The re-submission of the paper "The Ballast Effect of Lithogenic Matter and its Influences on the Carbon Fluxes in the Indian Ocean" by Rixen et al. has addressed some points brought up by both reviewers, most notably the omission of the box model calculations and of the "POCexcess" term used. These have reduced the bulk of the document.

For re-evaluation towards final publication I concentrated on specific revisions based on review comments already made, the arguments made in the discussion and the general readability of the document.

Readability: Unfortunately it is not my opinion that this has improved, and this is the primary setback to recommending publication. The abstract alone makes difficult reading. The second sentence refers to the "open Indian Ocean" i.e. all except the JAM site. But at the JAM site, it is only one season which falls out of this pattern, the other fits perfectly (and it is unclear why see comment below). This should be specified. The next sentence " At trap sites in the river-influenced northern and central Bay of Bengal and off South Java lithogenic matter was the main ballast material and its content strongly influenced organic carbon fluxes favoured by weakly pronounced variability of primary production at our trap locations in these regions." What does this mean "favoured by weakly pronounced variability of PP"? So if the variability of PP was strong, lithogenic matter would not favour organic carbon fluxes? This I did not see in the paper, so how can it go into the abstract? Line 21 speaks of the "low productive Java Sea" but this is not borne out in the data where does this assertion come from?
There are numerous typos and errors in the MS, particularly in the figure legends, that should be picked up.

Previous comment:
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 $gC/m^2/yr$ (over 50% difference) at WAST for example. The authors may be missing important insights by ironing over inter-annual variations.
Revised manuscript: Authors move this section to the Results & Discussion (p 8, l 17-27). The paragraph where they discuss this does not convince. True, they have no estimates of PP in the individual years, and perhaps the temptation is to assume that mean annual values are a good proxy for what is happening at the sites. Maybe they are even right, but the argument does not hold up. We learn nothing by comparing 61% interannual differences (that could be due to differences in PP) to a 60% trapping efficiency estimate in a totally unrelated study, except that the numbers 60 and 61 are close to each other. The authors conclude " *However, the mean interannual variability was only 16.6 % implying that on the long-term run the reproducibility of the organic carbon fluxes measured by our deep moored traps was much better than the possible error range of 60% and thus the error of the calculated monthly, seasonal, and annual means used in the following discussion is much lower."* This is a tenuous statement.

Again, reading is laborious if one takes the authors literally. An example: *In order to estimate possible error ranges we calculated and compared annual mean organic carbon fluxes (Tab. 3).*
But this comparison is not useful to estimate "error ranges" (whatever that means) it is useful to estimate variability of flux!! Further, the table shows annual fluxes where they were measured for at least 150 days (less than half a year depending which season was not measured, the entire difference could potentially be accounted for by this). At the JAM site in 2003, only the period of high flux was measured (after which data are not available) so obviously the mean flux would be higher this doesn't show anything (lines 18 and 19).

Previous coment: 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.
Author response: We have checked the influence of temperature and salinity and they were small. However, seawater temperatures and salinity were selected from the World Ocean Atlas 2013 for each trap site and presented in Table 5.
Revised manuscript: Please present these calculations if you made them.
How does this help? Are "sea water temperatures" mean over the entire water column? Their contrast to the SST shows that large changes in temperature could indeed affect sinking speeds differently at different depths. If the authors tested this, it would be good to see the results.

Results and Discussion: have been re-arranged.
4.1 I commented upon above.
4.2 is titled " Seasonality and Java in Comparison to the Western Arabian Sea" How can one compare a characteristic (seasonality) to a site (Western Arabian Sea?). We learn that the Arabian Sea and Bay of Bengal have more and less pronounced seasonality respectively, which is well known and understood (the literature is cited). The last paragraph states that " *Monthly mean satellite-derived primary production rates, which we selected for the trap sites and sediment trap data show a similar seasonality (Fig. 3 b,d)* But exactly this is negated in the last sentence of this paragraphs in which this "similar seasonality" is not seen at JAMS.

4.3 tries to explain this discrepancy, but fails to provide more than rather confusing conjecture. The first merely states that the satellite data may be too weak to make this comparison in the first place. The second reasoning states that reconstruction of SST from foram shells correlated well with satellite-derived SST (but this SST was doubted previously due to cloud cover!). It is not clear in which depth the forams grew, at which temperatures, whether these varied seasonally etc. etc. so this argument is either accepted on good faith or not substantiated. I chose the latter. Given the generation times of forams, it is entirely possible (probable?) that freshly sedimented forams (bringing with them a load of organic matter from the sediment surface) resuspended at the shelf/slope could have been laterally transported to the deep traps, and bring with them this signal. Indeed this is the most likely explanation. The JAM site is unlike all others - a continental margin site, and these are characterized by large amounts of lithogenic material being resuspended (often where the M2 internal tide impinges on the slope) and transported to deep traps. This lithogenic material could sweep large amounts of organics with them if this were the case, then the effects of lithogenics on sinking speed would not the be the causative factor for the pattern seen.
In the original manuscript, the authors even mention freshwater diatoms found in the traps that would, qualitatively, support the strong lateral transport hypothesis, but this is missing from the revised manuscript.
For the third conjecture, in the last paragraph, the authors use a 14% increase in lithogenic contribution in winter to explain the strong pattern seen between December and March (fluxes increase by 100% though PP stays constant). It seems extremely unlikely that this would be adequate in strength as a cause.
The alternate explanation, that laterally transported lithogenic and organic matter-rich material (possibly with a riverine source) reached the trap, is not explored.

Previous comment: 4.4. 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? The main message, that lithogenic matter enhances POC flux is stated but not critically discussed.

Revised manuscript: This comment carries forward to the revised manuscript

In conclusion, the paper has been improved to the extent that omissions have been conducted that shorten it. The basic weakness, that arguments are hard to follow and inconsistent, and that the manuscript makes difficult reading and thus burdens reader evaluation, remain. If the purpose of a scientific paper is to allow evaluation of data and argument, this one falls short. However, it still contains a good data set, which it would seem a shame not to publish. I leave it to the editor to make a decision on publication. Should it be accepted, there are numerous typos and errors in the MS, particularly in the figure legends, that should be picked up and a language editor should check the paper.