We thank the reviewer for their constructive comments. We appreciate their acknowledgements of the importance of the key takeaways from the manuscript, namely the importance of the initial particle size distribution slope in POC flux attenuation and the need for the inclusion of zooplankton-induced fragmentation into BCP models. We acknowledge their suggestion to further explain the potential regional variability of fragmentation given the GLOBESINK size profile dataset. To the best of our ability, we have addressed this main request, some of the reviewers comments refer to aspects of work that are currently undergoing (and will be released in the near future) and do not fit the remit of this manuscript. However, we thank the reviewer for these particular questions as they act as justification for our current work.

We have addressed the reviewer's comments in the pages below. Reviewer comments are shown in bold text and our responses in standard text, with actioned responses in green.

The major changes are as follows:

- Figure resolution and structure have been amended where suggested
- Further discussion of the potential regional variability of biologically-mediated fragmentation has been added throughout the manuscript where suggested

The minor points have also been addressed, including reference suggestions and suggested elaboration of certain arguments.

We believe these additions will improve the manuscript's clarity and hope the editor is in agreement.

Jordan Toullec, 19 Aug 2026

Referee/Response/Actioned Response

General comment

The manuscript by Naidoo-Bagwell et al. proposes a mechanistic 1-D model to explore the role of biologically-mediated fragmentation in carbon flux attenuation. The authors performed simulations with varying particle properties. Different PSD slope experiments (S = 0, 2, 3, 4) were conducted (following Clement et al., 2022), along with seasonal experiments.

The introduction is well-written and well-referenced, providing readers with access to the fundamental knowledge about the BCP to evaluate the pertinence of zooplankton fragmentation in ocean transfer efficiency.

The idea that the initial particle size distribution slope is a critical factor in POC flux attenuation is very interesting, as it controls the relative abundance of large, fast-sinking particles capable of transporting carbon deeper into the water column. This aspect deserves to be highlighted within our current understanding of the BCP. Zooplankton-induced fragmentation and its effects on size profiles and flux attenuation need to be incorporated into BCP models to better represent POC flux attenuation with depth.

However, I have a few (moderate) concerns about the manuscript in its current state.

One of the main shortcomings of the manuscript is that, given Fig. 1 (bioregions), I was expecting the authors to establish a comparison between bioregions. Which bioregions are the most impacted by zooplankton-mediated fragmentation?

I think the quality of the figures can also be improved (size, resolution, organization, etc.).

The discussion is short (which is appreciated), yet the interpretation of the model outputs is not sufficiently developed. The discussion is somewhat frustrating, as it reads more like a list of model limitations and gaps, which is not particularly compelling. Several aspects (detailed below) can be improved and developed.

Based on these points, I recommend a moderate revision of the manuscript before it can be considered for publication in Biogeosciences.

Abstract – we do not make any changes to the abstract, see below responses.

Lines 15-17: Would it also be interesting to mention the re-aggregation processes that can occur in mesopelagic layers, as well as the opposing trend (i.e., increasing POC flux)? This is particularly relevant regarding fecal pellet production and repacking, which can significantly increase POC flux.

We thank the reviewer for this point. Whilst these processes occur within the mesopelagic, they are not included within the framework of our modelling study, as drivers such as turbulence become negligible with depth (Takeuchi et al., 2019). Our study is primarily focusing on the absence of fragmentation within BCP models. Further work, related to the impact of a separate fecal pellet particle type is currently being conducted within our model.

Line 20: Why only copepods and not all zooplankton taxa?

Whilst many zooplankton can fragment particles, the relatively high abundance of particle-associated copepods and the nature of their fragmentation, we assign them as the principal agents of fragmentation, as per Mayor et al. (2020). Additional taxa could be added to these experiments, although this would require more computational power and distinct separation of how these taxa differ regarding ingestion and grazing rates.

Line 29-30: Does it consider a conversion between marine snow POC mass (grazed and ingested) and fecal pellet egestion? (e.g. a fraction of marine snow POC into fecal pellet POC)?

Fecal pellets are currently not included within this model, given the small size of particle-associated copepods we assume their fecal pellets provide a negligible impact on POC flux (Poulsen and Kiørboe, 2006). Further work, related to the impact of a separate fecal pellet particle type is currently being conducted within our model with a manuscript in preparation.

Introduction

Lines 48-49: The way the sentence is written suggests that particle mass and morphology are consequences of acceleration or deceleration, whereas the opposite is true (or mass and morphology dictate particle dynamics). Alternatively, is the relationship bidirectional?

We thank the reviewer for pointing out this lack of clarity. The sentence has been amended to clarify that the opposite is true.

Line 50: More recent publication suggest that the ballasting effect accelerate the velocities (e.g. Laurenceau-Cornec et al., 2020).

We thank the reviewer for this citation recommendation. This citation has been added.

Lines 76–78: Fragmentation cannot be observed directly, but can zooplankton-associated particles within marine snow be assessed using UVP6 images? Are there any references for this?

PAC behaviour is primarily observed within lab studies, we are not aware of datasets that explicitly characterise the amount of PAC-particles, rather their abundances and biomasses in situ. UVP images can also delineate particle type, but not if a copepod is attached. The relevant citation (Kiørboe, 2000) and text have been added.

Lines 87-88: Does PAC also consider grazing and POC conversion into fecal pellet?

See response to Line 29-30 in the Abstract section.

Methodology

Line 101: a "Fluffy" aggregate should be shortly defined here.

Defined, referring to porosity and morphology.

Line 109: Is 0.01 d-1 based on literature? Is it supposed to change with temperature and depth?

Yes, 0.01 d-1 is a value taken from the literature. While these rates can change with temperature and depth, we apply a constant rate in this study to directly assess the impact on change in size and flux when separate particle transformations are enabled in the model. A citation has been added (Bach et al., 2019).

Line 123: Why the fraction of ingested POC (ψi) prescribed at 0.3? Can you add maybe a reference about it?

Further information on this is provided in the supplementary material as mentioned, but this value is derived from Mayor et al. (2020). This reference is added and reiterated in the revised manuscript.

Lines 129: Can you introduce what is a Holling III functional response (what feeding trait does it correspond?) + reference?

Added. Please see response to Eq 3 comment by the other reviewer.

Line 151: What "f" prefix standing for?

This denotes seasonally changing particle flux where the total initial particle flux magnitude is varied. This is defined in the corresponding text as well as the table caption.

Table 1: The column Reminerealisation and Fragmentation (tick and exclusion) are not necessary in this table (the first column is explicit).

These columns have been removed.

Results

Fig. 2: The figure resolution is very low. It should be improved for the final submission. The x-axis label (Average Size (µm)) should be visible on the subplot (F) (or on each subplot). The x-axis of subplot (b) and (e) are cropped. Have you also considered showing the aggregates and zooplankton abundance profile? In the same format as Fig. 2.

We thank the reviewer for addressing this issue. Figure resolution and suggested annotations have been updated accordingly. We do not have data from GLOBESINK for aggregates and zooplankton as outlined in the Methodology. Within the model, we apply a PAC biomass relationship to POC, thus PAC biomass profiles would follow the same trend/profile as flux profiles. We have included a PAC biomass profile for the 3.2 FRAG+REMIN experiment in the supplementary material and referenced to it in the main text (see below reviewer comment).

Lines 265-270: I think it could be relevant to display profiles of vertical PAC distribution (particles abundance and zooplankton abundance input in the model) at this step to figure it out.

See previous response regarding PAC relationship to POC flux.

Line 285: "as you get deeper" sounds very unformal, prefer "as depth increases"

Changed.

Fig. 5. Using two subplots might be more appropriate than a dual y-axis in this case. To improve the readability of this graph, I suggest indicating the POC flux loss due to fragmentation directly within the integrated POC flux loss bar (e.g., using a stacked bar plot with two colors).

This has been separated into two subplots with the stacked bar format applied that the reviewer has suggested.

Does POC flux loss due to fragmentation (%) is still relative to NONE? Does 100% loss (red bar) due to fragmentation mean that all the loss is due to fragmentation? which is expected in FRAG?

Yes that is correct. We have amended the text description to reflect this.

Fig. 5. Is a bit confusing.

See earlier response, reviewer suggestions have been applied for clarification.

Lines 318-319: "Fragmentation is the dominant attenuator of flux in the upper mesopelagic, where particle-associated copepod (PAC) and other zooplankton biomass is greatest", See my previous comment about the addition of zooplankton abundance vertical profile.

See previous response regarding PAC relationship to POC flux.

Fig. 8. I recommend changing the color scale gradient by a cyclic color gradient (e.g. twilight, hsv…) it would considerably highlight the winter vs summer months influences. Fragmentation (dashed lines) and remineralization (solid lines) could be also presented in the subfigure (d). I recommend splitting this graph into 2 separated graphs (fragmentation and remineralization).

Figure size ratio and orientation have been improved for better readability. Figure size has been altered as suggested by the author. The remineralisation lines within subplot (d) have been removed for readability, as their value is implied (remainder not caused by fragmentation). We thank the reviewer for their suggestion of changing the colour scale, as this experiment is purely exhibiting increasing or decreasing particle flux, we feel using a red-blue scale (increasing vs decreasing) is sufficient.

Discussion

Lines 426-426: Why don't present the regional variability of biologically mediated fragmentation in attenuating POC flux?

Whilst we cannot directly apply these results to the GLOBESINK dataset, we thank the reviewer for suggesting that assumptions about potential regional variations in fragmentation's role by reviewing the size profiles. References to figure 1 and the potential role of biologically mediated fragmentation has been added to the text. Another manuscript is currently being written where model output is directly applied to the GLOBESINK dataset, allowing comparison in behaviours between the different biomes. This work is to be released in the near future but is not within the remit of this manuscript, we thank the reviewer for their interest.

Lines 445-446: "although no consistent net effect on fragmentation was found, » In Toullec et al. (2019), aggregates were fragmented after 48 hours in the *Temora longicornis* (cruise feeder) incubation compared to the control, where neither fragmentation nor re-aggregation was observed.

This sentence refers to the number/size of aggregates produced due to fragmentation, which informed our decision to model it as a logarithmic vs random process. This clarification is now included in the text.

Lines 449-451: If you are referring to the fecal pellet emission and carbon flux boost (higher sinking rate and lower remineralization rate of fecal pellet), I think the idea can be better

developed in the discussion, regarding the attenuation effect of PAC-meditated fragmentation.

We agree with the author, model absences have been included towards the end of the discussion, but this section can further cover the role of fecal pellets. We assume PAC fecal pellets to be too small, thus have a negligible role, also larger fecal pellets from other zooplankton are not currently included in our model. This absence is currently being addressed in model development, with results from this alteration likely to be released soon, however this development is not under the remit of this manuscript. We have added text to address and develop the idea relating to fecal pellet emission and carbon flux boost as the author suggests.

Lines 465-467: Does this aspect vary with latitude (or bioregions)?

Please see response to first discussion comment (Lines 426-426). We have added discussion of potential latitudinal and regional variation.

Lines 472-475: Biogenic ballast (opal and calcite/aragonite) are expected to vary regionally and seasonally. This should considerably enhance the sinking rate and remineralization (both seasonally and regionally). This aspect could be also developed in the discussion regarding the model input.

We have added a reference referring to the potential variation of the ballasting effect (Cram et al., 2018).

Concluding remarks

Line 496: Once again, you have the possibility to at least present the regional variability.

Please see response to first discussion comment (Lines 426-426). We have added a sentence to address the potential of exploring the regional variability of fragmentation.

References

Bach, L. T., Stange, P., Taucher, J., Achterberg, E. P., Algueró-Muñiz, M., Horn, H., Esposito, M., and Riebesell, U.: The Influence of Plankton Community Structure on Sinking Velocity and Remineralization Rate of Marine Aggregates, Global Biogeochemical Cycles, 33, 971-994, https://doi.org/10.1029/2019GB006256, 2019.
Cram, J. A., Weber, T., Leung, S. W., McDonnell, A. M. P., Liang, J. H., and Deutsch, C.: The Role of Particle Size, Ballast, Temperature, and Oxygen in the Sinking Flux to the Deep Sea, Global Biogeochemical Cycles, 32, 858-876, https://doi.org/10.1029/2017gb005710, 2018.
KiØrboe, T.: Colonization of marine snow aggregates by invertebrate zooplankton: Abundance, scaling, and possible role, Limnology and Oceanography, 45, 479-484, https://doi.org/10.4319/lo.2000.45.2.0479, 2000.
Mayor, D. J., Gentleman, W. C., and Anderson, T. R.: Ocean carbon sequestration: Particle fragmentation by copepods as a significant unrecognised factor?, BioEssays, 42, 2000149, 10.1002/bies.202000149, 2020.
Poulsen, L. K. and Kiørboe, T.: Vertical flux and degradation rates of copepod fecal pellets in a zooplankton community dominated by small copepods, Marine Ecology Progress Series, 323, 195-204, 10.3354/meps323195, 2006.
Takeuchi, M., Doubell, M. J., Jackson, G. A., Yukawa, M., Sagara, Y., and Yamazaki, H.: Turbulence mediates marine aggregate formation and destruction in the upper ocean, Scientific Reports, 9, 16280, 10.1038/s41598-019-52470-5, 2019.