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Interactive comment on "Carbon export and transfer to depth across the Southern Ocean Great Calcite Belt" *by* S. Z. Rosengard et al.

Anonymous Referee #1

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Review of Rosengard et al. "Carbon export and transfer to depth across the Southern Ocean Great Calcite Belt"

The manuscript by Rosengard et al. assesses the influence of the mineral content of sinking particles on the export and transfer efficiency of particulate organic carbon (POC). In situ pumps were used to obtain size fractionated particles which were analysed for both POC and mineral content (biogenic silica (BSi) and particulate inorganic carbon (PIC)). Comparisons are drawn between particle composition and flux data, and relationships are found between POC and BSi export out of the euphotic zone, and POC transfer efficiency and PIC export. The authors conclude that diatom-dominated communities support high export out of the euphotic zone but are rapidly remineralised in the mesopelagic, whereas communities dominated by small calcifying

organisms such as coccolithophores show lower attenuation in the mesopelagic. The study supports previous work hypothesising the importance of phytoplankton community structure on particle export and transfer efficiency. Overall, the paper is well written and needs only minor corrections before publication.

General comments

The authors fit the classic power-law curve of Martin et al. to their flux profiles. As they point out, this requires some careful thought about the relevant export depth. To avoid this, and to make comparison between stations simpler, an alternative is the exponential fit of Buesseler and Boyd (2009, L&O). If one makes the assumption that sinking speed is constant with depth, the estimate of remineralisation length scale is independent of choice of export depth (see e.g. Marsay et al., 2015, PNAS). Of course, the authors would have to check whether the exponential fit was robust for their data. In the discussion (page 2869), the authors actually switch to a discussion of remineralisation length scale which is inconsistent with the use of transfer efficiency and 'b' up to that point. Perhaps reframing the manuscript in terms of remineralisation length scale would be useful?

The paper provides some interesting particle composition data, which are used to explain trends in POC flux and suggest a strong role of ecosystem structure. It would be beneficial to the interpretation to include more information on the phytoplankton community composition in the study regions (using past studies if in situ data concurrent with sampling are not available). This would provide further weight to the explanations hypothesised by the authors. A plot showing diatom vs coccolithophore dominated regions based on PIC and BSi ratios could be a useful addition.

The authors have collected a wealth of data on particle composition with depth. It would be beneficial to show profiles of ratios of Th with POC, PIC, and BSi to give an idea of the variability with depth and hence how appropriate the choice of the ratio at a depth of ZPAR is for calculating the export flux. This information could perhaps be

included in supplementary information.

Specific comments

Page 2845, line 3: replace "∼1000 m" with "(from the base of the euphotic zone to ∼1000 m)"

Page 2845, line 8: add reference (maybe Kwon et al. 2009 Nature)

Page 2845, line 25: replace "remaining intact on its way down", with "reaching the deep ocean"

Page 2848, line 21: give duration of pumping and volume of water pumped

Page 2849, line 27: give duration of acid fuming

Page 2850, line 11-13: 'We focus our assumption. . ...because we assume they contribute disproportionally to sinking fluxes'. Are the authors able to test this assumption with their dataset? The <51um data which were collected do not seem to be used in the manuscript?

Page 2853, line 4: Why is the 0.3% light level chosen to represent euphotic depth, when it is more commonly defined as 1%?

Page 2854, line 4-6: The authors should identify somewhere (one of the tables?) which stations did not show a classic Martin curve.

Page 2858: The authors use 'productivity' or 'primary productivity' when they mean (satellite-derived) 'chlorophyll'.

Page 2859, line 23-27: could be worth tabulating these data from other studies to allow reader to easily compare

Page 2861, lines 14-16: Maiti et al. actually showed a strong correspondence between PP and export (just the direction of the relationship was opposite to previous studies).

Page 2862, discussion of T100>1 values: if the sampling took place post-bloom, when

PP is declining but export is still high, this may result in ratios >1, i.e. non steady state (Henson et al. 2015, GBC).

Page 2862, line 25: Are the authors able to specify the "sampling issue"?

Page 2864, line 20-24: sentence could be made clearer, also repeated "2.199"?

Page 2867, line 1: "further strengthening". . .. Do the fits of any of the other stations improve if only looking at upper 500 m? If attenuation over only upper 500 m was plotted against [BSi] and [POC], is a higher correlation observed?

Page 2868, line 22: what is the 'model diatom community'?

Page 2879, line 1-5: Figure 10b does not show a clear separation of diatom versus coccolithophore-dominated regions. Is there any statistical significance in the difference attenuation coefficients for these two regions (based on [PIC]:[BSi])? It could be worth plotting regression lines in figure 10b, for only diatom-dominated and only coccolithophore-dominated regions. The plot suggests that there would be no relationship between attenuation coefficient and %>51 μm POC for coccolithophore-dominated regions.

Page 2879, line 12-23: Could the differences in dissolution rates of PIC and BSi explain the differences in attenuation for the regions dominated by diatoms vs coccolithophores?

Page 2879, line 21: [PIC]: [Si], should be [BSi]?

Table 2: Is b calculated from Zpar to Zpar + 100, or deeper? Is it calculated from > 2 data points? Does the deep transfer efficiency (e.g. POC at ∼800m/POC at Zpar) add anything useful to the discussion? Does 'no fit' mean that the fit was not statistically significant? At what level?

Figures 1, 5, 6 and 8: Would it be possible to use more precise front positions e.g. Orsi et al 1995, Thorpe et al. 2002?

Figures 5-8: Is it area or diameter of circle which corresponds to flux? Difficult to see spatial trends from these figures.

Figure 2: I found it very hard to distinguish the different particle types in this photos – possible just the quality of my printout!

Figure 4: very long, lots of info, and not sure it's needed in the main text – could this be moved to supplementary?

Figure 9B and 10B: the regressions seem to be very much driven by station GB1-85 which appears to be an outlier. Did the authors test whether the relationships remain statistically significant if this data point was excluded?

Interactive comment on Biogeosciences Discuss., 12, 2843, 2015.