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Interactive comment on "The Ballast Effect of Lithogenic Matter and its Influences on the Carbon Fluxes in the Indian Ocean" *by* Tim Rixen et al.

Anonymous Referee #2

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The submitted paper by Rixen et al. contains an impressive data set on particle fluxes to the deep sea from the Arabian Sea, Bay of Bengal and a single site at the Java margin. Given the importance of the Indian Ocean in the global carbon cycling, its large storage potential for carbon at the sea bed, climate feedback effects of the monsoon system, and large land-based runoff, all in close proximity, the paper presents a timely and welcome data set. Rixen et al. show that the ballast effect of lithogenic material can greatly increase organic carbon fluxes. Particularly intriguing is the implication that human land-based activity, through influencing river runoff, could enhance the strength of the biological pump. The basic conclusions of the paper are not new, but support and add to what has already been published on particle fluxes to the deep sea and add detail to this region.

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Discussion paper

The authors' use of a simple box model gave the impression of being almost an afterthought to the paper, and it was neither introduced nor is it extensively and critically discussed. I have however not concentrated on this aspect in depth in this review.

The paper makes difficult reading and the very imprecise use of terms, ill-defined concepts and methods make a critical evaluation difficult and laborious. I recommend that the paper be re-submitted after a major revision in order to make it readable and thus allow better evaluation of its content.

Some comments follow as examples:

Abstract :

Page 1, line 15:

"our results suggest that a preferential export of organic matter in slower-sinking particles reduces the transfer efficiency of exported organic matter in high-productive systems compared with low-productive regions."

What is meant by comparing "systems" to "regions"? Is the comparison between sites or between seasons? Fig. 7, that is presumably referred to here, uses data (not shown individually) split by seasons. The authors intention is not clear; do they mean that both high-and low transfer efficiencies can exist at different seasons at one site, or do they mean that they differ between sites on the annual average? With unclear wording, this is difficult to decide. As noted below, the seasonal data (split into its components POC/lithogenic/carbonate etc.) should be shown in this manuscript.

Line 20

"By enhancing the export of organic matter into the deep sea, the ballast effect increases the residence time of these nutrients in the ocean"

presumably the deep ocean is meant – nutrient residence times in the (entire) ocean remain dependant on sources and sinks, which are not affected by the ballast effect.

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Introduction:

P3, lines 23-24: The rationale to this box model – why, state-of-the art etc. should be given, or the box model left out of the paper altogether.

P3, line 25 onwards. Part 2. “Study Site” is a lot of textbook information, it is not clear what is necessary for this paper. Should be shortened and made more concise.

Methods: P5, line 17-22: should be part of discussion, not methods

P5, line 25 variability is < 17% not <+ 17%

P5, line 26. What justification do they have for ignoring inter-annual differences in flux - just the relative standard deviation (not the standard deviation, as they say), compared to a general trapping efficiency (literature value), is doubtful reasoning. Especially in an area where inter-annual differences in the strength of the monsoon can be expected to cause corresponding flux differences, this needs to be expanded on. Though relative SD is “only” 17%, the ranges are large – between 43 and 69 gC/m2/yr (over 50% difference) at WAST for example. The authors may be missing important insights by ironing over inter-annual variations.

P5, line 28. The seasons are referred to differently throughout the manuscript – winter/winter monsoon, summer / summer monsoon, intermonsoon, rainy season. This makes laborious reading; please standardize.

General questions on methods:

When delineating seasons have the authors accounted for the time lag of several weeks to a month that it takes for material from the surface reach the deep-sea? Which surface productivity areas have they taken to compare production to flux? Have they used particle backtracking?

P5, line 30 NEAST and EP3 are left out of Table 3. Why?

P6, line 10. Easier to follow later if export production is abbreviated as POCexport

P7 Sinking Speeds: Table 4 shows the values used for calculation and these are given in the text, but justifications are not forthcoming. Is the temperature of 10°C realistic? What is the temperature dependency of the results? Similarly, for salinity. The authors show in Fig. 2 that their traps were in a region of widely varying T & S, and indeed this is what characterises the Indian Ocean. So where are the limits of applicability of their calculations? Indeed, they vary density and keep the other variables constant, but perhaps it is density that should be constrained and the other variables altered. This needs to be better justified.

Results and Discussion:

After struggling through sections 4.1, 4.2 and 4.3. I could not glean clear messages.

4.1 Organic Carbon Fluxes into the deep sea: The first paragraph describes previous literature results and indicates that seasonality depends on distance from the coast (intentionally or wrong sentence structure?). The second paragraph describes Figure 4.

What is the main message of this section? What is discussed?

4.2 Java in comparison to the western Arabian sea: Why compare these? Why then leave out the (comparable, since closest to the margin) EPT station? What does one learn from this comparison? The text brings up several possible explanations for seasonality in the Java traps, but then negates them all. The seasonal lithogenic data are cited but not shown in the tables or figures.

4.3. Primary production and organic carbon fluxes: This section made very confused reading (see detailed comments below). Besides comparing three models for calculating and extrapolating fluxes (equations 1,2 and 3), and finding that they differ widely, there is no clear message. What do we learn from this? What POCexcess represents is not clear, making it difficult to comprehend what Figure 7 shows (where data are left out of the regressions with no justification.) The main message, that lithogenic matter

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enhances POCexcess flux (but see above) is stated but not critically discussed. In several cases, very convoluted sentences and imprecise phrasing makes comprehension difficult. It would help if a clear message was given at the start of these sections, and the discussion brought in support of these. In several cases, the authors talk of seasonal fluxes, but these are not shown anywhere, making it difficult to follow based on the figures and tables. These should be shown.

Detailed comments:

P10, line 18. Fig 2 does not show pronounced phytoplankton blooms

P10, line 25-27 This sentence says that seasonality depends on distance from the coast. Is this intended or is the sentence wrongly formulated?

P10 line 28 At which sites – AS or BoB?

P11, line 2 – what does ENSO have to do with this here?

P 11, line 4 – Fig 2 e and f does not show this

P 11, 4.2 What is the message of this paragraph? None of the explanations apply, since they are negated in the discussion. Lithogenic flues at JAM are the highest compared to some stations in the western Arabian sea but not to EPT (the most similar in terms of being near-margin), where they are even higher. Does proximity to the coast play a role? The EPT trap is at a very shallow depth, so direct comparison of fluxes is difficult. Why is EPT left out of Tables 2, 5 and 6 and not discussed here?

P 11 line 22. Data not shown (lithogenic matter 60%) . Which are the “rainy season” and “upwelling season”? Difficult to follow this reasoning.

Line 24 lithogenic matter >55% - where is this shown? Which season? P 11, line 30 “the seasonally averaged organic carbon fluxes and export production rates were compared” Where are seasonally averaged fluxes shown? It took a while to figure out that export production was actually POCeuphotic. Use POCexport throughout or call it

euphotic zone export for easy understanding.

P11, line 31 – 32. The sentence is unclear. “The ratio between the organic carbon flux and the export production defines the transfer efficiency (Teff) of the exported organic carbon (Francois et al., 2002). Multiplied by 100, it represents the share of the export production rates, which is respired in the water column.“

Do the authors mean “The ratio between the organic carbon flux at trap depth and the export production defines the transfer efficiency (Teff) of the exported organic carbon (Francois et al., 2002). Multiplied by 100, it represents the share of the export production rates, which is not respired in the water column.“

Perhaps I have misunderstood, but please clarify by precise wording.

P12, line 2 “.. and at the Bay of Bengal” At what depth?

Line 3 “.. of exported organic matter” at what depth? Do they mean at the base of the euphotic zone? And “reach the traps” – at trap depths varying from 1500 – 3000 m? Unclear what the message of this sentence is.

Line 3 “varying SST mainly causes this difference”. I presume the authors mean “Different SST values used” since SST did not actually vary.

Line 7. Between 5 and 72% of “the organic matter “ (WHICH organic matter?) “reaches the deep sea”. What does this exercise teach us? Using three varying formulae give widely differing estimates – the value of 72% seems unrealistically high. Again, what is the message from this exercise; the reader awaits a critical discussion.

Line 9. “Eq 1 is > 6 times higher”.. Surely they mean LOWER?? Or which values are referred to?

Line 15. Why is a Michaelis-Menten model used? The data appear to show a threshold cut-off at low export production. Perhaps a two-step linear relationship, with a shift at around 50 gC/m2/yr, maybe more appropriate. Please justify.

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Line 17: did diatoms dominate the traps in these seasons in this study? Para starting line 20 and Fig 7a: If I have understood the text, the red line is merely the inverse of the black one, so what is its use? It is not clear – even after re-reading several times, how POCexcess is calculated and, above all, why? This appears to hold a circular argument. It is not clear why the use of Eq.1 is emphasised for Fig 7b – in fact, the text does not allow clear understanding of this entire paragraph.

Sentence starting line 26: surely the reference is to Fig 7c?

Fig 7: several data points are left out of the regression with no explanation. Some mechanistic understanding should be given.

This basic difficulty of comprehension and lack of messages continues throughout the paper, to which I am not responding in full. Notwithstanding these critical comments, I do feel that the data should be published and that a major re-write would be worth the effort. As it stands, as a reader I was hopelessly lost and at times intensely frustrated.

By bringing out a few key messages, that the data do contain, and limiting the discussion to only those aspects that relate to these, the paper could make a good contribution to understanding controls on particulate fluxes in the Indian Ocean.

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