee #2 for her/his relevant suggestion regarding the style of the manuscript. As suggested by Referee #2, we replaced expedition acronym by the sampling season. To improve the readability of the manuscript, we worked on sentences to make them shorter. We also requested the RAPTRAD company to perform a careful reading and editing

Introduction:

We fully agree with Referee #2 and we added a sentence in page 3 line 2 emphasizing how estimation of carbon export and remineralisation could improve global coupled climate models. Specifically, we mentioned Cabré et al. (2015) and Oschlies et al. (2017) studies, which pointed out the fact that global coupled climate models exhibit difficulties in reproducing the spatial extent of hypoxic waters. In addition to inaccurate

representation of physics, they attribute these difficulties to a too large estimation of the downward flux of particles and a too weak remineralisation. Any progress in knowledge on export and remineralisation will dramatically improve prediction of climate models. The feedback of lower O2 availability in the ocean on the fate of OM and particles are also an important aspect to explore, and motivating this study.

Page 5 line 14:

Referee #2 is right and we rephrase the sentence of the page 5 line 21 to point out the fact that Teff indicates only the ratio of the POC collected in the deeper trap as compared to POC collected in the upper part.

Page 7 line 2:

Referee #2 is right and we rewrote the sentence page 7 line 12. Indeed, bio-physical processes occurring between both traps may modify the sinking rate and therefore affect the transfer efficiency. However, in this study, particles size appeared to be relatively constant between traps and whatever the season. The disaggregation effect did not seem to strongly affect the transfer efficiency.

Page 12, paragraph C:

In agreement with Referee #2's comments, we restructured the paragraph C by re-grouping information on organic matter (elementary ratio and their transfer efficiency, in page 12 and 13), and grouped information on CaCO3. We also modified sentences to point out the fact that the presented results should be considered as a suggestion and call for further analysis.

Lines 20-26:

In agreement with Referee #2's comments, we attempted to shade our interpretation on the CaCO3 fate. We modified this section from line 38 of page 12 to line 17 of page 13. Indeed, if OMZ waters are known to be characterized by low pH conditions, no direct measurement of pH were carried out during the deployment. Nevertheless,

cross-section of pH was measured at the end of the mooring deployment (austral summer 2014), and exhibits a vertical displacement of the calcite horizon. Therefore, the influence of pH modulation on CaCO3 dissolution could only be suspected. Also occurrence of plankton community composed of relatively soft CaCO3 would imply a greater dissolution and therefore, affect the transfer efficiency of this element. Nevertheless, the changes in the CaCO3 transfer efficiency occur not only seasonally but also intra-seasonally. In addition, the upper CaCO3 flux, the particles composition regarding C, N, P and BSi, and in terms of δ13C, remain relatively constant.

Page 13, lines 35-36:

Referee #2 is right and we rephrased the sentence page 14, lines 18-21 to remove any ambiguity. Indeed, during all sampling periods, the organic matter composition remains relatively constant and therefore does not allow to fully investigate the influence of the organic matter quality on the transfer efficiency.

Figure 3:

We modify figure 3 accordingly. It now contains axes labels and a different color bar that we hope better highlights different O2 levels.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2018-103, 2018.

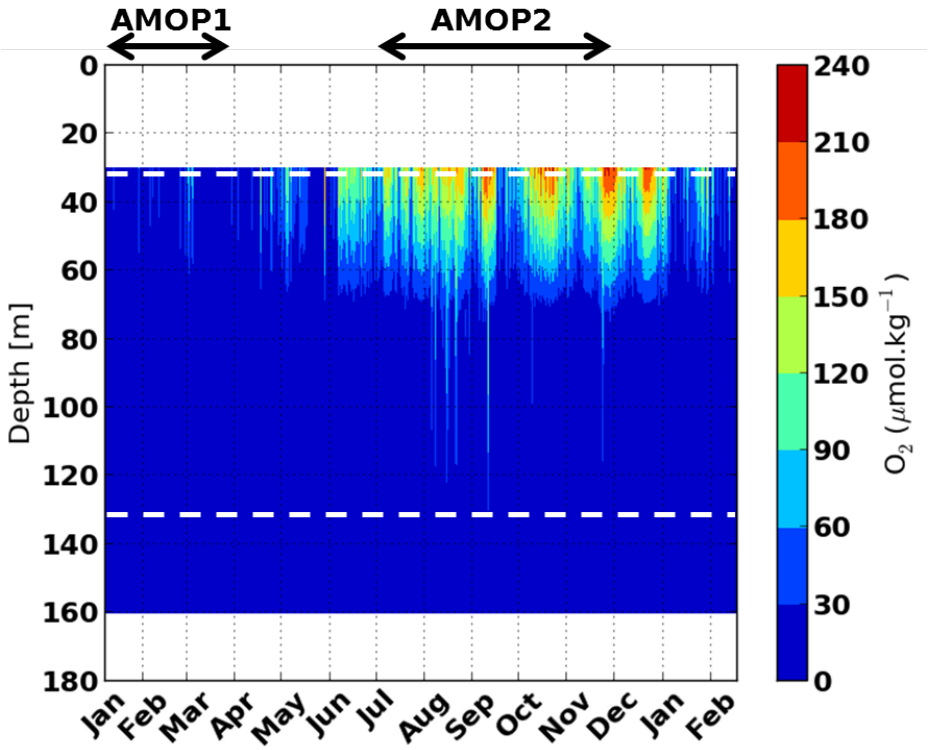
AMOP1
AMOP2
Depth [m]
0
20
40
60
80
100
120
140
160
180
Jan
Feb
Mar
Apr
May
Jun
Jul
Aug
Sep
Oct
Nov
Dec
Jan
Feb
240
210
180
150
120
90
60
30
0
O_2 (μmol.kg^{-1})

Fig. 1.