Karl Banse

We are particularly thankful to Karl Banse for his 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

The article by Roullier et al. (2013) on fluxes of large (> 100 um) particles and organic carbon across the Oxygen Minimum Zone (OMZ) of the Arabian Sea made on a NW to SE section in March 2010 stands out by the quantitative interpretation of in part novel measurements. It is an admirable open-sea effort on the small, 36 m long ship engaged in the Tara Oceans expeditions.

What is missing in the paper is to convey a sense for the effects on the fluxes of the variability of the data in the replicate casts, as well as of global coefficients (for example, as taken from Guidi et al., 2008, as cited), or of the unconvincing choice of 50 m for zref (Eq.7, p.19278).

I do not want to suggest a full-fledged sensitivity analysis but I just ask to state what, for example, a change by 20% of the used values would mean.

Thank you for your remark. For each station, we calculated the mean and the standard error of the exponent (Martin b coefficient) by considering the zref value from 40 to 60m depth (50m +/- 20%). Thus, for stations, 36, 37, 38, 39 and 41, the standard error is less than 5% of the mean b value, for station 42, the variation is about 10% and for station 40, the variation is close to 20%.

As to the write-up, please, state the date of the cruise on p.19274 and enter “inferred” on Line 5, p.19288, before “biogeochemical”.

Done

Also, were all the instruments, listed as "added" on top of p.19275, used on all 45 CTD casts and if not on how many?

This wording was confusing. All instruments/sensors were mounted on the rosette frame so all casts are equal in term of parameters. The term “added” was used to oppose physical to biogeochemical data. We therefore changed “added” by “mounted” to avoid confusions.

In the figures depicting profiles, the lettering of the axes is too small. In the caption of Fig. 11, state the depths of the two right-hand panels. What do the arrows in the right-hand panel of 11 A designate?

The labels size has been increased. We added a legend indicating depth layers on figure 11 and we removed the arrows that present a specific particle population that is not discussed in the text.

My principal comments address the vertical distribution of mesozooplankton and the treatment of fluxes. For the former, I add an early literature reference, while for the latter, I have substantive questions and suggestions.

1. Recall that the intermediate minimum of biomass as related to the O2 minimum (Fig. 7, stas. 38 40) is present in the three major oceanic OMZs. In this paper, the increase of biomass in the lower OMZ is tentatively suggested as partly due to particles, which had been relatively little degraded or removed by grazers while settling through the overlying intense O2 minimum (sect. 4.3), similarly to Wishner et al. (1998, as cited). Previously, Vinogradov and Voronina (1961/1962: 529 and Fig. 3) made a similar inference for food, settling from above but not utilized in the OMZ, as supporting higher biomass of mesozooplankton at depth.

Thanks for mentioning this reference that we have omitted in the manuscript. This has been corrected.

2. In Fig. 11 the Martin model or curve does not fit the flux data well, neither near the surface nor if extrapolated to greater depth. Also, Line 20, p.19287 states that the model cannot be applied to sta. 37, yet the station is one out of two depicted in that figure. Why?

Sorry for the confusion. We added the Martin curve to show how badly it was fitting to the flux profile. So we have removed it from the figure to make it clear.

And, is the station among the five included on Line 21?

No, this station is not among the five included on Line 21.

Why use the Martin model at all? Because of its design, it will always fail when the depicted variable increases at depth. Also, its exponent is biologically or geochemically meaningless and cannot be experimentally verified. Instead I suggest plotting the data in Fig. 11 (perhaps the original raw numbers, rather than the plotted means?) and the fluxes or particle volumes at stas. 38 and 39, on double-log paper (see my attached figure exhumed from the Gray Literature). The differing slopes of regression lines in such plots reflect the shifting balance between (supply or growth) and (mortality

or loss), both being the rates that we want to know for the mesopelagial - and in ecology in general. (Russian oceanographers used double-log plots 20-25 years before Martin et al., 1987, as cited.)

This is an interesting point. The use of the Martin model has been debated for a long time and is still largely used in the literature. I agree with Pr. Banse that this model is definitively not the best one to model flux in the water column, as many particle profiles do not show an exponential decrease. However this model is presented in the literature since the 80's and gives reference value for the strength of the reduction of the flux between a reference depth and a bottom depth.

In the present dataset we used the reference depth of 50 meters that can be discussed and reviewer 3 also raised this point. In most case, sediment traps used a reference depth of 100 or 150 m so why using 50 m in the present study? These depths of reference could not be used with the OMZ dataset as they are already in the core of the OMZ for some profiles. Therefore 50 m was chosen as the best compromise between traditional method and depth of the top of the OMZ.

Also, this depth is not critical because of the depth resolution of the particle flux from the UVP. Indeed the usual sediment trap method uses only few data point to fit the Martin model (usually 3 to 10 in best scenarios). When using the Martin model on the UVP data, we fit the model to flux estimated between the depth of reference (50 m here) 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 fitting the Martin curve to 100 sediment traps placed every 5 meters in the water column. Therefore the starting point of the profile is not an issue here. This method relying on fluxes estimates provide a much better confidence on the quality of the fit that any other method to date.

Then, the question regarding the increase of the flux with depth is mentioned by Pr. Banse. We did not use data point below the bottom of the OMZ when fitting the Martin Curve.

The use if the Martin model was limited to provide an estimate of the attenuation of the flux through the OMZ that could be comparable to other estimates present in the literature. Even though the exponent of the Martin curve is "meaningless" (it doesn't reflect rates or any biological of physical parameters), it's a convenient value that reflects how fast the flux decrease with depth. This value is also a reference in the literature and cannot be avoided even If the scientific community should probably slowly shift toward a more meaningful representation of the "remineralization".

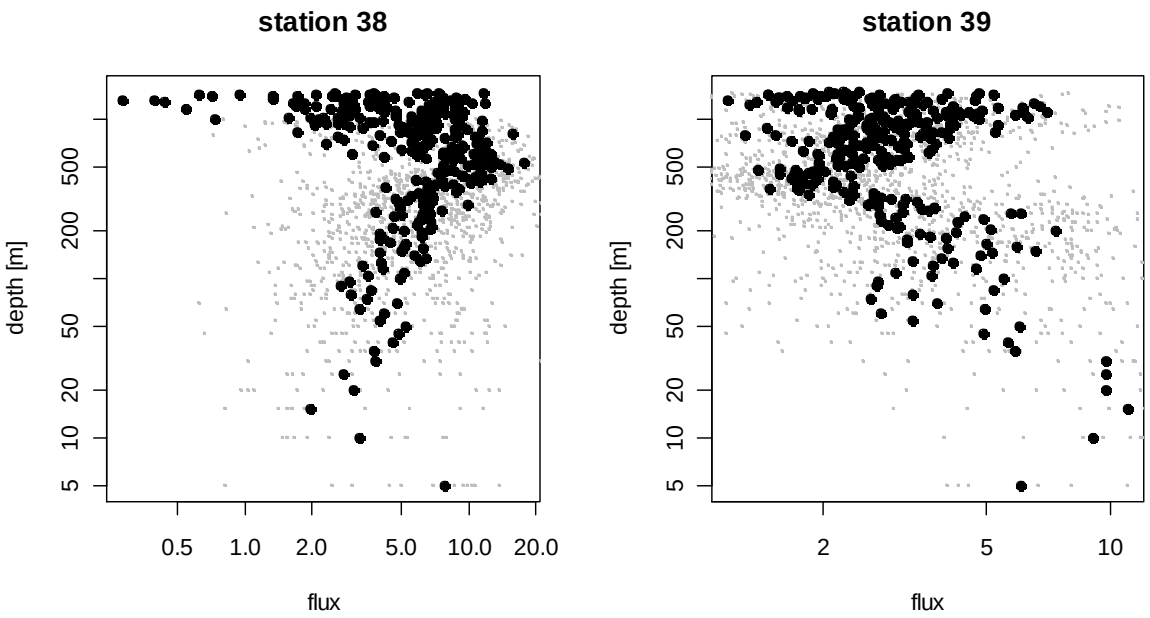

Figure 1. stations 38 and 39, log-log plot of flux vs depth (grey points are raw values i.e. for each cast and in black is the mean profile).

We presented the log-log plot unfortunately our dataset is limited with depth 1500 m compared to the deep profiles from Banse (Figure 1). Those two figures confirm the deep increase flux associated to the lower oxycline at station 39. For the station 38, the intermediate increase near 500 m depth is also observed in the LPM vertical profiles (fig. 9 of the manuscript).

Regarding the spelling in the headline of the paper, our consensus here is that it has to be Particle, but not particleS.

Indeed, we corrected the title of the manuscript.