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

We thank Reviewer 2 for the careful and constructive review of our manuscript. We greatly appreciate the positive evaluation of the dataset and are pleased that the reviewer finds the manuscript worthy of publication following minor revisions. Below, we respond to each comment point by point and indicate how the manuscript has been revised in blue text.

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.

Response: We thank the reviewer for this thoughtful comment and agree. Whenever possible, the base of the euphotic zone is the most appropriate depth horizon to estimate export efficiency, particularly in regions where the euphotic zone depth varies substantially among stations, as in our study.

We would like to clarify that in this study, POC export fluxes are in fact consistently calculated at the base of the PPZ (used to operationally define the base of the euphotic zone), not at a fixed depth of 100 m. We believe that the reviewer's comment above may have been a misunderstanding. The reference to “100 m” throughout the manuscript corresponds to fluxes measured "100 m below the base of the PPZ", which are used to calculate transfer efficiency. In the revised manuscript, we will make sure that this is explicit, such as in line 333: “*In general,* *considering all stations, $^{234}$Th fluxes increased slightly from the base of the PPZ to 100 m below the base*".

- 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.

Response: The reviewer makes a good point. We will clarify this aspect by (a) slightly changing Figure 8 caption and (b) adjusting the text:

In the revised manuscript, Figure 8 caption will be rewritten as:

*Figure 8. Biological carbon pump efficiency (BCP) comparing bloom studies across different regions (see Table 2). The x-axis depicts transfer efficiency (flux at 100 m below PPZ base/flux at PPZ base) and the y-axis export efficiency (flux at PPZ base/integrated NPP within PPZ). Contour lines represent overall BCP efficiency (percentages), and symbol area is proportional to NPP. The "East 1" and "East 2" symbols and the diamond correspond to this study. The diamond represents the overall BCP efficiency for the Labrador Sea 2022 bloom, calculated using satellite-derived NPP integrated from the bloom peak to the final day of sampling. Studies are colour-coded according to the dominant phytoplankton species. The ALOHA site (Buesseler and Boyd, 2009) is also shown as a low-end member. PPZ = primary production zone, NPP = net primary production.*

We will also clarify in text in L541:

*The generally good agreement between in situ and satellite-derived NPP over the course of the expedition (Fig. S6)* provides confidence in *the use of satellite NPP for calculating the overall efficiency.*

We also add a new sentence (added to the results, line 312):

*"The measured in-situ Integrated NPP (mmol C $m^{-2}$ $d^{-1}$)and satellite derived NPP were in reasonable agreement, with a mean measured-satellite ratio of 1.25 ± 0.69 (n = 5), supporting the use of satellite-derived NPP for estimating export efficiency."*

**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.

Response: Thank you. We agree the wording is repetitive and could lead to confusion. We will revise this section of the Abstract to make it clearer. The overall efficiency value of 6% refers to East 1 conditions approximately two weeks after the bloom peak. The value 29% refers to East 2, during later post-bloom conditions, and the apparent increase reflects declining NPP rather than enhanced export fluxes:

*Using in situ net primary production (NPP) rates, we determined that 2 weeks after the bloom peak, only 6% of NPP was exported to 100 m below the euphotic zone. Three weeks after the peak, this value increased to 29%, reflecting a decline in NPP while POC fluxes remained relatively constant. However, when using satellite-derived NPP integrated from the bloom peak until its end, the overall biological carbon pump (BCP) efficiency was 6%, indicating that this Phaeocystis bloom represented a low-efficiency export system.*

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

Response: Agreed. We will revise this wording to: "along the bloom period" to emphasize that our observations capture the temporal evolution of the bloom rather than a temporal snapshot: "*We stress the importance of multiple observations of both NPP and POC export along the bloom period for estimating meaningful BCP efficiencies*".

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

Response: Thank you. We will improve the contrast of the shaded area in Figure S2 so that the "window of success" is more clearly visible.

**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.

Response: We agree. This sentence (L45 – 46) is now written as: *"Of the material exported below the euphotic zone, most is remineralized in the ocean interior, while only a small fraction reaches depths where $CO_2$ can be stored for several centuries or longer"*.

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

Response: We agree and will revise the sentence accordingly.

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

Response: Stephens and Roca-Martí are both first co-authors of this publication, which is why the reference is formatted as Stephens and Roca-Martí et al., 2025.

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.

Response: We appreciate this suggestion and agree. We will add the following sentence in the Introduction: "*Therefore, our new observations will provide an important baseline for constraining POC export in the Labrador Sea under rapidly changing hydrographic and ecological conditions*."

**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.

Response: Thank you. We agree that the satellite-derived NPP approach should be described explicitly in the Methods rather than only appearing later in the manuscript. In the revised manuscript, we will include the information below, which was previously 'hidden' in the supplementary material:

*"Satellite NPP was calculated following the methods described in Devred et al. (2025). Briefly, the satellite product is based on daily 4-km MODIS-Aqua level-3 fields of remote sensing reflectance, sea surface temperature (SST), and photosynthetically active radiation. Chlorophyll-a was derived using a regionally-tuned version of the MODIS OC3M algorithm, and gaps in chlorophyll-a and SST were filled using Data Interpolating Empirical Orthogonal Functions (DINEOF)."*

The comparison of in situ and satellite NPP (Fig. S6) is now included in the Results section (see our response to the 2nd general comment above).

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.

Response: Indeed, uncertainty bars are already included in the existing Figure 5, but they are small and hard to see. We will add the following sentence to the caption: "*Analytical errors are represented by horizontal bars and errors less than ~3 umol dpm$^{-1}$ are not visible against filled symbols*".

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.

Response: We appreciate this comment and agree that our description of the marine snow catcher fractions was too brief. Our intent here was not to resolve a full sinking velocity spectrum, but rather to distinguish operationally between material that sinks into the MSC lower compartments during the settling period and material remaining suspended, following standard MSC usage. We agree that terms such as "fast-sinking" and "slow-sinking" should be clearly framed as operational definitions based on the sampling protocol, not absolute or directly measured sinking velocity classes. Note also that we were not attempting a one-to-one size-fraction comparison between MSC fractions and pump size fractions. The purpose of the MSC-LVP comparison was to test whether the bulk C/Th ratio of sinking material collected by MSC is consistent with any of the C/Th ratios measured with LVPs. As described in section 2.4, we use the >51 µm fraction in our flux calculations given the similarity between sinking MSC C/Th ratios and those measured in pump size fractions >51 µm.

We will revise the description of the MSC to increase clarity. The paragraph will read as follows:

*"At Station 12 in East 1, particles were also collected using marine snow catchers (MSCs) as an independent method to constrain POC/$^{234}$Th ratios in sinking material. Three MSCs were deployed approximately 3.5 h prior to the large volume pump deployments at similar depths (165, 265, and 485 m; ± 5 m). 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$^{-1}$, were siphoned from the base. Fast-sinking particles, with velocities >18 m d$^{-1}$, were trapped in the tray at the bottom of the base section. On average, 2.1 L (of 5.0 L) from the base and 1.4 L (of 2.6 L) from the tray were filtered through 25-mm-diameter Ag filters (0.45 µm nominal pore size) for the determination of $^{234}$Th and POC in slow- and fast-sinking particles. Blank corrections were applied by subtracting the average of two filter blanks from total $^{234}$Th and POC measurements, and the slow- and fast-sinking particle fractions were subsequently combined. Average uncertainties were 1% for POC and 17% for particulate $^{234}$Th, resulting from the blank correction (POC and $^{234}$Th) and counting ($^{234}$Th). The sample from 165 m was excluded from analysis due to a low signal-to-noise ratio in the $^{234}$Th measurements."*

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

Response: The MSC fractions are not strictly size defined, so the MSC sinking fractions may include both "large" (>51 µm) and "small" (<51 µm) particles. However, the good agreement between the C/Th ratio in sinking material (MSC) and the C/Th ratio in >51 µm particles (LVP) suggests that the sinking fraction likely mostly included large particles during our study. We hope the changes made in response to the point above clarify that the MSC comparison is not intended as a strict size-fraction equivalence test, but rather as an independent comparison with sinking material.

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

Response: We thank the reviewer for this suggestion. We have decided to keep this section within the Methods as its purpose is to describe and justify the methodological approach used to derive POC fluxes, rather than to interpret the results. We are also reluctant to further lengthen the Discussion section, as, following Reviewer 1's recommendations, we will incorporate the "Comparison with other studies" subsection into the Discussion (Section 4.1), which will already result in an additional 14 lines.

As an alternative, we will add the following sentences at the beginning of the Discussion (L407) to provide an overview of the section and to direct readers interested in the methodological details of the POC fluxes to the relevant section where this information is provided:

"*We begin the Discussion by providing an overview of the POC fluxes measured in this study and comparing them with those reported in other studies. We then examine the possible reasons why the massive Phaeocystis bloom observed here did not result in enhanced fluxes, with a particular focus on the ballasting hypothesis. Lastly, we present estimates of BCP efficiency during this Phaeocystis bloom using both snapshot and longer-term NPP observations. The assumptions underlying the export calculations, including the steady-state framework, are described in Sect. 2.5.*"

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

Response: Thank you. Please see our response to the comment above.

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

Response: Yes, the reference will be added to the end of the sentence (Yashayaev, 2024).

Yashayaev, I.: Intensification and shutdown of deep convection in the Labrador Sea were caused by changes in atmospheric and freshwater dynamics, Commun. Earth Environ., 5, https://doi.org/10.1038/S43247-024-01296-9, 2024.

**Results**

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

Response: Agreed. Figure S6 will be mentioned after describing in situ NPP (L312).

In the revised manuscript (L312), we add:
*The measured in-situ Integrated NPP (mmol C $m^{-2}$ $d^{-1}$) and satellite derived NPP were in reasonable agreement, with a mean measured-satellite ratio of 1.25 ± 0.69 (n = 5),*

*supporting the use of satellite-derived NPP for estimating export efficiency.*

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.

Response: We disagree on this point. The C/Th ratios in >51 um particles change from ~ 40 to ~4 µmol $dpm^{-1}$ between near surface (40 m) and greater depth (490 m; see Fig.5). This is a difference well above the error bars. We will slightly reword the related sentence to make this clearer: "*The C/Th ratios in >51 um particles change from 41 to 4.0 µmol $dpm^{-1}$ between near surface (40 m) and 490 m (Fig. 5)*".

**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.

Response: We agree that the original wording was ambiguous. We are saying that, although POC fluxes at the base of the PPZ were overall moderate to high, they were not enhanced in the *Phaeocystis* bloom region (East 1 and East 2) relative to the stations sampled outside of the bloom (Central). We will revise this sentence to make this comparison explicit and avoid any inconsistency with the later discussion: "*POC fluxes at the base of the PPZ were, overall, moderate to high, with average fluxes of 8 and 11 mmol C $m^{-2}$ $d^{-1}$ in East 1 and East 2, respectively, and 17 mmol C $m^{-2}$ $d^{-1}$ in Central. This indicates that POC export was lower in the Phaeocystis bloom region relative to the stations sampled outside of the bloom*".

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.

Response: In agreement with prior work, our results suggest that remineralization of sinking particles mostly occurred within the euphotic zone, as is already mentioned in a later part of the discussion. We will now refer to this fact in this paragraph and add a sentence commenting on the lack of flux attenuation down to 500 m:

"*Our observations of large excesses of $^{234}Th$ relative to $^{238}U$ at around 70–100 m in the Eastern Grid (Stations 9 and 12) and Station 28-1 (Fig. 4) indicate remineralization of sinking particles within the euphotic zone in the Phaeocystis bloom region, and limited POC export to the mesopelagic. In general, POC fluxes did not decrease from the base of the PPZ to 500 m, except for East 2 where all stations showed a consistent decrease (30–60%). This lack of flux attenuation down to 500 m suggests that the material that was not subject to remineralization in the euphotic zone was more refractory in nature or sank at relatively fast rates. POC fluxes at 500 m were on average 9 and 7 mmol C $m^{-2}$ $d^{-1}$ in East 1 and East 2, respectively, and 13 mmol C $m^{-2}$ $d^{-1}$ in Central. Therefore, our results also show lower fluxes at depth in the Phaeocystis bloom region than at the stations outside of the bloom. Despite the limited flux attenuation found in the upper mesopelagic, taken together, these findings suggest a limited role of Phaeocystis in export.*"

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

Response: Yes, thank you for pointing this out. Here, we meant that POC fluxes at 500 m in the *Phaeocystis* bloom region (East 1 and East 2) were lower than those measured at the stations outside of the main bloom area (Central). We will revise the sentence to make this comparison more explicit as specified in our previous response.

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.

Response: We agree with the reviewer. We will restructure the Discussion in the revised manuscript by moving this paragraph from Section 4.1 to Section 4.2. This will allow us to provide a clearer link between the literature and this study. We will first describe the different evidence from the literature on the role of *Phaeocystis* on export and the circumstances when *Phaeocystis* can led to enhanced fluxes, followed by a more specific discussion on the ballasting hypothesis and our results of POC and bSi fluxes (giving information on the ballasting role of diatoms).

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.

Response: In our study, the evidence points more specifically to substantial recycling occurring in the shallow upper water column above the base of the PPZ, whereas attenuation below the PPZ was limited. We will revise the wording as follows: "*Our observations of large excesses of* $^{234}$*Th relative to* $^{238}$*U at around 70–100 m in the Eastern Grid (Stations 9 and 12) and Station 28-1 (Fig. 4) indicate remineralization of sinking particles within the euphotic zone in the Phaeocystis bloom region*" (Section 4.1) and "*In our study, the large excesses of* $^{234}$*Th observed within the euphotic zone in the bloom region (Fig. 4), also support remineralization of sinking particles in the upper water column during Phaeocystis blooms*" (Section 4.2).

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

Response: Agreed. We will define this more explicitly (please see response above).

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.

Response: This paragraph summarizes what other studies have found regarding ballasting in *Phaeocystis* blooms. In the following paragraphs, we discuss our POC and bSi data and we conclude that diatoms did not appear to play an important ballasting role (L498-500). However, we agree that we should link that finding with the low export efficiencies estimated in Section 4.3. In the revised manuscript, we will include the following sentence (L552): "*Insufficient mineral ballasting may have contributed to the low export efficiency and overall low BCP efficiency observed during this massive Phaeocystis bloom*".

We also note that the present dataset does not allow us to directly quantify the role of TEP or aggregate properties. These factors will be assessed in following studies (L501-504).

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

Response: We agree that the connection between the bSi results and the ballasting hypothesis, as well as the broader interpretation of the low fluxes obtained was not sufficiently explicit in the original text. In the revised manuscript, we will clarify that the bSi and bSi/POC data are used primarily to assess the potential role of diatoms as ballast for sinking organic matter. In this context, the relatively low bSi/POC ratios in East 1 are consistent with a limited contribution of siliceous ballast, which may help explain why POC export was not enhanced in the bloom region despite high biomass and NPP. By contrast, East 2 showed somewhat higher bSi/Th and bSi/POC ratios, suggesting a relatively greater contribution of diatom-associated material to the sinking particle pool during the later stage of the bloom. However, we do not present a clear quantitative relationship between bSi (or bSi/POC) and the magnitude of POC fluxes or attenuation across stations. We therefore treat the role of bSi in a qualitative matter and interpret it as one of several factors that may have influenced export efficiency, rather than as a dominant control that can be demonstrated directly from the present dataset.

In the revised manuscript, we will include a sentence at the start of the paragraph (L485) and reword the following sentences to make the connection clearer: "*If insufficient ballasting limited export, then we would expect a low contribution of dense mineral phases such as bSi to sinking particles. In line with this, molar bSi to POC ratios in particles >51 µm within the PPZ were lower in East 1 (0.02–0.08) relative to those in Central (0.13) and East 2 (0.15–0.20), which likely suggests a change in phytoplankton community composition towards more diatoms in East 2. This observation is consistent with genomic samples taken during the cruise showing a lower relative contribution of diatoms to biomass in East 1 (Stevens-Green et al., 2024; Romanelli et al., 2026). Likewise, bSi fluxes at the base of the PPZ were 2-fold lower in East 1 than in Central and East 2 (Table 1), indicating a smaller contribution of siliceous plankton to export flux in East 1. However, below the PPZ, bSi fluxes in East 1 and East 2 became more similar, with fluxes within the same range at 500 m. Overall, bSi/POC ratios were relatively low in all areas, with averages from the base of the PPZ down to 500 m of 0.14 ± 0.03 (East 1), 0.18 ± 0.06 (Central), and 0.22 ± 0.04 (East 2), supporting a limited* *contribution of siliceous ballast in this study.*"

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.

Response: Other factors that may help explain the low fluxes observed, including the biochemical composition and morphology of particles and their susceptibility to be degraded, are beyond the scope of this paper. However, we have expanded and improved the discussion related to the ballasting hypothesis and its connection to the low fluxes and efficiencies observed (see above), and thus think that the reviewer's point is addressed.

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.

Response: In the revised manuscript, we will include, in addition to Henson et al. (2015), two additional references to acknowledge earlier studies (Smetacek et al., 1984, Kiørboe et al., 1996) that provide evidence that NPP and export are separated in time by periods of days to weeks. As discussed in the manuscript, this time lag between NPP and export ("today export comes from production from several days ago") has been shown to explain the inverse relationship between NPP and export efficiency in different oceanic regions (Laws and Maiti, 2019; Roca-Martí et al., 2017). That is why we think that to evaluate export efficiency, it is required that we go beyond snapshot observations of production and export and considering longer time scales.

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.