Reviewer #3

We are particularly thankful to the anonymous referee for her/his numerous comments and suggestions on our manuscript. We agree with most comments and have modified and/or updated the manuscript accordingly. Details and answers are provided below in italics.

Comments to the Authors

General comments: This manuscript by Rouiller et al. presents a novel dataset and interesting hypotheses concerning the transformation and export of particles settling through the oxygen minimum zone (OMZ) of the Arabian Sea. Deep particle size and abundance observations were collected using an emerging optical technique, the Underwater Vision Profiler (UVP), and few data of this type exist at all, let alone in OMZ regions of the ocean. The authors use a particle tracking model to estimate source regions for settling particles, and to rule out significant horizontal advection as the source for large- and small-sized particle maxima observed beneath the OMZ layer. They finally conclude that zooplankton-mediated processes may enhance deep carbon flux in OMZ vs. non-OMZ areas.

I have two major concerns with the observations and modeling results presented, which the authors must address in order to better support their conclusions.

First, it is not clear that the velocity outputs from the circulation model the authors used to drive their particle-tracking model were sufficiently validated for the sampling time period and location. This makes it harder to support ruling out horizontal advection as the source of particles observed beneath the OMZ.

This is a good point. We have now included references to the quality control of MERCATOR model velocity outputs (Lellouche et al., 2013) and run simulations to assess the sensitivity of our results to the underestimation of velocity magnitude. See full answer below.

Second, the authors base their conclusion that enhanced export is occurring beneath the OMZ primarily on their observations of enhanced large-particle abundances in this layer. However, the data presented in the manuscript (Table 3 size-spectrum slopes, Figs. 8, 9, and 11) do not clearly show large-particle maxima at these depths. Some stations do show enhanced maxima in beam attenuation, which is due to small particles, but I would not expect small particles to strongly enhance export, at least not with the necessary settling velocities to rule out advection from beyond the OMZ.

You are right that only SPM (beam attenuation) shows a clear change at the lower oxycline in most stations (38, 39, 40, 41). However the LPM increase at the lower oxycline is observed at stations 37, 39, 40 and may enhance export. See full answer in the specific comment on p. 19279 line 18

Specific comments:

p. 19274 section 2.4: Please briefly describe the geography of the Arabian Sea and the seasonal period during which the cruise took place.

We added the following paragraph in section 2.1 :

"The first stations of the transect were done in the north of the basin, south of the Persian Gulf, a shallow semi-enclosed basin connected to the Arabian Sea through the Strait of Hormuz. The circulation in the Arabian Sea and in the Persian Gulf is primarily driven by the Indian monsoon regime which is characterized by a complete reversal of the subsurface coastal circulation along the year (Schott and Mc Creary, 2001). The transect was covered in 3 weeks and took place from March 12th to April 4th (Table 1), corresponding to the end of the Northeast Monsoon when cool and dry Northeast winds blow across the Arabian Sea resulting in an anticlockwise surface circulation in the basin. The transect included 7 stations of which 6 were directly inside the strong oxygen depletion zone (between 200-800m) and the last one (station 42) was located at the eastern boundary near the Maldives (Fig. 1 & Table 1). A total of 45 CTD rosette casts were performed (from 3 to 12 profiles at each station) and approximately 20% of the casts were done at night."

p. 19275 lines 16-19: How long before and after the reported transect was the O2 sensor calibrated? How did you ensure the sensor did not drift? What do you mean by "default"? (perhaps the wrong word?)

This part was unclear and is now rewritten (section 2.3). Oxygen sensor calibration was done prior departure in Nice, France in July 2009 and in Cape Town, South Africa in July 2010 (5 months after the Arabian Sea transect). Post-cruise calibrations performed by SEABIRD in July 2010 indicate that the oxygen sensor did not drift significantly over the period. We now use a value of 5µmol/kg to determine the vertical extent of the OMZ layer instead of DI..

p. 19276 lines 1-6: In general, this DI concept makes sense, but it is sensitive to the magnitude of biological oxygen production in the euphotic zone, and ignores the possibility of horizontal advection. I am concerned that oxygen consumption in the OMZ might not be best defined according to O2 concentrations directly above. Is there a consistent deep-water value that might be more appropriate?

As this point raised several questions from reviewers, we decided to remove this part of the material and methods by replacing it with a more simple method which consists to define the OMZ core according to oxygen values less than 5µmol/L.

p. 19276 lines 10-12; p. 19277 line 23-p. 19278 line 6; and Figures 8 and 9: Nowhere do you provide counting uncertainty or details about your binning/averaging procedures for the UVP derived observations of LPM. Please give these details and also report the uncertainty on your measurements. To clarify what I mean by counting uncertainty: if the UVP sample volume is 100 cm^3, then one cannot accurately quantify fewer than 10 particles/L without binning the data. The LVP profiles in Figures 8 and 9 may require depth binning in order to reduce the uncertainty, unless the UVP sample volume was large (state its size) or averaging multiple casts at each station was sufficient. If the latter approach has already been taken, please include the uncertainty in particle abundance profiles.

We have clarified this important point in the manuscript. One image is roughly 1 L (Picheral et al., 2010) and the image acquisition frequency is on average 10 images s^{-1}. The UVP is mounted on the CTD rosette, which is lowered at a speed of $1ms^{-1}$. For each profile (CTD cast) we compute the average particle concentration per 5 m vertical bin, corresponding to an observed volume of 50 L. Then we compute the average profile per station (n=3 to 12, 6 profiles/station on average). So the observed volume for each point in the displayed vertical profiles ranged from 150 to 600 L, which allowed us to estimate particle abundances even at low concentrations < 0.1 part. L^{-1} (Fig 9, particles >1mm). For clarity, a moving average (45m window) was used for figures 8 and 9.

The method section 2.6 was modified to:

"Each 5 m bin particle concentrations was the average concentration in a 50 L observed volume. Averaging particles profiles per stations yield an observed volume ranging from 150 to 600 L per depth bin (n=3 to 12, 6 on average). For clarity, the average profile was smoothed with a 45m wide moving average. Note that below 1000m depth, only one or two UVP casts were performed."

Concerning the uncertainty of total particles abundance, figure A1 below presents individual casts for each station (3 to 12 casts per station).

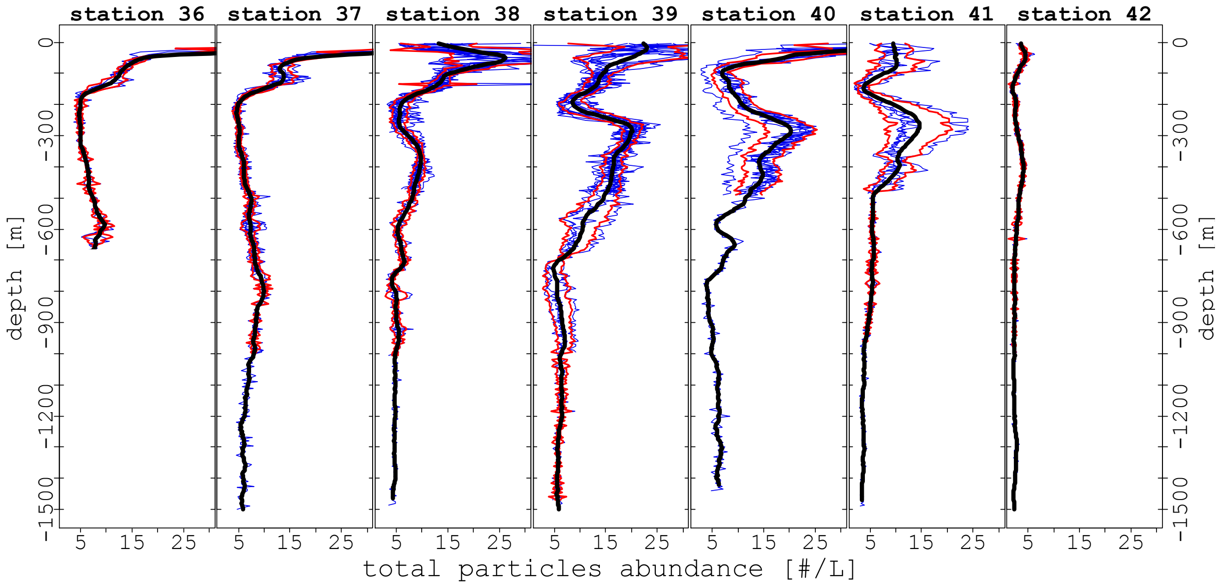


Figure A1. Vertical profiles of total particles abundance for each cast (thin blue lines) with their corresponding mean (thick black line) and standard deviation profile (mean +/- std, thin red lines), for each station.

The standard deviation of total particles abundance is approximately 1.64 #/L on average over the complete dataset (~ 20% of the mean value). Note that the standard error is larger for stations 40 and 41. We added this information in the methods section 2.3.

p. 19276 lines 12-13: "Only images from recognizable objects larger than 500 µm were sorted using an automatic classification followed by manual validation." It is hard to understand what is meant by this sentence – were particles larger than 500 µm only counted if they were recognizable? Is there a separate data set consisting just of this "recognizable" subset of >500 µm? If the latter, one would think that this dataset might contain information about zooplankton that could be used to substantiate subsequent discussion.

This is true, there are two datasets, one containing information for all objects >100µm (from automatic image analysis) and one for objects >500µm containing the images which can be classified semi-automatically (vignettes displayed on Fig 10). Among images, zooplankton are quite rare <0.005 ind. L^{-1} in OMZ (Figure 7), compared to particles of similar size (<0.1 L^{-1}). So in oligotrophic waters, zooplankton organisms are not well sampled by UVP. Therefore, we did not use the UVP data for zooplankton and instead used the net data.

Section 2.5 was modified to make it clear.

p. 19278, lines 2-3 and Figure 9: If particle counts have been normalized to bin width then the axis labels in Figure 9 should read [#/L/micron], not [#/L].

The abundances are normalized only when calculating size distributions in figure 11.

Section 2.6 was modified to make it clear.

p. 19278 lines 8-9: Since you did not directly measure carbon or mass flux and instead use a (globally-tuned) model to assume settling velocity as a function of particle size, there are some important caveats with your flux estimates that you ought to address.

Yes there are some assumption behind the use of a global relationship between particle size distribution (PSD), concentration, and carbon flux. Limitations of the method are now addressed in the manuscript since this point was also raised by reviewer 2.

However, although this method has limitations, it provide a much better vertical resolution of particles concentration and calculated flux than any other conventional tools (especially sediment traps). Indeed, sediment traps would largely integrate space and time making it highly difficult to understand mechanisms happening below the OMZ. We think that using PSD as a proxy of Carbon flux is highly valuable in this particular context. Even if the value of the flux might be off, the general trend would be conserved providing vertical resolution needed to study the impact of the OMZ and the zooplankton communities on flux attenuation.

p. 19278 lines 9-10: What do you mean by "profiles of flux were adjusted by the Martin Model"? I assume that you mean you fit the Martin model to your flux data? Please clarify this. Also, the Martin model is often not a good one for measured flux profiles (including many of yours, particularly those where modeled flux increases with depth). You should better justify its use.

Our wording was confusing. Further, this is an important point that needed some explanation. Reviewer 1 and Pr. Karl Banse also raised the question of relevance of using the Martin curve giving the deep flux increase. In fact, we fitted the Martin curve only to the data above the oxycline. This point is now better explained in section 2.6. Indeed, fitting the Martin curve to a flux profile that shows an increase with depth is contradictory to the purpose of the use of the model. The Martin curve might not be the best model to fit to flux data however it's still highly used in the literature and we wanted to present it here for 2 reasons (1) to highlight the fact that this model cannot be use in all situation, (2) to present the relative decrease of carbon flux between the top of the OMZ and the bottom depth of the OMZ. In no circumstance, the model is used to present what is happening below the OMZ. Therefore, even if the Martin model is highly debated and somehow can be seen as obsolete, the b still represents a standard value that allow comparison with other oceanic regions.

p. 19278 line 14: Why zref = 50 m (rather than euphotic zone depth)? Your stations have deep fluorescence maxima that vary from 22-73 m. Using a fixed reference depth for the Martin curve fit among all stations may only exacerbate problems with the poor fit of that model to your data.

The reviewer is right and using a variable depth to fit the Martin curve would be more appropriate. However we wanted to use a method allowing comparison with b obtained from sediment traps. In most case, sediment traps used a reference depth of 100 or 150m. These depths of reference could not be used in the present dataset as its already entering the core of the OMZ for some profiles. Therefore 50m was chosen as the best compromise between traditional method and depth of the top of the OMZ.

Also, this is not a critical issue because of the vertical resolution of the particle flux from the UVP. Indeed the usual sediment trap method uses only few data point to fit the model (usually 3 to 10 in best scenarios). When using the Martin curve on the UVP data, you actually fit the model to flux estimated between the depth of reference and the bottom depth with a 5 meters bin interval. For example, fitting the martin curve to UVP flux data between 50 and 550 meters would be equivalent to fit the Martin curve to 100 sediment traps placed every 5 meters in the water column. This method, also relying on fluxes estimates, provides a much better confidence on the quality of the fit that any other method to date.

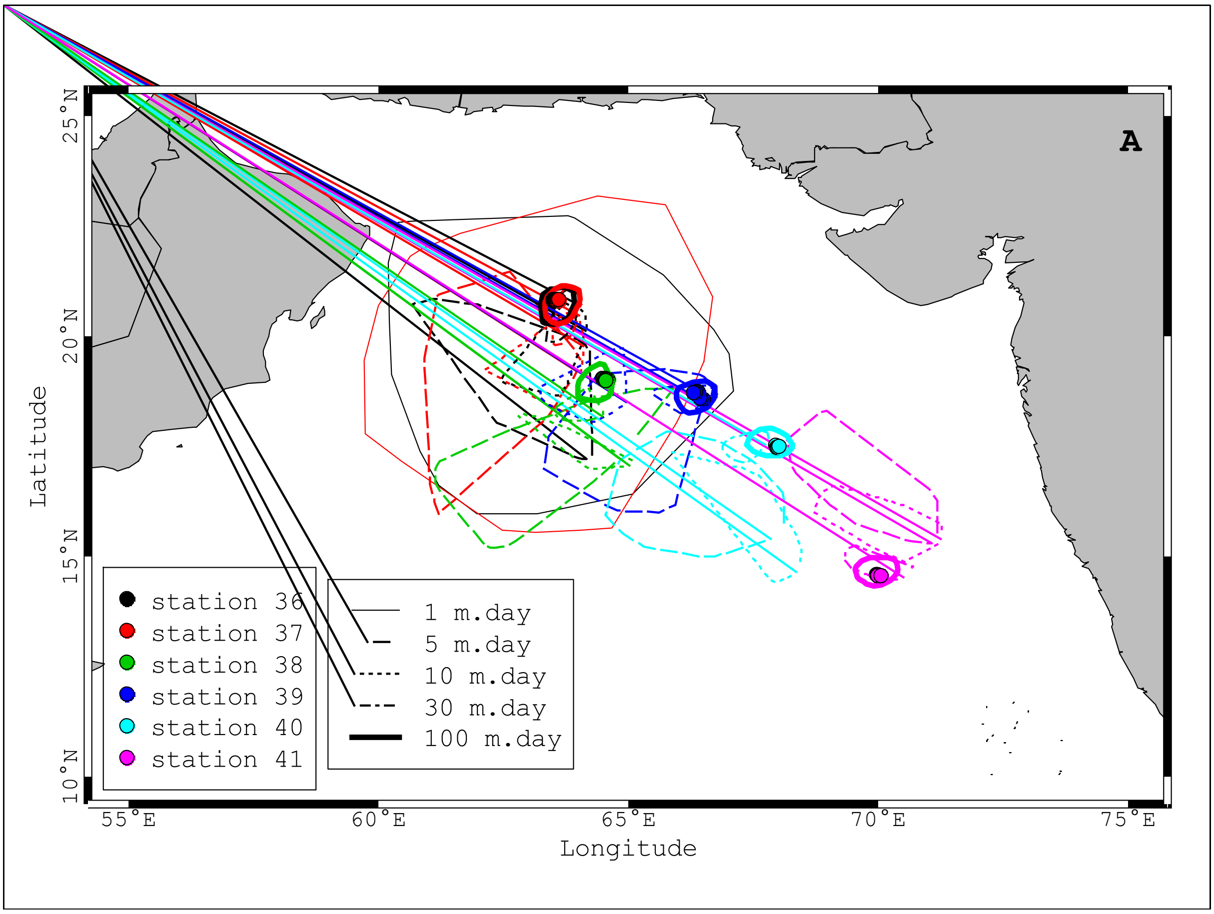
p. 19278 lines 23-26: 1) Do you mean that 3 years of data were used in the validity comparison to T,S climatology? It does not make sense that you would use outputs from 2009 and 2011 to drive a particle-tracking model for comparison specifically to 2010 observations. 2) I'm not sure if it is sufficient to assume that a model correctly reproducing T and S relative to climatology will also correctly reproduce horizontal velocity, which is the relevant parameter for your particle tracking model. I think additional validation of the velocity field is necessary. Could you compare 2010 output to shipboard ADCP, local Argo trajectories, etc?

1) The text was not clear and it is now corrected. For our results we only used trajectories seeded at the time of the actual cruise (March 2010). Trajectories for years 2009 and 2011 were not used in the present work. This is now clarified in the Methods section 2.7.

2) Unfortunately we did not have an ADCP onboard TARA, so we do not have synoptic in situ velocity measurements. However MERCATOR PSY3V3 outputs are routinely quality controlled: you can see qualification reports at http://www.mercator-ocean.fr/eng/science/qualification/ and Lellouche et al, 2013 their Fig 14., done over year 2010 and including comparison with velocity derived from surface drifters.

The comparison with velocity derived from surface drifter shows a tendency of PSY3V3 velocity outputs to underestimate surface velocity magnitude by approximately 20%, but a good accuracy of *the simulated velocity directions. This underestimation implies that the lateral spread of particles we have simulated may be underestimated.*

We thus have run simulations with current velocity increased by 20% and compared the results to initial simulations (figure A2). For all stations, few differences are noticed. For stations 40 and 41, the envelopes are wider and extend further Northwest (station 40) and North (station 41) for 5m/day and 10m/day settling velocities. For station 40, the 5m/day envelope extends northward toward stations 38 and 39 but given the low settling velocity and the time course of the bloom, those particles could not originate from the bloom area. As with initial results, none of the simulations indicates a successful transport of particles from the coastal zones north or east of the stations.



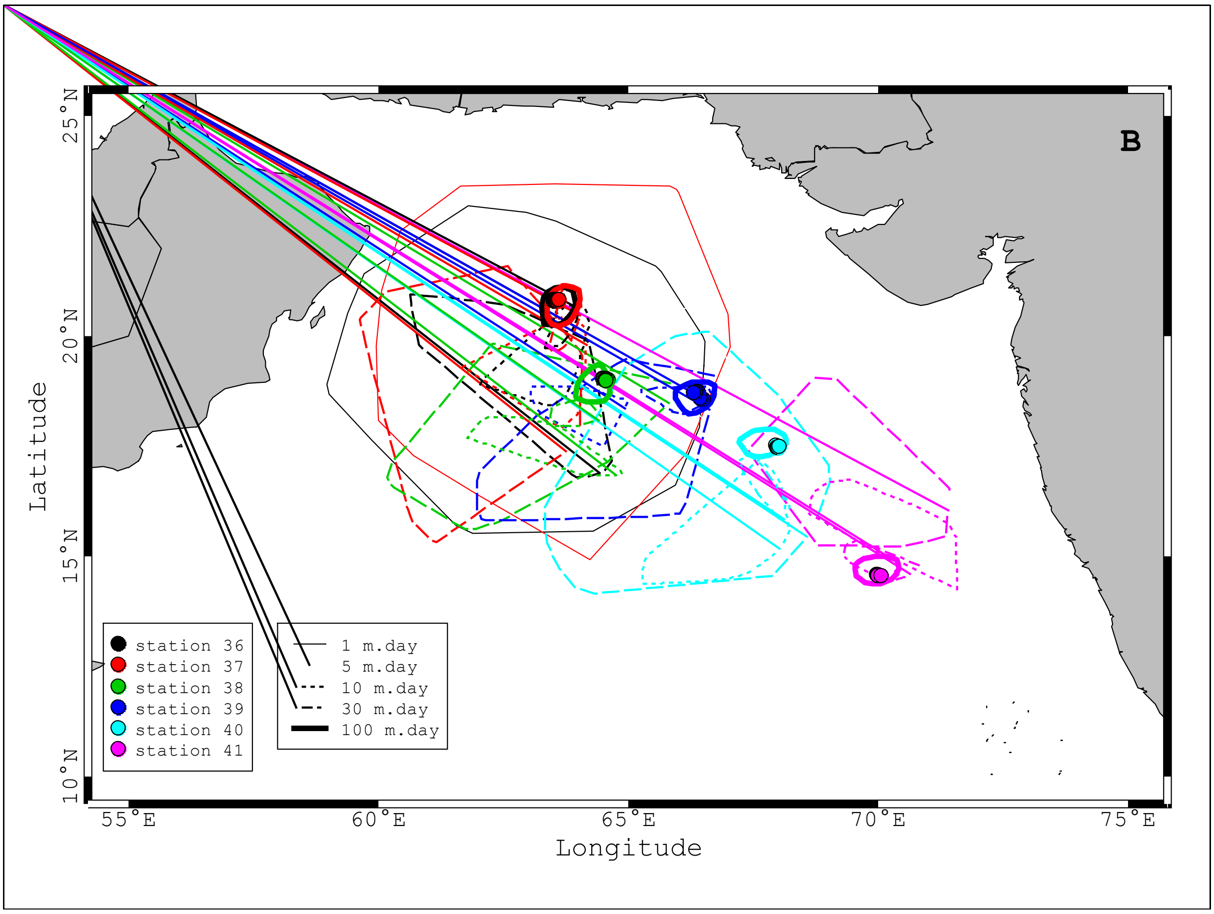


***Fig. A2.** Results from backward Lagrangian simulations: A) initial plot as in the manuscript and B) the same but with current magnitude increased by 20%. Each envelope contains the 75% of the initial particle number locating possible source of particles observed in each vertical profiles. Five settling velocities were used (see legend). Note that envelopes are drawn only if more than 100 particles have reached the surface.*

We added in the method section 2.7 the references for validation and the potential underestimation of model velocity magnitude, and in the discussion section 4.2 we added a sentence on the sensitivity of the results:

"Sensitivity to the underestimation of the velocity magnitude is low. Simulations with current velocities increased by 20% do not indicate a successful transport of particles from the coastal zones north or east of the stations"

p. 19279 line 18 (also p. 19286 lines 15-16): That a deep particle abundance maximum exists in every station in the UVP data is dubious. It is not immediately apparent in Figures 8 or 9. Particularly after accounting for counting uncertainty in the UVP data, it may become even less clear.

You are right that only SPM (beam attenuation) shows a clear change at the lower oxycline in most stations (38, 39, 40, 41), while the large particle increase at the lower oxycline is only observed at stations 37, 39, 40.

Note that below 1000m only one or two UVP casts were available (this is now explained in the methods). Therefore the observed volume (approx. 50 L per 5m bin) may not allow to detect small changes in large rare zooplankton and aggregates at this layer. Changes observed are about 1 particle in 50 L (station 39 Fig 9) thus we are close to the detection limit.

However, our dataset and also previous findings suggest that this layer may be the site of higher biological activity. So in the discussion section 4.3 we clarified the stations that show this LPM deep maximum and discussed the methodological uncertainties of the rare particle quantification with only 1 or 2 UVP profiles.

p. 19282 line 11: do you mean "decrease by a factor of two" rather than "decrease by two"?

Thank you for this comment. We meant "decrease by two". We corrected the manuscript.

p. 19282 line 12: 30-60 particles per square meter integrated from 0 to 100m seems much too low. Figure 8 shows LPM ranging from 2 to more than 30 per litre. Is 30-60 particles m-2 a typo? Did you mean in the "maximum" layer only? (and if so, how thick was the integration depth interval around the maximum?)

It was indeed a typo and an error. We wanted to say "30-5 particles per litre on average between 0 and 100m". Corrected.

p. 19283 line 20 – p. 19284 line 2: Can you move this part of the text earlier in the paper, perhaps to lead off the Results? It helps orient the reader to the rest of the results.

We consider this part of the text as necessary at this point of the discussion because it explains the presence of large aggregates. We feel that it does not belong to the results because we compare the findings with previous works.

However, following your advice, we added a sentence to introduce the results section 3.1.

p. 19286 line 24-25: If you can more rigorously show that your model reproduces horizontal currents correctly (as discussed above), it will strengthen this conclusion.

See answer above.

p. 19287 line 25-27: As it stands, the only clear, deep increase in particulate matter is in the beam attenuation signal corresponding to small particles, and these are just as easily explained, in the absence of advection, by abiotic mineral precipitation at the edge of the oxic layer and by increased bacterial biomass. Unfortunately the LPM data, as presented, do not unequivocally support your conclusion here.

Reviewer 1 and 2 raised comments and questions on the lower oxycline patterns. We agree that numerous physical and biological factors (mainly described in estuarine situation) could explain the observed layering in particles. Physical aggregation processes rely on increased scavenging by change in the redox state of metals in oxygen deficient water (Rue et al, 1997; Nameroff et al., 2002) or turbulence in midwater (Stemmann et al., 2004). Also these processes are described in the literature; their efficiency at producing particles up to few tens of microns has not been demonstrated or even is low. Once the collision between organic or inorganic precursors has occurred, they need to stay bounded. So their aggregation depends also on their sticking properties. The latter can be affected by direct effect of high metal concentrations (Mari and Robert, 2008), modification of pH (Riebesell et al., 2007; Mari, 2008), and to microbial processes (e.g. Muylaert et al., 2000). A recent study in the South Pacific OMZ (Ganesh et al., 2014) has shown that particle attached bacteria are particularly present (and possibly active) in oxygen deficient water. So, knowledge on the processes leading to the formation of small particles remains weak especially in OMZ anaerobic systems. Despite these weak evidences, repeated observation by us and other (Wishner et al., 1995, 1998 and 2008; Lee et al., 1998) supports the fact that this lower oxycline is a site of enhanced bacterial and plankton activity acting on particles.

So, our new dataset on particles size distributions does not explain the mechanisms but it supports previous works. Future study should focus on this layer. We have modified this part of the discussion to be more comprehensive and more careful with our main hypothesis.

Section 3.3 was modified to include a new paragraph on this issue.

All figures with gray oxygen overlay: Please add an O2 axis.

We corrected by removing oxygen profiles from figure 2. Thus figure 4 is the first one showing oxygen values. For the following figures, we simply added reference to figure 4.

Figure 8 caption: Please note the scale difference for cp as compared to Fig. 5.

Thank you for this pertinent remark. We corrected figure 8 in which initial curves were smoothed.

Technical comments:

There are many minor grammatical errors and poorly-constructed sentences and paragraphs in the manuscript, which should be corrected before the paper is printed

The new version will be proof read by a native English speaker.

Cited references

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