Dear reviewer #2,
thank you for the time you spent on reviewing our manuscript and your valuable comments. You and Dr. Wilson who also acted as reviewer agreed that the way we introduced "Excess POC flux" was confusing and suggested to delete the modeling from the manuscript. As pointed already out in our response to Dr. Wilson we will remove the modeling and the "Excess POC Flux" section from the manuscripts.

Furthermore you criticized that our manuscript is difficult to read because of imprecise phrasings and messages, which are hidden behind convoluted sentences. The great detail at which Dr. Wilson commented on our manuscript indicates to us that it may be not as unfit as asserted by you. However, we are convinced that your suggestions to give a clear message at the start of each section and to standardize the terminology used will make our manuscript much stronger. Please find attached in a separate file our detailed point-by point reply to your comments.

Point to point reply

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.

Issues raised by the reviewer will be addresses below:

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.

Will be separated into another ms.

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?

Our NEAST record covers only one season and this season was considered, but NAST and WPT were left out because our record did not even cover one season. ETP was left out because the trap was deployed at water depth of 590 m. Extrapolating the ETP data to the water-depth at which the other traps were deployed (> 1800 m) causes large uncertainties.

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

Will be done.

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.

We have checked the influences of temperature and salinity and they were small. However, we will include T and S profiles obtained from the individual sampling site.

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?

It is only an introduction into processes, which have been suggested to control carbon fluxes at the sampling sites, and the seasonality is shown in figure 4.

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.

As mentioned before ETP was deployed at a water-depth of only 590 m. WAST and JAM represent two extremes: Both traps were deployed in seasonal upwelling systems. Despite lower primary production the fluxes at JAM were as high as at WAST, indicating firstly a decoupling between primary production and export production. Secondly, JAM was the only site at which the satellite-derived record on primary production overlaps with sediment trap data.

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 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.

The purpose of Figure 7a was to show the poor link between export production and organic carbon fluxes which for us was the first indication of the ballast effect. The Excess POC flux, which represents the deviation of the data points from the Michaelis Menten typ of trend line, supported this assumption, as it correlates with the lithogenic matter content. However, this will be explained more clearly or even left out as the correlation between lithogenic matter content and the organic carbon flux shows the same.

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

This will be changed into 'regions characterized by an enhanced primary production'.

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

Yes, because nutrients are consumed close to the coast and nutrient-depleted low salinity water hinders vertical mixing and associated nutrient inputs from below into the euphotic zone by increasing the stratification further offshore.

P10 line 28 At which sites – AS or BoB?
in the BoB - will be added

P11, line 2 – what does ENSO have to do with this here?
We will delete this.

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

? – Fig 2 e and f show primary production rates in the summer and winter, which in both cases are higher off Oman than off south Java. The respective data were

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?

As mentioned before ETP was deployed at a water-depth of 590 m and WAST and JAM represent two contrasting extremes.

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?

An additional figure showing lithogenic matter fluxes will be included.

P 11, line 30 "the seasonally averaged organic carbon fluxes and export production rates were compared" Where are seasonally averaged fluxes shown?

No but figure 4 shows the monthly means used to calculate seasonal means. Seasonal data will be added

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.

Will be done.

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.

Yes, that is what we meant. It will be corrected.

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

Trap depths are given in Table 1, but will also be mentioned in the text

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.

This will be clarified

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

This will be changed.

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.

We discussed this on page 15 but will move it to page 12.

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

It refers to the POC export production (export from the euphotic zone). At the same primary production the export production derived from Eq. 1 is higher as that from Eq 2. This will be clarified.

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.

It shows the best fit but as also suggested by Dr. Wilson other fits can be checked. However, since this approach is too confusing we suggest to delete it from the ms. (see below)

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.

As mentioned before Excess POC flux represents the deviation of the data points from the Michaelis Menten type of trend line. This could be explained more clearly, but considering the confusion it creates and since the correlation

between lithogenic matter content and the organic carbon flux shows the same, it will be left out.