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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* M. Rembauville et al.

Anonymous Referee #3

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General Comments This manuscript proposes that there is strong attenuation of POC flux in the spring bloom observed at Kerguelen in the KEOPS2 experiment. To this end it centres on the analysis of one year of sediment trap data from a deployment at 289m water depth. It also relies on comparison of this data with other KEOPS2 studies in this issue using a wide variety of methods including short deployments of free drifting conventional and gel traps (Laurenceau et al.); 234Th method (Planchon et al.) and video profiler (Jouandet et al.) as well as direct comparisons with KEOPS 1 results.

As such, this paper in particular and the associated papers in the KEOPS 2 set, in general, represent a microcosm of the issues around the understanding of biogeochemical ocean flux. These include the comparison of different sampling methods, different analytical techniques (both direct and indirect), that are often deployed at different times and for different durations.

This paper is especially ambitious. It is one thing to show a discrepancy between primary production (measured from incubation of water-column bulk sub-samples or from satellite-derived estimates) and export – the so-called "high biomass, low export" regime. It is altogether something else to demonstrate export attenuation or flux attenuation, since this requires that a particular settling episode is tracked downward through the water column.

One of the main concerns is with the potential errors arising from the reliance on the comparison of different methods in determining flux attenuation. The moored traps were deployed at one depth and so there can be no direct comparison with similar collection devices to determine flux attenuation. In its present form this paper does not convincingly demonstrate flux attenuation at the 90% level proposed. Furthermore, there appear to be a number of inconsistencies and errors that detract from the overall thesis. The paper therefore needs major revision.

Specific Comments: A case in point is the following paragraph from the Discussion section: Section 4.3 Rapid flux attenuation at A3 (p17058 lines 10-20) "The sediment trap record obtained from station A3 provides the first direct estimate of seasonal and annual POC export from the iron-fertilized Kerguelen bloom. The annual POC export of 0.1 molm-2d-1 at 300 m (Table 1) is significantly lower than indirect estimates of POC export (5.1 molm-2d-1) at the base of the WML (200 m) on the Kerguelen Plateau (Blain et al., 2007). The Kerguelen Plateau annual POC export approaches the median global ocean POC export value comprising shallow and deep sediment traps (83 mmolm-2yr-1, Lampitt and Antia, 1997), but is also close to values observed in HNLC areas of the POOZ (11-43 mmolm-2yr-1 at 500 m, Fischer et al., 2000). Moreover, the magnitude

of annual POC export measured at 300 m on the Kerguelen Plateau is comparable to deep-ocean (> 2 km) POC fluxes measured from the iron-fertilized Crozet bloom (60 mmolm-2d-1, Salter et al., 2012)."

There appear to be some inconsistencies in this section. Firstly, earlier, in Section 3.4, the authors state that "the annually integrated POC flux was 98.2 mmolm-2yr-1 (millimoles per square meter per year)", but in Section 4.3 they quote an "annual POC export of 0.1 mmolm-2d-1 (moles per meter squared per day)". Do the authors actually mean 0.1 mmolm-2yr-1 (moles per meter squared per year)? – In Section 4.4 they appear to revert to 98.2 mmolm-2yr-1 and it would help to keep units consistent. Secondly, the study of Blain et al. (2007: Nature, 2007, 446, 1070-1074) is quoted as reporting an estimate of POC export at the base of the mixed layer of 5.1 mmolm-2d-1 (moles per square meter per day). Scrutinising Blain et al. (2007), the nearest value that appears to correspond to this may be from Table 1 (of Blain et al., 2007): 5,047 mmolm-2 (millimoles per square meter) but this is in fact a "Seasonal budget" and so corresponds to an annual flux or flux for the growing season. When this is taken into account and converted to an average daily flux the value obtained would correspond closely with the figure for Thorium-derived POC export of 24.5 mmolm-2d-1 (millimoles per square meter per day) also given in Table 1 of Blain et al. (2007). Thirdly, the authors quote a value from Salter et al (2012) of "60 mmolm-2d-1 (millimoles per square meter per day)" whereas this should actually be per year (see Salter et al., 2012, table 2). Again, consistency in units might help prevent confusion such as this.

This section needs to be re-written with clarification. It would also help to have some consistency in units to support comparisons of daily or annual POC export.

In the next paragraph: p17058, line 21 "The POC fluxes.." The authors remark that the measured POC fluxes are low and discuss possible evidence for under-collection. Although they quote various studies that suggest errors ranging from 0.1 to 3 x compared with 234Th methods they omit to mention more recent work that shows a 20-fold underestimate of fluxes in moored conical traps as compared to free drifting traps also

in the Southern Ocean (Buesseler et al. 2010). The authors then appeal to a direct comparison with KEOPS1 results. (p17059 lines 2 – 5) "Even if we assume that our A3 sediment trap did undersample the particle flux, it seems unlikely that this in itself could explain the significant reduction in POC flux observed between 200 and 300m (Table 3)." The case for comparison to a different season in an earlier year is not well made. This notwithstanding, one of the 4 comparators in Table 3 actually indicates a 15% flux increase at 330m relative to 220m.

Next there is a comparison with other KEOPS2 studies using contrasting techniques: short deployments of free drifting conventional and gel traps (Laurenceau et al.); 234Th method (Planchon et al.) and underwater vision profiler (UVP) (Jouandet et al., 2014). The only one of these to sample at a similar depth is the UVP that measures particle size and concentration. This has been related to flux, but this single point from a method that does not collect settling particles is not compelling. As the authors correctly state: "The diversity of approaches prevents absolute comparison of the fluxes."

A one month lag between productivity and export peaks is invoked based on comparison of the largest sediment trap fluxes and satellite measurements of chlorophyll a - derived surface productivity. This supposes that the production that generated the flux occurred within the satellite detection depth limit of around 20 m. In fact there is increasing evidence that production that contributes substantially to POC flux may occur deeper than the satellite detection limits or not contain sufficient chlorophyll a to cross the threshold for satellite-defined blooms (e.g. Villareal et al. 2011, Journal of Geophysical Research).

Technical corrections P17046 line 9 "it's" incorrectly used P17047 Line 24 sentence: "Having access to. . .." – meaning unclear P17048 Line 12: sentence "Alternative explanations. . ." This does not seem to be an alternative explanation but rather restates the significance of zooplankton P17049 Line 7 "The net effect.." – needs "of" inserted. P17051 Line 12 1st sentence needs "was added" at end. P17056 Line 28 "shows" incorrect Figure 1. The isobaths "grey lines" cannot be seen Figure 6 – needs

more explanation in the caption

Interactive comment on Biogeosciences Discuss., 11, 17043, 2014.