Interactive comment on “Export fluxes in a naturally fertilized area of the Southern Ocean, the Kerguelen Plateau: seasonal dynamic reveals long lags and strong attenuation of particulate organic carbon flux (Part 1)” by Rembauville et al.

Response to reviewer #2.

We thank the anonymous referee #2 for the careful reading of the manuscript and the constructive comments that have helped to improve the original version. Following the reviewers comments, we have significantly revised the manuscript. All of the reviewer’s suggestions have been taken into account and the resulting modifications appear in a revised version of the manuscript attached as a supplement to this answer. We are hopeful that our careful revisions address the main concerns of the reviewer and the revised version is now acceptable for publication in Biogeosciences.
R2-Cx : Referee comment, **R1-Rx:** authors response. Changes in the manuscript are in the supplement to this answer.

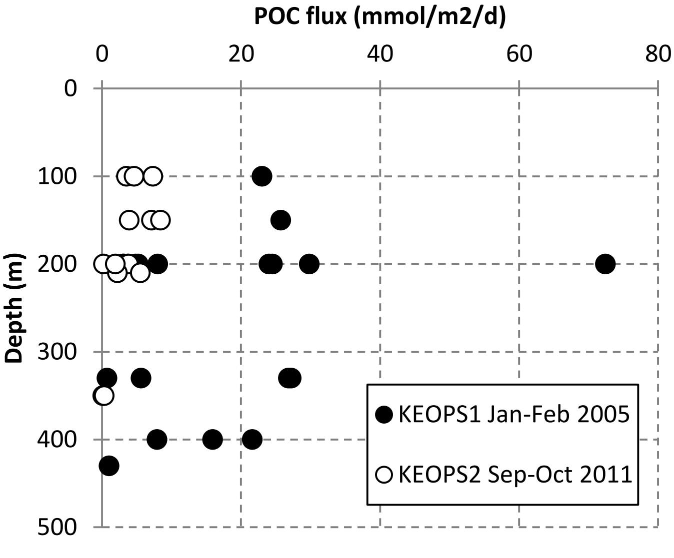
R2-C1. *The Southern Ocean is a large component of the oceanic carbon sink, and knowledge of particle export and remineralization in this area is important for understanding the global carbon cycle. This interesting manuscript presents a 1-year time series of mid-water particle export, collected using a moored sediment trap at 300 m depth in the Permanently Open Ocean Zone overlying the Kerguelen Plateau, together with a comprehensive set of physical parameters. Reported annual export rates were overall low (98 mmol m-2 y-1), with two periods of high export rates lagging peaks in surface chlorophyll by approximately 1 month. The manuscript is well written, the results are well presented and the method limitations and uncertainties are clearly explained. I do, however, not fully agree with the interpretation of the flux synthesis. In particular, the conclusion that POC flux is rapidly attenuated is not clearly supported by the results.*

R2-R1. We thank the reviewer #2 for finding the manuscript interesting and appreciate the positive comments about the presentation of our results. Given the concerns raised by the reviewers #1, #2 and #3 we have undertaken a major revision of our paper. Notably we have removed the flux attenuation calculations based on different flux estimation methods as well as the bacterial carbon demand calculation. Consequently the discussion sections 4.3 and 4.4 were fully rewritten. Following the reviewers keen interest in the temporal lags between chlorophyll and flux data we have dedicated Section 4.3 to the discussion of the seasonal patterns of export relative to the surface biomass accumulations. Section 4.4 has been rewritten to discuss the large differences between flux estimates at 200 m and 300 m by our sediment trap method, but indeed those of independent estimates. We have attempted to constrain the “trapping efficiency” of our sediment trap deployment by comparison to ^{234}Th estimates of POC export, although it should be noted that the latter technique is not considered to be an independent reference. Nevertheless, we find smaller fluxes measured by the sediment trap relative to those obtained with the thorium technique. Consequently we proceed to discuss the potential biases that might have impacted the collection efficiency of the moored trap, and finally a make direct comparisons with independent datasets at depths > 300 m that also report low POC fluxes. In spite of the calculated trapping efficiency there are still very large differences in flux estimates at 200 m and 300 m that are evident from multiple independent approaches and are consistent with attenuation reported in other areas of the

Southern Ocean. The discussion therefore ends on the short evocation of the ecological factors that might be responsible for the low fluxes at ~ 300 m.

R2-C2.*The main discussion focus of this manuscript is based on the comparison of flux estimates from a long-term moored sediment trap to those from short deployments of various sampling devices (drifting sediment traps, UVP, 235Th; Table 3). The authors argue that there is an apparent rapid reduction in POC flux between 200 and 300 m depth. The first problem with this analysis is that the individual flux estimates had been collected using a wide range of equipment types integrating over different time periods; a direct comparison of the individual measurements to calculate flux attenuation is therefore tricky. A closer look at the data in Table 3 reveals moreover that there is no consistent trend in fluxes with depth. For example, according to the UVP, fluxes increased with depth in Feb 2005. When plotting all POC flux data against depth from Table 3 an overall decrease of POC flux with depth becomes apparent. Fitting a power-law gives an attenuation coefficient (b) of 1.16, which is considerably lower than the proposed 7-11.3. Rather, this attenuation coefficient lies well within the reported range of 0.4-1.7 (Martin et al. 1987; Marsay et al. 2015). Looking at Figure 8, one may argue that there is strong flux attenuation in Oct/Nov. A power-law fit to the approximate data gives b ~ 5. Yet, the same problematic applies as discussed above: As fluxes had been collected using different methods, it is questionable how valid the regression is. Based on the presented data I therefore disagree with the notion that 'rapid attenuation of flux beneath the WML is a genuine ecological feature of the Kerguelen Plateau bloom.'*

R2-R2. The following figure summarizes all the direct estimates of POC export available for the A3 station. All the data are now reported in Table 3 in the new version of the manuscript and Figure 8 was deleted.



As pointed out by reviewer #1,#2 and #3, there is a high variability between methods that prevents the calculation of a robust attenuation factor with depth. This variability appears higher that the seasonal variability we observed in the moored trap and therefore the direct comparison of our data with various upper ocean estimates appears even more inconsistent. We deleted the b-values calculations that mixed the moored trap data and the other estimates. However, our data are close to other deep estimates (>300m) reported in spring by Jouandet et al., (2014) (0.1 – 0.3 mmol m^{-2} d^{-1}) and in summer by Ebersbach and Trull, (2008) (0.7 mmol m^{-2} d^{-1}).Calculating b-values from these two independent dataset lead to **3.3** and **4.1**. These values are significantly higher than the 0.5 - 1.7 range reported in Buesseler et al. (2007) and fall into the range recently proposed for the Southern Ocean of 0.9-3.9 (Lam and Bishop, 2007; Henson et al., 2012; Cavan et al., 2015). It is likely that under trapping due to hydrodynamics and particles consumption by zooplankton in the trap funnel (suggested by reviewer #1 as the main limit to this study, see R1-C2) can partly explain the low POC fluxes we observed at 300 m, but their relative importance compared to geniune flux attenuation cannot be estimated at present. We integrated this discussion in section 4.4.

R2-C3. *The authors next suggest mechanisms behind the apparently high flux attenuation. They explore the potential role of bacteria in remineralizing the POC flux by calculating bacterial carbon demand. The use of bacterial carbon demand in this context is incorrect; rather they should compare flux attenuation to bacterial respiration (see Giering et al. 2014). It is moreover not clear where the estimate for the flux attenuation (22.9 mmol m^{-2} d^{-1}) comes from. According to Table 3, averages for POC flux at around 200 m and around 300 m depth are 22 and 12 mmol m^{-2} d^{-1}, respectively. The POC loss between these two depths (~10 mmol m^{-2} d^{-1}) is similar to calculated integrated bacterial respiration (5-10 mmol m-2 d-1). As the data do not support the conclusion that bacteria cannot account for the loss of POC flux alone, the subsequent discussion is somewhat uncalled for.*

R2-R3. Giering et al. (2014) justify the use of the microbial respiration rather than microbial carbon demand by stating that at *'steady state'*, each atom of organic carbon that is not respired stays in the ecosystem loop and is likely to contribute to POC and DOC vertical fluxes. Authors also demonstrate that in the oligotrophic (ALOHA) and mesotrophic (K2) Ocean, microbial carbon demand largely exceeds carbon supply. This is a strong hypothesis that might be true at long timescale considering that biomass accumulation is null. However this hypothesis is questionable in our case in the very productive, naturally fertilized waters were primary production exceed microbial carbon demand in the mixed layer (Christaki et al., 2014) and where strong zooplankton biomass accumulation is observed in both spring and summer (Carlotti et al., 2008, 2014). Therefore we hypothesize that an important part of organic carbon used by microbial heterotrophic production does not return to vertical fluxes. This is why we choose bacterial carbon demand rather than bacterial respiration. However, as reviewer #1 and reviewer #2 considered that the previous flux attenuation calculation was not relevant based on the available data, we deleted the section concerning the microbial contribution to flux attenuation in the new version of the manuscript.

R2-C4. *Unfortunately, the current version of the manuscript needs major revision as the central conclusions do not appear to be supported by the analysis. Nonetheless, the core* *results of this manuscript are very interesting, in especially the offset between peaks in chlorophyll and export fluxes. I would suggest stepping away from analysing pieced vertical profiles and instead discuss the seasonal progression. There has also gone a lot of effort into the enumeration and identification of swimmers; I wonder whether it is worth looking into the ecology of these swimmers.*

R2-R4 Following on reviewers #1, #2 and #3 comments, we revised the manuscript and deleted the quantification of the attenuation of POC fluxes that integrate moored trap data and other methods. Notably, the discussion sections 4.3. and 4.4 were fully rewritten. Given the interest reviewers #1 and #2 formulated for the seasonal progression, we dedicated the new section 4.3. to the discussion of the flux seasonality relative to the surface chlorophyll signal. Moreover, the biological composition (diatoms and faecal pellets) of the export fluxes is fully detailed in the companion paper (Rembauville et al., 2014) and efficiently explains the seasonal dynamic of the export. A recent study in the Arctic reports in details the abundance and life stages of swimmers in a shallow sediment trap (Matsuno et al., 2014). In our study, the taxonomic level of swimmers determination was low and we suppose that swimmer catching by sediment trap is highly selective and probably not representative of zooplankton seasonal dynamics. The main purpose of swimmers enumeration was to make sure that they didn't correlate with any mass, POC or PON flux. Swimmers seasonal dynamic is briefly discussed is section 4.2 'Swimmers and particle solubilization' in the context of the seasonal mesozooplakton successions described in (Carlotti et al., 2008, 2015). Finally, faecal pellets fluxes are fully detailed in the companion paper (Rembauville et al., 2014) and also compared to mixed layer zooplankton observations in spring and summer (Carlotti et al., 2008, 2015).

We feel that our revised paper is an important contribution to the Biogeosciences KEOPS2 special issue because:

(i) Our paper serves as an important companion dataset for the detailed biological and geochemical analyses presented in part 2

(ii) The paper is significant in its own right because despite over 8 papers on export processes from KEOPS 1 and 2, our dataset serves as the only annual record of flux from this important iron-fertilized site. This has allowed us to uniquely identify the significant temporal lags between the accumulation of surface biomass and export out of the mixed layer.

(iii) Together with our own data we synthesize the various POC flux estimates obtained from the iron-fertilized Kerguelen bloom to bring together numerous lines of independent evidence supporting a scenario of significant flux attenuation between 200-300 m. We acknowledge however that our paper should represent these arguments in a more qualitative manner and more dedicated techniques would be required to quantitatively constrain attenuation. Our findings are thus in line with an emerging biogeochemical paradigm in the Southern Ocean that high biomass blooms fertilized by iron do not necessarily lead to significant export into the bathypelagic ocean.

We hope that following our careful attention to the reviewers comments and significant modifications for the manuscript our paper is now considered acceptable for publication in the Biogeosciences KEOPS 2 special issue.

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