

Response to reviewers

We thank the reviewers for their valuable comments which we believe have helped to improve the manuscript. We have incorporated as many of the reviewers comments as possible, please see detailed responses to each reviewer below.

To summarise the main differences to the text, we have incorporated chlorophyll calibrations and clarified the temperature and salinity used in our CTD profiles, improved figures as requested and moved some deployment information which was in the supplementary into the main text. We have also revised our figure 2 following discovery that one of the tracks needed to be updated. We have incorporated some statistics as requested (comparison between CTD parameters POC flux as well as determining differences in cylindrical FP volumes with depth). We were unable to incorporate both reviewers requests for supporting acoustic data to provide evidence for DVM, as such we have altered our discussion, suggesting our results reflect krill vertical movement rather than only a synchronous DVM type behaviour. Though we cannot provide supporting data on the vertical distribution of krill during the trap deployments we believe the patterns in our results still reflect krill defecation at depth is an important driver of our observations (this is discussed in more detail below and in the discussion section of the paper).

We will respond to each of the reviewers comments directly below, for readability our responses has been italicised to clearly sperate it from the reviewer comments. line numbers refer to the new revised text, rather than the original draft.

Reviewer 1

The study summarises the results of drifting sediment traps in February 2024 at different sites in the Weddell Sea. The study adds urgently needed data on particulate organic carbon and nitrogen flux in a remote, and therefore understudied, region and highlights the role of krill faecal pellets in the carbon export.

It's a good manuscript overall, but could do with a bit more data and statistics to support the statements.

- For example, you present microplankton sampling in the methods but do not come back to the data.

The microplankton data have been incorporated to support specific interpretations of some of the flux data (i.e. influence of sea ice, POC:PON ratios) but are not the focus of this paper, the microplankton are discussed in relation to a sea ice signal in lines 391- 402 to demonstrate that our 'short term' assessment of ice presence/absence does not necessarily reflect the biological and biogeochemical signal and help interpret some of the POC:PON ratios.

- The drivers of the different carbon export regimes are not explored in depth. Could you use any of the CTD data? Depth of the euphotic zone, nutrients, chlorophyll-a (measured, instead of calculated from the fluorescence), microplankton abundance...

Correlations between CTD data and the flux have been added to the supplementary and our results section (lines 293 – 299), however in general we would caution against overinterpretation of the CTD beyond supporting data. Both the chlorophyll max and chlorophyll max depth correlate with POC flux, however, in the case of particulate flux there is a temporal mismatch between primary production and collection in the trap. In addition, for 4/5 deployments the

traps are freely drifting (as much as 13km) and so are collecting in regions adjacent to but not precisely the same as the CTD data, (creating a spatial mismatch as well). Consequently, we would caution against over interpretation of the CTD and focus on the magnitude and contents of the flux itself. In quantifying and describing these fluxes we feel key drivers of the particulate flux (largely krill FP) are discussed in this study. Though descriptive we feel this is a powerful and accurate way to gain insight into flux drivers, following other flux studies such as Belcher et al., 2017; Cavan et al., 2015; Manno et al., 2015

- What did you do with the zooplankton swimmers? Were they added to the flux? Also, did you find any molts?

We have clarified that this is a poison free trap, in the methods (deployment and sampling section line 138) and that therefore no method to remove swimmers is necessary (line 168). We also added that no moults were observed in any samples analysed by microscopy (clarified in text on line 211).

Where are the other zooplankton, or better, faecal pellets produced by other taxa besides krill? Weren't they important enough to contribute in the same way to the faecal pellet flux? Why not?

The overwhelming majority of faecal pellets were krill (98% by volume). Oval and round pellets (from other zooplankton) only represented 1% of faecal pellet volume each. This has been clarified in text (lines, 307 & 407). The dominance of krill pellets is common in the Southern Ocean, where krill swarms often dominate zooplankton biomass. We have updated the discussion to compare our results to other (longer term) sediment trap studies in the region, which also demonstrate the importance of krill FP within the flux over other zooplankton types (lines 428-431).

- Any krill migration data you could access from the voyage, e.g., nets or acoustics, to support your DVM hypothesis?

*Unfortunately, similar to comparison with CTD data there is a temporal and spatial mismatch between any acoustic data and release of the traps. The traps did not drift within the ships path and the ship did not conduct a systematic approach to determine DVM depth of krill, or krill biomass, continuing other cruise operations once traps were released (i.e. including moving to different sampling locations, remaining on station, deploying numerous different types of equipment). In terms of net data, we conducted targeted fishing and stratified mammoth net hauls, both of which are not appropriate measures to determine krill DVM depth. As such we cannot provide acoustic or net data to support our DVM hypothesis. Consequently, we have removed all mention of DVM in the text and instead referred to 'vertical movement' this movement may be largely synchronous (e.g. like DVM) or indeed may not be and may reflect krill dispersal in the water column. Though we are unable to provide krill DVM data, our observations are in line with previous dvm assessments of *Euphausia superba* in the productive season (Bahlburg et al., 2023; Smith et al., 2025; Tarling et al., 2018) and as such we feel our hypothesis that the peak of cylindrical FP volume at 100 m likely reflects krill defecating at depth remains reasonable.*

In general, it would be good to know if you think the voyage happened during a bloom or post-bloom. The highest value of chl-a you found is 0.8 ugL-1 – is that a lot or rather typical for the region and time of year?

Our sampling took place towards the end of the productive season is discussed in the first paragraph of the discussion section. To clarify and add to this discussion chlorophyll data from other studies in the region have now been added for comparison to the following paragraph where our most productive station is discussed (lines 368-372).

The sediment trap deployments are rather short and therefore favour the collection of fast-sinking particles, which, of course, would be dominated by fast-sinking krill faecal pellets. This potential bias needs to be discussed.

We have added a small section to the discussion in regards to this topic (lines 414 – 419). When we compare our results to other sediment traps in the region (including those collecting over a longer time period), these studies see similar trends in the dominance of krill pellets in the flux. As such, although our deployment times are shorter, they are still representative of the region and reflect the influence major grazers like krill can have on the POC flux.

It would be great if you could add a data availability section.

The data availability section is present on lines 518-523 - maybe this was removed for open review but it is present along with DOIs for data (?)

Points by line:

Unless otherwise stated below, all points by line were incorporated into the text, we thank the reviewer for their helpful observations. Where appropriate/required we have specifically elaborated on the changes made

20: Passively sinking of carbon-rich particles? Particulate processes not clear

These processes are not just strictly passive as our results outline (with the added boost from migrating krill), have amended to 'Here, the biological carbon pump is dominated by carbon-rich particulates which are both actively and passively transported (e.g. zooplankton faecal pellets and phytoplankton detritus).'

96: downward fluxes?

Prefer use of 'vertical carbon flux'

111: how far did the other deployed sediment traps drift?

Added table with drifting distances to the main text (Table 1)

117: Any preservative in the sediment trap?

No preservative used, methods have been updated to clarify.

132-133: Check if only CTD profile data was analysed in R.

CTD and flux data analysed in R but not all data analysed in R (with image and satellite data processed differently, have updated to reflect this).

136: Any zooplankton swimmers that were picked out beforehand? –

No picking required, swimmers were not present (traps were not poisoned). Methods have been updated to clarify.

178: This is the first time you mention microplankton. Would be useful to have it mentioned in the introduction and in the aims of the papers. Also, add a definition of microplankton. What happened to the microplankton in the trap?

We have added a definition of microplankton in the methods. Unfortunately, we do not have resources to analyse the microplankton present within the trap, and the overwhelming majority of particles were faecal pellets in the main. The microplankton are supporting data and are not the focus of the paper and as such were not mentioned in the introduction and aims, in order to frame this study and keep the focus on magnitude of the flux and strong influence on FP.

In general, did you conduct any statistics on potential drivers? This study is rather descriptive. Any of the CTD parameters that could be used to explain the difference in fluxes.

We have added some statistics but please see our comment in regards to your second bullet point above.

203: Maybe more oceanographic?

Kept geographic

215: Where do you describe the chlorophyll-a measurements? Add a clearly marked section to the methods.

We have updated our chlorophyll values and added a methods section to indicate the chlorophyll calibration

Figure 6A: I can't really see any orange in that graph. There were no round faecal pellets? POC flux: x-axis looks crowded. Reduce labels.

We have added a supplementary figure (4) in which the contribution of different sized fp is more obvious (though still minimal) and updated the figure in the main text so that all FP are represented in green to minimise confusion, we have explicitly stated in the results the small contribution of round and oval pellets.

Figure 7: Reduce tick marks on the x-axis. Could you merge with Figure 6A?

We have reduced tick marks as requested but prefer to keep these data as separate figures owing to the more speculative nature of the presence of eggs as part of the flux.

302-304: do you have any information on nutrient concentrations from water sampling with the CTD on this voyage, that could support this statement?

For the purposes of this paper, we do not have any (micro)/nutrient data, however the increased concentration of (micro)/nutrients on the Weddell Sea shelf is well documented in the studies referenced.

354-357: temporal mismatches should be relatively small due to the shallow depths and also the short deployment times. However, this could be a good spot to mention that you might not catch all sinking particles, only faster sinking ones.

We have added a section discussing our shorter deployment times (which may bias towards faster sinking particles) (lines 415-419).

360-361: What times of day were the traps deployed? Does it make sense to propose DVM (noting that there could be inverse migration too). I think deployment dates, duration and times could be put in a table in the methods.

Have added deployment times in a table (table 1) into the main text along with the percent of time the deployment took place in daylight. Times are also available on figure 2. We have also softened our hypothesis from 'dvm' to vertical movement, which can account for inverse migration and dispersal throughout the water column rather than solely dvm.

366-368: defecate at depth, or faecal pellets just sink out very fast?

We expanded on our previous draft, discussing that the patterns we see may also reflect fast sinking rates and little fragmentation (lines 452-462), and that rapid sinking of fresh material likely is also taking place at S1 and S5.

373: Any information on nets or acoustics from the voyage that would support the hypothesis of migrating krill?

Please see above in response to your bullet point five.

REVIEWER 2

The manuscript by Atherden et al presents particulate organic carbon (and nitrogen) fluxes from a custom-built drifting sediment trap sampling three depths at five different locations in the Weddell Sea. The paper focuses on the potential export of carbon from Antarctic krill faecal pellets but also most interestingly krill eggs, and explores the influence of hydrography and sea ice cover on fluxes. The paper was a pleasure to read and was clearly performed to a high methodological standard. While these may not be the most globally significant findings, assessing the spatial variability of carbon flux estimates in Antarctic coastal environments and links to surrounding environmental parameters is much needed in krill biogeochemical research.

I have a number of minor comments, which after incorporating I feel the manuscript will be to a publishable standard.

1. It would be great to see some complementary data to support the diel vertical migration assertions. I'm unsure what other data streams were collected onboard but if available, this could come from ship-based or moored echosounder backscatter/ADCP amplitude in the area, or even net hauls from stratified depths? I recommend the authors incorporate this into their analysis to strengthen their conclusions.

Unfortunately, our stratified net hauls are not suitable for assessing krill DVM (as we a mammoth net with a 100 um mesh size) and krill were caught during the cruise using target fishing. There is a temporal and spatial mismatch between any acoustic data and release of the traps as the shift did not follow the traps at all but rather carried on with other cruise activities, and no systematic acoustic survey was conducted as the ship continued with other cruise activity once the traps were released. Though we cannot as yet provide krill dvm data from acoustic measurements on this cruise, our observations are in line with previous dvm assessments of Eupahsia superba in the productive season (Bahlburg et al., 2023; Smith et al., 2025; Tarling et al., 2018). However, to reflect that our results maybe the results of non-dvm vertical movement of krill (i.e. dispersal or less synchronised movement) we have removed all mention of dvm in our hypothesis. In place we refer to krill vertical movements or migration and acknowledge that krill is a highly motile

species (Lines 442-448). However, we still feel the hypothesis that our results likely reflect a significant component of krill biomass defecating at depth is reasonable.

2. I would encourage more discussion around the krill egg component as this is a novel aspect to your paper. Were you able to stage the embryos? Were there any trends in stage with depth? Further, particularly if ADCP data is available for current direction and velocity, it would be great to see some estimates into source regions or release locations for both krill eggs but also faecal pellets.

We agree that the assessment of the export flux of sinking krill eggs is a novel part of this paper, and thank the reviewer for noting this. However, the eggs were not staged, and their potential insights on krill life cycle was felt to be out of scope for this biogeochemistry focused study. Capturing Krill eggs in the trap was opportunistic (and surprising), but other members of the team are looking into the krill egg presence from the perspective of krill ecology (wherein they have more appropriate data from stratified net sampling for an ecological/behavioural focused analysis).

3. I recognise the drifting sediment trap apparatus will be presented in another paper under review/submitted(?), but I would ask the authors to comment on the efficiency of collecting particles with a surface tethered sediment trap. Previous studies have shown surface tethered traps may introduce unwanted microturbulence inside the trap which may alter the particles intercepted. I may be wrong but feel this is especially likely for the deployment tethered to the ship? Perhaps some comment around the efficiency of this in comparison to alternative traps (i.e. neutrally buoyant sediment traps) would be appreciated. Suggested ref: Baker, C.A., Estapa, M.L., Iversen, M., Lampitt, R., Buesseler, K., 2020. Are all sediment traps created equal? An intercomparison study of carbon export methodologies at the PAP-SO site. Progress in Oceanography 184, 102317.

We acknowledge that having to tether the trap at S1 does create methodological differences with stations that are free drifting. Consequently, we have added a section discussing the possible influence(s) of tethering at S1 (lines 356-367). As the fresh material present in the trap well represented the productivity present at station S1 we felt that it should still be kept in the study, despite the methodological differences and difficulties accounting for the impact of tethering.

Points by line:

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L96: I think you are mostly focussed on krill-specific fluxes, rather than all components of biogeochemical fluxes here.

Changed to specify POC and PON.

L107: I would also be interested to know the duration of each deployment as I think this will colour your interpretations. For example the shortest deployment of 11h is less than the average time for a full vertical migration cycle (i.e. krill would have only ascended or descended) so it would only capture half of this process.

Added removed some information from the supplementary and added a main text table (Table 1) of deployment times, % under daylight conditions and drift distances.

L143: Just flagging the reliability of standard deviations calculated with fewer than three replicates

Though we acknowledge difficulties with only two replicates, standard deviation is more appropriate than other measures such as standard error and is proportional to the range of samples.

L145: Do you mean a laminar flow hood? Else particles would be introduced to your samples

We didn't have access to laminar flow hood but have blank corrected to our samples to account for contamination, the fume hood was not turned on so there was not an active airflow.

L172: Make sure you include a point in the discussion about the variability of carbon concentrations in krill faecal pellets (i.e., from different diets/seasons/life stages etc).

This is included in the discussion (lines 424-429)

Figure 2: Just a suggestion, but you could colour the track line for each deployment by day night hours so we can see the deployment period more clearly. Please also note if this is UTC time?

Added UTC time for figure legend and a main text table detailing deployment times, drift distances and the % of the deployment which took place between apparent sunrise and sunset.

L262-264: did you run any statistical analysis to determine if FP volume was genuinely different with depth and between stations?

Have conducted a Kruskal-Wallis test to determine differences in individual cylindrical FP volume across our depths. Added to the results of this test to methods, results discussion and supplementary.

L265-268: This is really interesting as it's commonly thought that krill move off of the shelf to deeper waters to spawn. Were eggs not present at the other deployment sites (other than S1 and S3)? If so, please state that.

Clarified in the results section that eggs were only present at S1 and S3 (line 318).

L324-327: There's a lot of overlap between 3.2 – 13.2 and 6.2 – 12.6. Is this actually very different?

We have clarified that the comparison with ice algae (e.g. 'elevated' c:n) was in relation to pelagic phytoplankton and that our POC:PON values may be higher than previously observed krill pellets due to additional presence of ice algae at multiple sites. i.e. the presence of ice algae elevates the POC:PON of particulate material (including pellets) even further (lines 396 – 407).

L353: explain more about how zooplankton interact with particles at depth to reduce attenuation? Might you expect more attenuation if you had sampled deeper than 150m?

We have clarified 'interaction' by changing to grazing/fragmenting. Past 150 m it is my personal opinion that attenuation is likely to be low, the shelf was shallow and the flux was dominated by fast sinking krill fp. I have included (educated) yet speculative calculations on the proportion of carbon reaching the sea floor have been included in lines 465-482.

L359-366: This is where the addition of trends from acoustic backscatter (swarm depth, density, thickness) would really help your interpretations.

Please see our response to comment 1 above.

References

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