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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.

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Dear Dr. Wilson, first of all we would like to thank you for the time you spent on our manuscript and for the valuable comments and suggestions, which will certainly help to improve our manuscript. Please find below our general response and a detailed point-by point reply to your comments, which is attached in a separate file.

General response: You and reviewer#2 suggested to separate the modeling section into another paper and agreed that the way we introduced "Excess POC flux" was confusing. Furthermore in your opinion calculating organic carbon fluxes into the deep sea based on our current mechanistic understanding is not helpful due to the large uncertainty of the required parameter values and you disagree with the approach that we

adapted the equation introduced by Henson et al. 2011 to calculate export production based on primary production to the regional distinctions at our traps sites. You also did not accept the way we interpret the large differences in export production which occur if primary production is converted into export production by using the equation introduced by Henson et al. 2011 and Eppley and Peterson 1979.

We appreciate your suggestions and will defer the modeling section into another paper. Excess POC flux represents the deviation between organic carbon fluxes measured by sediment traps and those calculated by using a trend line resulting from a regression analysis, including export production and measured organic carbon fluxes. Excess POC flux correlates with the lithogenic matter content, which to us is a strong indication of the ballast effect: It distorts the link between export production and organic carbon flux at our trap sites and thus prevents the establishment of a convincing correlation between organic carbon fluxes and export production. We agree that trying to prove the influence of the ballast effect by showing that there is no convincing correlation or trend line and then using it to calculate Excess POC flux is confusing. Considering this, we understand your doubts. This approach will be removed from the ms as our simple correlation and the MLR also show the impact of the ballast effect on the organic carbon flux. This in addition to deleting the modeling section will help to considerably shorten and streamline the paper. However, correlations only indicate possible links, but without a mechanistic explanation the nature of the links remains elusive. Equation 10, which was obtained from Banse 1990, describes the individual processes controlling the carbon flux into the deep sea: export production, respiration and sinking speed. We derived export production from primary production by using three different and well-accepted approaches and choose parameter values from the literature to calculate respiration rates and sinking speeds. The correlation between the calculated and measured fluxes is a measure of the precisions of this modeling approach. Due to the large uncertainties, parameter value can be selected in a way that precision decreases. In contrast, selecting parameter values to improve the correlation between calculated and observed organic carbon

fluxes is a way to constrain their range. This in turn supports the interpretation of results we obtained from the correlation between POC fluxes and ballast minerals and MLR because it explains the underplaying nature of the links established by the correlations. Furthermore the attempt to link primary production and POC fluxes measured by traps by considering the individual processes described in equation 10 points also to problems. One problem is the large difference in export production obtained by the three different equations we used to convert primary into export production. Reviewer#2 suggests that this is addressed in an expanded discussion. You in turn suggested to state that the equation introduced by Henson et al. 2011 cannot be applied to our sediment trap data. You also disapproved to adapt it to the data we obtained at our trap site by modifying a constant. At this point we do not agree. Export production is an ecosystem function and ecosystem change temporally and spatially. Accordingly, also the constant in the famous and widely used Martin equation was often changed. On the other side the export production derived from the Henson et al. 2011 equation fits best to our sediment trap data, but it indicates that $\sim$70% of the export productions reaches the deep sea. Reviewer#2 considers this fraction as unrealistically high. The concept of Armstrong et al. 2002 to segregate organic export from the euphotic into slow and fast sinking particles offers explanation that may explain the much higher export production rates derived from the Eppley and Peterson, 1979 equation. The equation of Henson et. 2011 describes the export in fast sinking particles and the Eppley and Person 1979 equation calculates total export. This is the export in fast and slow sinking particles, whereas the slow sinking particles are decomposed within the thermocline and do not reach the deep sea. Alternatively we could also simply say that the export production derived from the Henson et al. 2011 equation fits best to our sediment trap data. It suggests that $\sim$70% of the export production reaches the deep sea, which is much higher than widely believed. Considering these aspects and your very constructive and helpful minor comments we were very surprised that you finally recommended to reject the paper, which to our opinion is not justified.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2018-111/bg-2018-111-AC1-supplement.pdf

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