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

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I am interested in this manuscript having previously published on the ballasting hypothesis and because the manuscript uses the sediment trap dataset I compiled for that paper (Wilson et al., 2012). I have a number of specific comments concerning the use of the dataset in this manuscript as well as some general comments having subsequently read through the manuscript.

In general, I found the premise of explicitly examining ballasting of organic carbon in the Indian Ocean interesting as this is an area where the role of lithogenic material may be potentially very different to other regions in the global ocean, helping discern between the potential roles of lithogenic material and $CaCO_3$ in the ballast hypothesis. However, I think that there are some general issues that stop this being realised (see comments below). I suggest that the biggest potential of the study lies in



the seasonal data highlighted in Figure 5 which is briefly mentioned. Given the range and source of seasonality, its relationship with riverine fluxes and lithogenic fluxes, an approach comparing timings of POC and ballast mineral fluxes using similar statistical measures used previously ("carrying coefficients": Klaas and Archer 2002; Wilson et al., 2012) would be extremely interesting.

## 1 Specific Comments on the use of the Wilson et al., (2012) dataset

I have the following concerns/questions about the use of the compiled sediment trap dataset from Wilson et al., (2012) that I would like to see addressed:

1. The dataset is presented in Figure 11b in the black dots. The numerical values of the data do not match the dataset in Figure 1b of Wilson et al., (2012). I have included the equivalent figure, plotted directly from the supplementary material of Wilson et al., (2012), with this comment for comparison. POC is reported in units g m$^{-2}$ year$^{-1}$ in both figures but the upper value in the manuscript is $\sim$ 2.5 rather than 7.0 in the original figure. I cannot find any description in the text stating that the dataset was specifically subject to any filtering or corrections. Notably, this is not the case in Figure 13c where the POC axis values are the same! Figure 11b also reports mass fluxes of PIC, again in g C m$^{-2}$ year$^{-1}$, and again the axis values are wrong (compare Figure 11b with the figure included in this comment). Figure 11b shares visually similar features with the original plot so I am unsure what has happened. Any change made to the dataset or the way it is reported should be described clearly in the manuscript text.

2. The dataset in Wilson et al., (2012) consists of 156 datapoints but Figure 11b reports 104 datapoints. I am unsure if this includes the additional data from the Indian Ocean but either way a third of datapoints have been omitted. This also

seems the case for Figure 13c. This is also evident from visual comparison of Figures 11b and 13c with the equivalent replicated figures in this comment. Again I can find no description in the manuscript text describing what has been omitted and why. This also requires clear description in the manuscript text.

3. The dataset is not cited properly in the figure caption for Figure 13. It reads Wilson et al., (2002). It should be (2012).

## 2 General Comments

*Treatment of uncertainty in Section 3.2 on Ballast Effect*

The choice of parameter values when estimating factors such as density and sinking velocity in this section seems somewhat arbitary and does not include an assessment of the uncertainty associated with these values. Only one estimate of export production is considered when there are other models available that may differ in magnitude (see Henson et al., 2011). The authors take the mean of coccolithophore and foraminifera $CaCO_3$ densities (1.55 g cm$^{-3}$ and 1.7 g cm$^{-3}$ respectively. Another cited density of 2.71 g cm$^{-3}$ for calcite is not considered) to use as a representative density for $CaCO_3$. Opal is treated similarly. However, a similar spread of values for lithogenic material (1.4 to 2.72 g cm$^{-3}$) is not treated in the same way and the upper value is used. This treatment seems inconsistent and given that, within the ranges stated, the density of lithogenic material could be lower than $CaCO_3$ the current findings may be biased towards finding lithogenic material as the most important ballasting mineral. The authors should demonstrate that the results are robust to these uncertainties. Choices for parameters such as the decay rate should be also be justified.



The authors present an estimation of the density of particles in equation 11 as a mass-weighted average of the densities for each flux component (e.g., $\frac{\sum_{i=1}^{n} w_i x_i}{\sum_{i=1}^{n} w_i}$ where $w_i$ and $x_i$ are the $i$th weight and data point of $n$ samples). The sum of weights (the % of each component) for every sample in Table 2 are $<$100% because the total flux reported does not equal the summed masses of POC, $CaCO_3$, Opal and Lithogenic fluxes (Table 2). Therefore, dividing by 100 is incorrect and instead it should be the sum of the weights (% of each component) for each respective sediment trap sample.

*Box Modelling*

In general, I find the the box modelling in this manuscript to be a black-box exercise. The model is insufficiently described: there are no governing equations described, there are no parameter values given, and the description in the text is extremely minimal. As it stands, there is not enough information to assess what is in the model or whether it's appropriate. Therefore it is impossible to reproduce or validate this model and its results. For example, it is not clear whether a POC/PIC rain-ratio of 0.7, cited from Klaas and Archer (2002) for depths $\sim$1000 to 3000m, is used as an export ratio or is applied at depth. The manuscript does not describe how the export of $CaCO_3$ and attenuation of the sinking flux is represented in the model which is a key component of ballasting and it's interaction with the carbon cycle. At the very least, an adequate description (equations and parameter values) could be included as supplementary to the manuscript. Ideally, this would include the model code or a link to a repository containing it.

The discussion of preformed nutrients also seems moot here. It seems as if they are not actually modelled and if so any interpretation of the interactions between POC fluxes, preformed nutrients and $CO_2$ are inferred rather than quantitatively demonstrated. The dynamics of preformed nutrients and atmospheric $CO_2$ have been extensively explored in models (Ito and Follows 2005; Marinov et al., 2008a; Marinov et al., 2008b; Duteil et al., 2012) and demonstrate that the Southern Ocean is a fundamental region in setting the global balance between preformed and regenerated nutrients. The box model here does not include a representation of the Southern Ocean or high latitudes and is therefore not appropriate to use for preformed nutrients or to base interpretations of high versus low latitude sensitivities. Lithogenic fluxes are significantly spatially variable so it is also unclear how appropriate a global box model with no high/low latitude or basin resolution may be in resolving their importance.

Similar modelling experiments using a box model to explore the effect of $CaCO_3$ ballasting on atmospheric $CO_2$ have been published previously by Barker et al., (2003) which is not cited in the manuscript. This paper is important context for the modelling in the manuscript. In general, I am not sure whether the global modelling adds to the understanding of ballasting in the Indian Ocean or how it is informed by insights into ballasting in the Indian Ocean generated by the manuscript.

## 3 References


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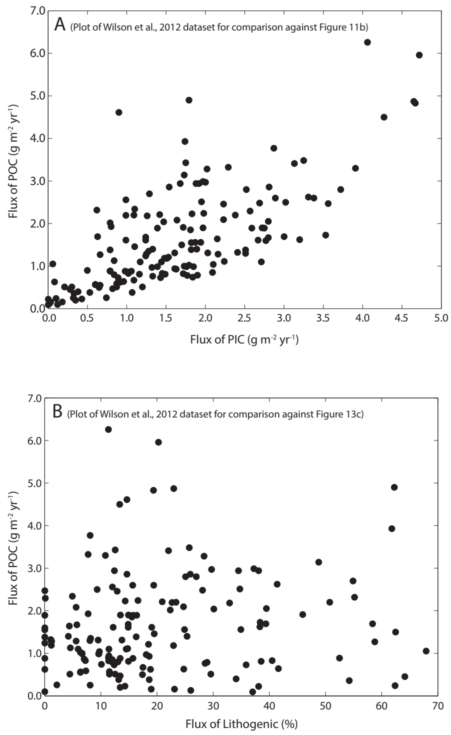
A (Plot of Wilson et al., 2012 dataset for comparison against Figure 11b)
Flux of POC (g m-2 yr-1)
Flux of PIC (g m-2 yr-1)
0.0
1.0
2.0
3.0
4.0
5.0
6.0
7.0
0.5
1.5
2.5
3.5
4.5
B (Plot of Wilson et al., 2012 dataset for comparison against Figure 13c)
Flux of POC (g m-2 yr-1)
Flux of Lithogenic (%)
0
10
20
30
40
50
60
70

**Fig. 1.**