

## ***Interactive comment on “The Ballast Effect in the Indian Ocean” by Tim Rixen et al.***

### **Anonymous Referee #2**

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Rixen and co-workers aim in the title and the abstract of their work to investigate the relative importance of primary production and ballast effect on organic carbon export in the Indian Ocean. Based on field data from several long-term sediment traps and some “experiments”, the authors conclude that their data show that the organic carbon flux in high productive systems could be as high as in low productive systems influenced by river discharges.

General comments: It is certainly an interesting approach to investigate the influence of lithogenic material discharged by rivers on the marine organic carbon flux and I definitely agree that this may cause high vertical flux also in low productive systems, but I do not think that the data presented in this work are sufficient to proof this. Most of the sediment traps of this study are located quite far from land (Figure 2) and no details are presented 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

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at EPT and JAM), and could cause ballasting there. Instead, the authors give mainly general statements on vertical flux and ballasting, which are hard to read, because they do not focus on the central theme (Indian ocean) but switch between upwelling areas, high/ low latitudes, different regions, without any clear logical order. In addition, the manuscript is quite wordy and lacks the Material and Method section as well as a clear structure (e.g., results are presented in section 2.2, even if section 2 is titled “study area”; methods like the formulae on sinking velocity in the Results and Discussion section). In conclusion, I think the basic approach of linking sediment trap data with the river discharge and lithogenic input is highly interesting, but this manuscript does – in the present form – only present few field data that are (in a rather confusing way) put into “experiments” and some very general conclusions drawn from it. I would therefore recommend the rejection of the present work.

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.

Page 1, line 9: “data obtained from a sediment trap experiments” – remove “a” and change “experiments to “deployments”

Page 1, line 21: Please explain what preformed nutrients are, if you want to use this expression in the abstract, because I do not think that this is a very commonly used expression.

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. In addition, the sentences are often unclear and difficult to understand (e.g. page 1, line 29: “photosynthesis and precipitation converts [ . . . ] dissolved inorganic carbon into particulate organic and inorganic carbon” - I have problems understanding how dissolved inorganic carbon is converted into inor-

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ganic carbon?). At the end of the Introduction, the two sentences are used to describe the Material and Method used in this manuscript, which is in my opinion absolutely insufficient.

Page 1, line 31: carbonate counter pump: Please give a short, well-formulated definition how this pump works the first time you mention it to give your reader the possibility to follow your thoughts

Page 1, line 31: “opposing effects” – which??

Page 2, line 9ff: “key sites at which this occurs are regions of deep and mode water formation [...] Here first of all upwelling and deep mixing entrains nutrients from the deep ocean into the surface.” This just does not make sense for me at all.

Page 2, line 25: “No reliable methods to measure export production.” Why not? Don't you think sediment traps can be used?

Page 3, line 2: “feeds the microbial food web”. Strange wording. I think not the food web is fed, but microbes.

Page 3, line 5: “Contrary to this passive transport POC forms large particles sinking out...” Is sinking not also a passive process?

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. 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.

Page 6, line 6: “reverse circulation in the surface ocean circulation (Fig. 3-4)”. I guess you refer to Fig. 4?.

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Page 6, line 29 to beginning of page 7: All these statements are neither supported by data in the current paper nor literature. I wonder therefore really on which background the authors make them.

Page 7, line 4: When where these fluxes measured? Please give detailed deployment times (incl. periods over which the flux was integrated into one sampling bottle)

Page 7, line 22: “data could [...] be directly linked to each other” – how?

Page 7, line 23: Typo “Fug.”

Results and Discussion This section starts with a description of primary production and vertical flux at the sampling site and the authors state that they found a trend of spatial and temporal variability of the ballast effect in their data. 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. 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. 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”. 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. 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  $84.5 \text{ m d}^{-1}$  in the first experiment (p. 11, line 21), but on page 12, line 4 and 6 they suddenly use completely different sinking velocities). The last two paragraphs in Results and

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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 CO<sub>2</sub> following Wanninkhof (1992) (is this still accepted, 25 years later?) and the chosen parameters (e.g. 280 ppm of atmospheric CO<sub>2</sub>) make me doubt the actual value of the presented model. 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 CO<sub>2</sub>), which make me wonder what the authors want to state here.

Page 7, line 29: “although the same problem” – which problem??

Page 7, line 31/32: please give numbers on primary production and vertical flux, not only relative “higher” and “lower” statements

Page 8, line 1: “suggests that a less efficient respiration [...] increases the carbon flux off Java” Why is the respiration less efficient? Please elaborate, why you think it is like that

Page 8, line 4: Please present the equation by Eppley and Peterson and move this sentence to a M&M section – with a detailed description on how you did the monthly means of which data. Your description here is very confusing for me.

Page 8, line 13: Which trend and why do the data scatter – what does that mean?

Page 8, line 20ff: It is very difficult to understand which global data were used and how the recycling efficiency was determined

Page 11, line 13: Grammar “this parameters”

Page 11, line 13-14: Which “measured ones”? Where are these data presented?

Page 11, line 30: Figure 3.13 a – I guess this should be 13a?

Page 12, line 24: Where have the sediment traps in 1994/95 been deployed?

Page 12, line 23/24: “support the results” – in which way? How do they support them?

Page 13, line 1: Which sediment trap data did you use for this global compilation?

Page 13, line 3-5: I have a hard time following your thoughts here.

Page 17, line 9/10: “This implies that the ballast effect is strongest at high latitudes in the regions where the nutrients are formed” – I have really problems following this conceptual understanding

Page 17, line 26: “more marine dominated areas” - What do you mean by that?

Figures/Tables

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.

Fig. 1: What is “protected organic carbon”? Fig. 12: Which literature?

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