

We would like to thank Reviewer 1 for their thoughtful comments and suggestions and are pleased that the reviewer finds the manuscript worthy of publication following minor revisions. In the following, we have used [blue text](#) to highlight our responses to the reviewers' comments.

## **REVIEWER 1**

Review Roca-Martí et al. Sinking particle fluxes and biological carbon pump efficiency in the Labrador Sea during a *Phaeocystis* bloom decline

The paper describes and discusses the fate of a *Phaeocystis* bloom in the Labrador Sea in spring 2022, and impact on the biological carbon pump. The authors have a nice data set including bloom, late bloom and outlayer stations, and they have done a thorough job in methodological testing and discussing of premises on steady state for the models used, use of particles > 51 µm for the carbon estimates and stoichiometry. They also utilize a broader dataset to provide context to the observations, like remote sensing based primary production in the period before and during the field sampling to evaluate the efficiency of the biological carbon pump.

I like the paper and work, I find the approach and methods useful and well described, and it fits well with the scope of BG. The dataset and concept of comparative testing of export within and outside a *Phaeocystis* bloom region is novel – including impressing many profiles for Th-based carbon export, and the data are elegantly used in combination with net primary production and quantification of both carbon and BSi to address the potential impact of diatom-associated export during this *Phaeocystis* dominated bloom. The title is appropriate, the abstract reflects the content well, and the references are appropriate.

There are some structural issues on placement of figures and a chapter on earlier studies in the region that could be improved. A few places the methodology needs some improved clarification, but in general is this a well written paper, with clear ideas and methods. The assumptions are valid and clearly outlined, and the conclusions are substantial and sound and based on the results. See specific comments for more details.

[Response: Following the reviewer's suggestion, we will revise the placement of three figures and the "Comparison with other studies" section. The details are explained below. We will also provide further clarifications regarding the methodology.](#)

This is obviously one of several papers from the expedition, and there are several references to publications in prep. These are hard to access, but may be in a more progressed state at the time of publication. A few places some more info could perhaps be provided. The figures are informative and well described in the Figure caption, and Tables provide valuable information on relevant numbers. A table including sampled depths could be useful (in Suppl.). The supplementary material is useful and works well as Suppl.

[Response: Regarding the references to publications currently in preparation, we expect that they will be at a more advanced stage by the time of publication. We cite three different papers in preparation, mainly \*Bertrand et al., in prep.\*, which will provide context for the BELAS-1 expedition and is expected to be submitted to \*Biogeosciences\* in the coming weeks. We will ensure that the references to \*Bertrand et al., in prep.\*, are updated to cite the corresponding EGU sphere preprint once available. In case of a delay in the preprint's publication, we will either remove the "\*in prep.\*" references while providing additional relevant information when](#)

needed, or replace them with references to conference presentations or personal communications, as appropriate.

**More specific comments:**

Figures (2,) 3, 4 and 5 are placed in the Material and methods section, but are just briefly referred to here, while they are described and discussed more in detail in the results chapter. Suggest to move them there to ease the reading of the results sections (e.g Fig. 3 in 3.1, Fig. 4 in 3.2).

Response: Thank you very much for this suggestion. In the revised manuscript, we will place these figures in the Results section as follows:

Fig. 3: Section 3.1

Fig. 4: Section 3.2

Fig. 5: Section 3.3

We would like to keep Fig. 2 together with Fig. 1 in the Methods section, as it provides contextual information on the chl-a concentrations during the sampling period for each region.

L116: Water was collected from six depths – which ones? A table in supplementary will do.

Response: In the revised manuscript, the following table will be added to the supplementary materials:

| <b>Station</b> | <b>Collection Depths (m)</b> |
|----------------|------------------------------|
| 4              | 5, 28, 40, 75, 95, 140       |
| 9              | 5, 35, 50, 80, 100, 161      |
| 11             | 5, 15, 33, 55, 77, 100       |
| 24             | 5, 40                        |
| 29             | 5, 40                        |
| 13             | 5, 20, 40, 70, 85, 100       |
| 7              | 5, 15, 25, 40, 50, 60        |

L119-120: type 1 water – please explain

Response: Type 1 water is ultrapure laboratory water that has very low concentrations of organic carbon (~ 2-10 ppb) and dissolved ions (resistivity  $\geq 18.2 \text{ M}\Omega\text{-cm}$ ). We will clarify this point in the revised paper.

Eq.1 and L142-143: perhaps my limited familiarity with these calculations, but unclear how  $\delta^{13}\text{C}_{\text{ex}}$  refers to excess  $^{13}\text{C}$  atoms in either PC or DIC, when both PC and DIC are part of the equation

Response: In the revised text, we will add more details pertaining to the calculation of the absolute uptake rate. Note, we also no longer use chlorophyll-a in these calculations, which addresses the following point.

The revised manuscript will read:

*“Absolute carbon uptake rates were calculated following Hama et al. (1983) using the following equation:*

$$\rho = \frac{PC \text{ at}\%xs}{DIC \text{ at}\%xs \times Time} \times [PC], \quad (1)$$

*where  $\rho$  is the absolute uptake rate. For particulate carbon (PC), atom% excess (at%xs) refers to the percentage of  $^{13}\text{C}$  enrichment at the end of the incubation minus the percentage of  $^{13}\text{C}$  at the beginning of the incubation. For dissolved inorganic carbon (DIC), atom% excess refers to the proportional change in  $^{13}\text{C}$  available in the water when the  $\text{H}^{13}\text{CO}_3^-$  label is added. Ambient  $\text{HCO}_3^-$  concentrations were estimated based on the salinity of each sample (Parsons et al., 1984). Absolute uptake rate represents how much  $^{13}\text{C}$  was taken up by the target community over the incubation period and is presented as units of carbon per volume or area per time (in our case,  $\text{mmol C m}^{-3} \text{ d}^{-1}$ ).”*

L151-163: the use of Chl a conc to calculate NPP is a bit confusing. Was this only for the central stations where NPP measurements were missing? Please clarify.

Response: Chlorophyll-a concentration was initially used to calculate NPP for all stations because of interest in extrapolating NPP in the Central Grid stations; however, extrapolating NPP in the Central Grid stations was not possible due to high variability in the NPP-to-chlorophyll-a ratio across the Eastern Grid. In the submitted version of the paper, we continued to use the chlorophyll-a concentration to calculate NPP in the Eastern Grid stations, despite moving away from attempts to calculate NPP in the Central Grid stations. However, considering the reviewer's comment, we now think that using chlorophyll-a concentration to calculate NPP unnecessarily overcomplicates the calculations. In the revised manuscript, we will therefore use a trapezoidal integration without chlorophyll-a to calculate NPP. There is an average difference of 4.5% between the trapezoidal integration without using chlorophyll-a and the previous method that uses chlorophyll-a. Although slight changes have to be made to reflect the new numbers in Tables 1 and 2, this small difference (4.5%) does not impact the conclusions of the manuscript.

L156: Fluorometer profiles from the CTD – should this be fluorescence profiles?

Response: The reviewer is correct, we meant fluorescence, but this sentence will be removed from the revised manuscript since we will no longer use chlorophyll-a concentration to calculate NPP (see above).

L157-158: Since the Bertrand et al. paper is still in prep, it is hard to get the background for the non-corrected photochemical quenching related to a sub-chl max

Response: Any reference to non-corrected photochemical quenching will be removed from the revised manuscript. Since we will no longer use chlorophyll-a concentration to calculate NPP, (see above), we will remove this sentence from the revised manuscript.

L233: Similarly, is parts of method used for sampling marine snow catching deployments and processing referring to a Cisternas-Nova et al. in prep. Please provide the most important information in this paper.

Response: We will include the key methodological details regarding the marine snow catchers, ensuring that the manuscript contains all necessary information independently of *Cisternas-*

*Novoa et al., in prep.* Accordingly, we will remove the citation to the in-prep manuscript and add the following details:

*"MSCs are 1.5-m-tall, 100-L water samplers equipped with a removable base section designed to separate particles according to their sinking velocity under low-turbulence conditions (Giering et al., 2016; Riley et al., 2012)"*

*"Following MSC retrieval, particles were allowed to settle for 2 h, after which they were classified into three operationally defined fractions: suspended, slow-sinking, and fast-sinking (Romanelli et al., 2023, 2026). Suspended particles, with negligible sinking velocity, were collected from a central tap ~79 cm from the MSC top. Slow-sinking particles, with velocities <18 m d<sup>-1</sup>, were siphoned from the base. Fast-sinking particles, with velocities >18 m d<sup>-1</sup>, were trapped in the tray at the bottom of the base section."*

Giering, S. L. C., Sanders, R., Martin, A. P., Lindemann, C., Möller, K. O., Daniels, C. J., Mayor, D. J., and St. John, M. A.: High export via small particles before the onset of the North Atlantic spring bloom, *J. Geophys. Res. Ocean.*, 121, 6929–6945, <https://doi.org/10.1002/2016JC012048>, 2016.

Riley, J. S., Sanders, R., Marsay, C., Le Moigne, F. A. C., Achterberg, E. P., and Poulton, A. J.: The relative contribution of fast and slow sinking particles to ocean carbon export, *Global Biogeochem. Cycles*, 26, <https://doi.org/10.1029/2011GB004085>, 2012.

Romanelli, E., Stevens-Green, R., Cisternas-Novoa, C., LaRoche, J., Siegel, D. A., Carlson, C. A., and Passow, U.: Particle lability drives degradation dynamics and bacterial community assembly during a *Phaeocystis* bloom decline, <https://doi.org/10.64898/2026.04.19.716305>, 2026.

L375: Section 3.5 on the Comparison with other studies reads more like discussion than results to me. Suggest to move this to the Discussion section.

Response: We agree with the reviewer's suggestion. We will integrate the "*Comparison with other studies*" subsection into the first part of the Discussion (Section 4.1), which will be renamed "*4.1. POC fluxes in the central Labrador Sea during the decline of a Phaeocystis bloom and comparison with other studies*".

L418: 1 week – should be one week (spell out numbers <10)

Response: Thank you for the observation. We have decided to leave it as is according to the following Biogeosciences submission guidelines: "*For items other than units of time or measure, use words for cardinal numbers less than 10; use numerals for 10 and above (e.g. three flasks, seven trees, 6 m, 9 d, 10 desks)*". Since "week" is a unit of time, we understand that numerals should be used in this case. However, we are happy to change this at the copy-editing stage as requested.

L419: the Bertrand et al. in prep – can evt be a pers comm.?

Response: If available, we will replace the *Bertrand et al., in prep.* citation with the corresponding EGU sphere preprint citation prior to the publication of this manuscript

(otherwise we will delete the in-prep reference). In any case, we will include the following references: a conference presentation (Stevens-Green et al., 2024) and a preprint (Romanelli et al., 2026).

Stevens-Green, R., Romanelli, E., Cisternas-Novoa, C., Pollara, S., Jabre, L., Passow, U., Bertrand, E. M., LaRoche, J., Stevens-Green, R., Romanelli, E., Cisternas-Novoa, C., Pollara, S., Jabre, L., Passow, U., Bertrand, E. M., and LaRoche, J.: Exploring the Microbial Communities Involved in the Biological Carbon Pump Throughout the Decline of a Large *Phaeocystis pouchetii* Bloom in the Labrador Sea, in: Ocean Sciences Meeting (OSM) 2024, 2024.

Romanelli, E., Stevens-Green, R., Cisternas-Novoa, C., LaRoche, J., Siegel, D. A., Carlson, C. A., and Passow, U.: Particle lability drives degradation dynamics and bacterial community assembly during a *Phaeocystis* bloom decline, <https://doi.org/10.64898/2026.04.19.716305>, 2026.

L533-536: It is a valid and important point, but not new knowledge that NPP and export is separated in time by weeks – requiring a larger time scale to evaluate export efficiency. This is well done in the present paper, but I suggest acknowledging one or two of the earlier studies on this like Smetacek et al. 1984, and Kiørboe et al. 1996.

Smetacek V, Bodungen B, Knoppers B, Peinert R, Pollehne F, Stegmann P, Zeitzschel B. 1984. Seasonal stages characterizing the annual cycle of an inshore pelagic system. *Rapp PV Reun Cons Int Explor Mer* 183:126-135.

Kiørboe T, Hansen JLS, Alldredge AL, Jackson GA, Passow U, Dam HG, Drapeau DT, Waite A, Garcia CM. 1996. Sedimentation of phytoplankton during a diatom bloom: Rates and mechanisms. *Journal of Marine Research* 54:1123-1148.

Response: Thank you for pointing this out. Both references will be included, in addition to Henson et al. (2015), in the revised manuscript to acknowledge these earlier studies providing evidence of the time lag between NPP and export.