

Response to RC1: Elemental and isotopic constraints on zooplankton-mediated carbon fluxes in Ryder Bay, Western Antarctic Peninsula

Biogeosciences

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We thank the reviewer for their comments on our manuscript and address them below each point as follows.

This excellent manuscript suffers from a few issues that should be considered. It cites only publications from Antarctica where little is known from fecal pellets. From polar regions in the north a wealth of data on fecal pellets is available. They are ignored. Often it is claimed that we need polar, not Antarctic and Arctic science. The main goal of the manuscript is not reached because of the total exclusion of the rich Arctic and Sub-Arctic literature.

We thank the reviewer for raising this point. We would like to clarify that the manuscript does not claim to address polar carbon cycling as a whole, nor to speak on behalf of polar science more broadly; the stated aims and scope of this study are explicitly limited to the Ryder Bay coastal Antarctic system, and the discussion is framed accordingly throughout. Given the limited spatial and temporal extent of the dataset we present here, derived from a single mooring deployment over a four-week period, we consider it appropriate to maintain a discussion focused on the specific environment from which the samples were collected, rather than extrapolating to processes or systems beyond the scope of the data. We consider this regional specificity to be a strength of the manuscript rather than a limitation, in that it allows the mechanisms proposed to be evaluated directly against the conditions under which the samples were obtained, providing a basis for comparison with other understudied on-shelf coastal environments, both within and beyond the Antarctic.

We further note that a key finding of this study is the dominance of cylinder faecal pellets in summer particulate flux, with morphology and size characteristics consistent with production by Antarctic krill or large calanoid copepods, although the producing taxa were not directly identified in this study. The flux dynamics in this study are therefore closely tied to the ecology and seasonal behaviour of these specific taxa, which are largely restricted to the Southern Ocean and are not represented in Arctic zooplankton communities. This taxon-specific dependency limits to the extent to which direct comparison with Arctic faecal pellet studies, which are typically dominated by different zooplankton assemblages, can be expected to inform interpretation of the mechanisms driving flux in this system.

This is a research paper, not a review. The authors apply > 100 references. At times more than 10 references in the text. The reader understands that the authors know their literature, but I think that half of them would be more than sufficient.

We thank the reviewer for this comment and agree that the density of citations in places exceeds what is necessary to support the points being made. As a research paper reporting original observations, we recognise that the manuscript should not read as a literature review, and we will revise the text to reduce citation density throughout, retaining only the most relevant and directly supporting references at each point. Priority will be given to references that provide direct quantitative comparison to our results (e.g. reported ranges of values from independent studies) or that are the primary source for a specific claim, while more general background statements will be supported by a smaller number of representative references rather than an exhaustive list.

The problem of insufficient preservation of fecal pellets in the cups should be considered. Could it be that particulate N leaks out and give rise to high C/N ratios? They were in place for 2 years before recovery

We thank the reviewer for raising this important methodological consideration. We agree that the potential for preservation-related artefacts, including nitrogen leaching during the storage period prior to recovery and subsequent analysis, was not explicitly addressed and warrants further consideration.

We cannot exclude the possibility that preservation and storage resulted in some degree of preferential nitrogen loss. However, all samples in this study, across all sampling periods and all material types, were collected, preserved, and processed using the same protocol and preservative formulation. Any preservation-related artefact would therefore be expected to act systematically across all samples, rather than affecting comparisons among particle types or sampling periods. Furthermore, previous studies have shown that while preservation can influence absolute carbon and nitrogen contents, the effect on C:N ratios is generally small over the timescales considered here.

30. The process of coprorhexy and fecal pellet retention is an important one and should at least be mentioned. Also, the the data are from a post-bloom period with zooplankton playing a big role should be stressed.

We thank the reviewer for raising both of these points.

Regarding coprorhexy, we agree that this is a recognised process affecting faecal pellet retention and flux more broadly, and we will include a brief mention of it in the introduction alongside other biologically-mediated pathways affecting faecal pellet transport and degradation. However, we do not consider coprorhexy occurring within

the water column to be directly assessable using the sediment trap approach employed in this study, since our sampling captures material collected at a single depth rather than resolving changes in pellet integrity as material sinks through the water column. We have therefore not incorporated further discussion of this process into the discussion, as we are unable to directly evaluate its contribution to the material observed here.

Regarding the characterisation of the sampling period, we agree that the data strongly support a dominant role of zooplankton-mediated processes in driving the observed carbon flux, as already emphasised throughout the discussion. However, we do not consider the available data sufficient to characterise this period specifically as “post-bloom”. The POC fluxes observed here fall within the range reported in the literature for the summer season more broadly across the WAP (lines 250 – 253), rather than showing reduced flux magnitudes that would be expected following bloom decline. In the revised manuscript we will also add a plot showing the measured fluorescence from CTD casts at the timeseries station across the season, which shows that the bloom at this location has two peaks, one before the start of the sampling period, and one during the sampling period. Based on these factors, we prefer to retain a more general seasonal characterisation rather than specifying a post-bloom state that is not supported by the data.

185 Please revise all numbers. 92.49 +/- 75.17 is ridiculous! Often based upon two replicate aliquots. Statistics!

We thank the reviewer for this comment. Taking the specific example raised, the reported value ($92.49 \pm 75.17 \text{ mg C m}^{-2} \text{ d}^{-1}$) represents a mean and standard deviation calculated across four time periods that, as described elsewhere in the results, form two distinct groups (periods 2-3 are substantially elevated relative to periods 1 and 4). We agree that reporting this figure to two decimal places implies a level of precision inconsistent with the underlying variability, and more broadly that reporting a single mean and standard deviation across periods with strong temporal structure, derived from only two replicate aliquots per period, does not adequately convey this pattern or the associated uncertainty.

In light of this and related comments by the other reviewers regarding the statistical approach and sample replication, we will be revising our overall approach to statistical reporting throughout the manuscript. Rather than reporting formal significance tests (ANOVA) on data with only two replicates per group, we will revise the manuscript to report descriptive statistics only, describing sample means, standard deviations, and patterns of variation directly, with appropriate and consistent significant figures throughout. Further details of specific changes can be found in response to related comments from the other reviewers. We would also like to note that these changes will affect the reporting and presentation of the data only; the underlying values, patterns,

and their interpretations remain unchanged, and there will be no change to the overall findings or conclusions of the study.