

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

### **Anonymous Referee #2**

Received and published: 24 May 2018

#### 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

[Printer-friendly version](#)

[Discussion paper](#)



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 especially 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-remneralization in a warming, deoxygenating ocean.

[Printer-friendly version](#)

[Discussion paper](#)



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  $T_{eff}$  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  $CaCO_3$  content is very interesting and shows the importance of ballasting material. The speculation on pH changes however appears relatively farfetched 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  $T_{eff}$  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  $O_2$  levels?

---

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

Printer-friendly version

Discussion paper

