

Anonymous Referee #2

We would like to thank the referee for the valuable comments and suggestions, which will help us to improve the manuscript.

We are convinced that following the advice to add a section on Material and Method will help to clarify the structure of the revised ms and to solve almost all issues raised by the reviewer. During the restructuring of the ms detailed comments referring to specific pages and lines will be taken into account. Please find our responses to the more overarching comments in the following:

Comment: ... only present few field data that are (in a rather confusing way)

Answer: Here we would also like to mention that we wrote on page 5 line 8 -11: In this paper we compiled and analyzed sediment trap data collected by deep moored sediment traps at fifteen sites with the aim to study the influence of the ballast effect and on the organic carbon export in the Indian Ocean (Fig. 2, Tab. 1, 2). The presented data were obtained at 15 sites in the Indian Ocean. At four sites the fluxes were measured for more than 200 days per year over a period of seven and more years. Tables providing the respective information will be included into the revised ms. The most recent global compilation of sediment trap we are aware of was published by Wilson et al. 2012. It includes 156 data sets. If one subtracts the data measured by us the number of the data reduces to 136. Within this paper we updated our records from the Indian Ocean. Due to low interannual variability we calculate annual means and added again 15 sites to the data set. This increases the quality and the number of data within the global compilation by ca. 10%, and represents 70% of all sites from which particle flux data are available in the Indian Ocean. This is to our understanding a solid improvement of the global data set on particle fluxes.

Specific comments: Abstract: The first half of the abstract (line 9-17) describes the study, but the second half (line 17-27) gives in my opinion only general statements and relates partly to areas/ subjects, which are quite off the focus of the work. I would suggest revising the second half on the abstract to focus more on the study.

Answer: This will be done in revised version of the ms.

Comment: ... in which manner rivers influence EPT and JAM (e.g., distance to river, water discharge, sediment load of these rivers, CTD profiles showing freshwater layers at EPT and JAM), and could cause ballasting there.

Specific comments: Introduction: The introduction is with 4 pages quite long and I think it is difficult to understand, because neither the goal of the study or the study area is presented, nor is a clear logical structure recognizable.

Study area: In this section, first a detailed description of the monsoon system in the Indian Ocean is here given, followed by a section on the organic carbon export and results achieved by the sediment traps deployed during the present study. In my opinion, the description of the general monsoon pattern should be shortened, but details on proximity of rivers and river plumes, river discharges intensities and magnitude of entrained lithogenic material are crucial for the present study and should be included.

Answer: As mentioned before we appreciate very much the suggestion to add an additional section on Material and Methods and we will follow also the advice to focus

on the Indian Ocean and include more information on river discharges. This will help to restructure the Introduction, the Study Area and of course the discussion.

In addition, I wonder why results of the carbon export measured at the included in this section titled "study area", which I think is rather peculiar.

Answer: The reason to include this section into the Study Area was that processes through which the monsoon influences particle fluxes were already discussed in previous publications dating back to 1986. We assumed that they are generally accepted so that we could present them as part of the Study Area and focus on the ballast effect in the discussion. Will be changed in the revised ms.

Nevertheless, they use a particle size found by Iversen et al 2010 off NW Africa (page 11, line 22-24) without any justification. I completely agree that it is sometimes necessary to use data from other areas if they are the only available ones, but then a proper justification is needed why it is appropriate.

Answer: As mentioned by the reviewer there are no data on particle sizes available from the northern Indian Ocean and the Java Sea (at least as far as we know). Since NW Africa is an upwelling system as well as the western Arabian Sea and the region off South Java we assumed that those of NW Africa might be representative for our study sites. We will explain this better in the revised version and would be grateful for any hint to solve this problem in a better way.

A two pages long theoretical approach followed, which described how to determine the ballast effect. This is in my opinion very wordy and I do not understand why this section is presented in the results and discussion section and not in M&M.

Answer: We will include it in the a M&M section.

Further, the authors also cite McDonnell and Buesseler (2012), who found that there was no relation between particle size and sinking velocity (page 9, line 29-30), but I cannot see any proper discussion on why they still use a size – sinking velocity relation (I guess they do, as they use equation 4) it in their later "experiments".

Answer: What we meant to say on page 9 was only that the general assumption that larger plankton forms larger and thus faster sinking particles is not general rule. It could occur at specific conditions but is not a rule. The equation 4 and the discussion, which followed explains this in detail. As shown in table 3 we considered only constant particle sizes because we wanted to know how density changes affect sinking speeds and respiration.

The next subsection presents various densities of sinking material (Page 10, line 11-22), which is quite hard to understand in the text. I would suggest putting these numbers in a table instead of presenting them in the text.

Answer: This will be done.

The following section on the conducted experiments is very difficult for me to understand, because I have a hard time to trace what the authors did and why they did this (e.g. they get a sinking speed of 84.5 m d^{-1} in the first experiment (p. 11, line 21), but on page 12, line 4 and 6 they suddenly use completely different sinking velocities).

Answer: This will be explained in better way. Including a M&M section and putting the density numbers in a table will help to improve this part.

The last two paragraphs in Results and Discussion are very general and obviously, the authors suddenly aim to explain not only the Indian Ocean system, but the carbon flux global system under the impact of climate change with a model - which I think is absolutely not possible on the data presented in the current paper.

Further, the parameterization of the air sea flux of CO₂ following Wanninkhof (1992) (is this still accepted, 25 years later?) and the chosen parameters (e.g. 280 ppm of atmospheric CO₂) make me doubt the actual value of the presented model.

Answer: Considering the discussion in Takahashi, et al. 2009 we got the impression that Wanninkhof (1992) is still acceptable for a box model, which aims at studying the influence of the ballast effect on the CO₂ uptake of the biological carbon pumps.

Takahashi et al. modified a constant from 0.39 to 0.26.

W : $k_{av} = 0.39 * u^2 + (Sc/660)^{-0.5}$ T: $k_{av} = 0.26 * u^2 + (Sc/660)^{-0.5}$.

This could also be done in revised version. However, there was also the suggestion remove the box model from the ms (see our comment to SC1).

Finally, the last two paragraphs of the Results und Discussion contain several peculiar formulations/ concepts (e.g., p.14, line 9/10 “a successful ecosystem” – what is this?; p.14, line 29 “global warming might favor the export of particles [: :] by lowering the seawater density”, p.16, line 27/28 “all nutrients are regenerated nutrients, which means they are attached to CO₂), which make me wonder what the authors want to state here.

Answer: The aspects can and we will clarify in the revised version of the ms.

In general: I would strongly recommend reducing the number of tables and figures and present figures/tables where you present data absolutely necessary for your work

Answer: following the reviewer comments we have to add two to three more tables but we will try the remove the number of figures.