egusphere-2025-3671. Roca-Martí et al. Manuscript

This work provides high resolution measurements of sinking export fluxes (particulate organic carbon, POC; and biogenic silica, bSi) following a Phaeocystis bloom in the central Labrador Sea during a 2-week, using total and particulate ^{234}Th measurements. This long process study observed the decline of a specifically large bloom in spring 2022.

This is a robust, comprehensive, high-resolution dataset presenting export and transfer efficiencies data obtained from a bloom of Phaeocystis in a relatively under sampled area. ^{234}Th measurements are extremely detailed and results obtained are rigorous and the figures in the main text and in the supplementary material are very informative. The manuscript reads nicely, it is very well structured, with clear objectives and conclusions.

The results show a contrasting behaviour in relation to diatom blooms. Phaeocystis blooms are rarely examined in terms of the carbon export and sequestration during and the results prove the key influence of phytoplankton community structure on carbon export efficiency. The manuscript is worthy of publication following minor changes.

GENERAL COMMENTS:

- It is interesting that here two different depths, 100 m, used profusely in the past, and the base of the Ez, defined here as the base of the PPZ, were used to estimate the fluxes for all the stations. This is a good approach, as in many occasions, the choice of the depth at which carbon export is obtained might lead to over or under estimations if a fixed depth is used, instead of the base of the Euphotic zone. It is very nice that in line 333 the results using both depths are compared. However, in the abstract and in Figure 7 and the subsequent discussion, it is used 100 m to give the references data of export and transfer efficiency. Considering that those data are the ones used to compare to other location, in my opinjion, the standr metric to calculate the export depth should be, whnever it is possible, the base of the Euphotic zone, not 100 m.

- Similarly, the NPP in this paper has been calculated on site, and that improves temporal and geographical resolution of the NPP results. Additionally satellite NPP has been calculated, this allows obtaining a total vision of the evolution of the bloom throughout the whole season. And, interestingly, both results are compared in Figure S6 and they are in good agreement. The only differences arise at the beginning of the cruise, East-1, where in-situ NPP is twice the satellite one. This have implications in the total export and sequestration efficiency values in that period, although it does not change the main conclusions of this work. I understand that the use of in-situ NPP is more correct in this case, because it ha a better temporal and geographical resolution that allows to obtain more accurate export efficiency and sequestration results. However, it is important to note that most of the export efficiencies are usually calculated using satellite NPP, so when they are all compared in

Figure 8, the distribution of East-1 might be slightly different in the Figure, and exports would be a bit higher if satellite NPP were used. It is minor comment, but maybe it could be discussed in the text, without changing the Figure.

MINOR COMMENTS:

Abstract

Line 34 - 35. This sentence is very similar, almost a repetition, to that of lines 31 – 32. Furthermore it is necessary to emphasize that 29% export efficiency corresponds to the decline of the bloom, not the peak. It reads slightly confusing when one reads from line 30 to line 35 and a bit of clarification is needed.

Line 36 – 36. Maybe change "long term" to "across the bloom", or "along the season"

Figure S2. The grey shade of the window of success can not be seen very clearly.

Introduction

Line 45 - 46. This lines must be reworked slightly. It is not clear that only a small fraction of the material in the Euphotic zone reaches into the Twilight zone and from that, a variable fraction reaches the sequestration depth. You need to specify that that it is not "most of the material": it is most of the material that has been transported below the EZ up to what it is call the sequestration depth. And the material that does not reach this depth is remineralized back into CO2 and is not stored for centuries.

Line 51. Better quantification of particulate matter fluxes and its most influencing parameters.

Line 58. The Roca-Martí reference is in a weird format.

Line 79. Include a line here highlighting the importance of this paper to serve as a baseline for POC fluxes at the Labrador Sea in a rapidly changing environment with an uncertain evolution.

Methods

Line 115. Include in this section the description of the method to obtain NPP results from satellite that are so nicely shown in Figure S6. And mention here or in the results this figure and the comparison of both methods.

Figure 5. Uncertainty bars should be included directly in the figure. If they are not included for more clarity in the Figure, the average relative uncertainty (e.g. 5, 10, 15%...) should be included in the caption.

Line 231 – 233. This is an interesting approach. However, it is not clear how the authors are sure that base of compartment includes slow sinking (and which ranges are we referring by "slow"?) and the tray includes fast sinking (why "fast"? again, which range of SV are we referring to?). Besides, it is said that both fractions are later combined. I don't understand why this is done. If these POC/Th ratios from the MSC will be compared with the SAPS ratios, wouldn't make sense to compare the "slow" fraction with the small fraction from the SAPS and the "fast" fraction with large particles from the SAPS? Maybe, this will be described in other papers, but no questions should be left open in this paper.

Line 246 – 247. However, if I understood well, MSC contains fast and slow fractions, meaning that small particles are also included in MSC ratios.

Line 258. This section, in my opinion, it is not "methods", I would include this in the discussion section.

Line 282. I would move this section to discussion, together with the NSS discussion above.

Line 286. Is there a reference for the convection times?

Results

Line 300. Maybe this could be a good place to include the mention to Figure S6.

Line 339. Maybe I am not reading Figure 5 well, but according to the uncertainty, I don't find remarkable differences in the C/Th ratios.

Discussion

Line 432. This sentence must be rewritten, it is saying that at the decline of the bloom the POC export is not significant, which is actually something that could be expected, as it is the decline of the bloom. On the other hand, when one reads the whole discussion learns that the POC export is low during the bloom (East 1), but enhances when the bloom is declining (East 2), so I don't understand the sentence. Seems incoherent with the rest of the discussion.

Line 435. Figure 7 and Figure 8. This lack of attenuation of the flux down to 500 m is very interesting and could be further discussed. A transfer efficiency of 100% is remarkable, and the reasons for it should be commented on it.

Line 436. Do you mean lower fluxes that in the station out of the bloom?

Line 439 – 454. This last compilation paragraph of previous studies does not completely connect with the results of the paper. It will be interesting to include a discussion about how the results here exactly relate with the previous different hypotheses about Phaeocystis blooms, the discussion is now a bit loose.

Line 442. "Phaeocystis-derived material is largely recycled in the upper ocean". In which sense? Because what we see in this work is that it is largely recycled above the EZ, but from the base of the Ez to 500 m the recycling is negligible.

Line 447. Similarly, it is necessary to define "shallow".

Line 468 – 484. This is a very interesting discussion, but at the end of it, there is no specific conclusion regarding these specific results, right? Do the authors believe that the TEP is responsible here of the low expert efficiency in Eat 1, and how this relates to the higher efficiency in East 2.

Line 485 - 492. In this paragraph, how the bSi results shown here are related with the previous paragraph and , overall, to the low fluxes obtained? Is there any correlation between bSi, bSi/POC and the magnitude of the fluxes within stations and/or the attenuation differences within stations? Specially East 2

Line 501 – 504. Connecting to my comment in Line 468, this paragraph could be expanded or described a little bit. It looks as if the mains discussion about this will go to other papers, it should be included a preliminary hypothesis of why initially the bloom is not exporting the carbon efficiently, but in the second part of the cruise, when it is declined, efficiency increases dramatically.

Line 530. I am not sure hypothesis 2) is more likely, Henson 2015 reasons for the decoupling between primary production and export is attributed to different reason that here. This should be better argued.