

Detailed response to RC1:

I. Abstract:

[Line 22] “macronutrient additions”.

We corrected this following the suggestion.

II. Introduction:

[Line 77–79] I got confused about the references cited in the sentence. In Assmy et al. (2017 Sci. Rep.), the only statement about Si concentration is “Silicic acid concentrations in the upper 50 m during the bloom period remained close to winter values at $4.0 \pm 0.4 \mu\text{mol L}^{-1}$ (Fig. S8), suggesting that no substantial diatom growth had taken place in these waters...”. On the other hand, Assmy et al. (2023 Prog. Oceanogr.) have also described that their “data also clearly show that low silicic acid concentrations are not a prerequisite for *Phaeocystis* to bloom as also supported by earlier observations...”.

Thanks for pointing this out. While the publication assesses *Phaeocystis* abundance and silicic acid the link between nutrient concentration and *Phaeocystis* presence is only made in the Ardyna et al. 2020 study (including for the same expedition and dataset). We therefore removed the Assmy et al., 2017 reference here.

[Line 91–92] Why and how the ratio between diatom and *Phaeocystis* is important for primary productivity and export production?

We clarified the study aim following the reviewer comment. We now explicitly state that we aim at comparing primary productivity by *Phaeocystis* and diatoms using species-specific rate measurements and to test the ratio of diatoms vs *Phaeocystis* for export. A comparative assessment of productivity rates by *Phaeocystis* and diatoms under comparable conditions to our knowledge does not yet exist. However, a significant general difference in productivity and growth rates could be a deciding factor for the competition of diatoms and *Phaeocystis* in the field. We added this statement to the introduction.

In the previous section of the discussion we describe the prior evidence for strong export by diatoms and (mostly) poor export by *Phaeocystis*, which is the reason for our interest in the role of their relative contribution for C export. Beyond a linear relationship between diatoms and carbon export, a Gaussian relationship where the combined presence of *Phaeocystis* and *Chaetoceros* might increase aggregation and therefore result in higher carbon export would be potentially feasible. We rearranged our description of trophic transfer vs carbon export to facilitate comparison between the groups.

III. Methods:

[Line 156] The “realized” average concentration... please consider to replace the adjective by other word (observed or measured?).

We replaced the adjective with “observed”.

[Line 175] The seawater pH.

We changed the terminology to seawater pH.

[Line 193] Seawater pH.

We changed the terminology to seawater pH.

[Line 193–195] I don’t understand the statements of thermodynamic equilibrium constants. Please rewrite them for clarity.

The description of the constants was shortened and their purpose clarified.

[Line 207–208] Please cite references for the oxidized method. What is the recovery of the applied method?

The Hansen & Koroleff, 1999 reference that was cited in the following sentence is the reference for POP analysis. We adjusted the wording to better reflect this. This method is very well established in seawater analytics, including previous mesocosm studies i.e. Sommer et al. 2007 (Oecologia), Meyer

et al. 2016 (Biogeosciences). Gomez-Letona et al., 2022 (Front. Mar. Sci.). While recovery was not quantified using organic matter standards (potassium hydrogen phosphate was used as a standard for analyses), dedicated analyses in other publications determined the recovery to be around 97% (Berthold et al., 2015).

[Line 210–213] Please check the cited references are proper. What is the recovery of the method applied in this study?

Apologies for the poor citation. We clarified the reference. The time-course digestion applied here allows complete recovery of the biogenic silica fraction due to the time-course analysis and the subsequent dissolution of recalcitrant lithogenic silica. Due to the high lithogenic load in these samples, we used a method facilitating better lithogenic correction as done previously in both high- and low-latitude coastal systems (Varela et al. 2016, Krause et al. 2023).

[Line 263–267] The statements are somewhat unclear. Are these referred to specific element (C/N/Si) uptake rate? Is the percentage referring to atom%? But the Eq.1 seems referring to absolute uptake rate (Line 267–268).

We rephrased the method section and the description of Eq. 1 to clarify how absolute uptake rates were calculated using the isotopic composition in Atom% in enriched and unenriched control samples.

[Line 269–270] I don't understand this statement. The atom% of the stable isotopes are not acquired from the control set or initial sampling at T0 (Line 239)? Are the values for blank?

The values are for dissolved inorganic nitrate and silicic acid to calculate the stable isotope enrichment in the dissolved fraction using the nutrient concentration and the stable isotope spike isotopic composition and volume. We've clarified this in the text.

[Line 293] Atom%.

We corrected this.

[Line 301] Please revise the equations as Eq.2 and Eq.3.

We reformatted this accordingly.

[Line 358–368] I suggest moving the statistics part to section 2.5.9.

We moved the sentence about statistical analysis to the statistics section.

[Line 382–383] I don't understand this statement. What levels are included in the treatment and what is the sample size (n=?) of each treatment level?

We revised this section describing the revised statistics in detail (see main response)

[Line 383] Please clarify what relationships are examined using Spearman.

As outlined in the main response we replaced the use of Spearman rank with a full linear mixed model to evaluate temporal and Si effects in abiotic parameters, particulate organic matter and uptake rates and reported this in detail in the method section.

IV. Results:

[Line 393–394] I suggest just presenting PAR, which is much intuitive for phytoplankton study, and adding a short phrase to remind readers there was polar day during the experiment.

Since a conversion of Lux based on one day of PAR measurements is, due to the non-linear relationship between PAR and Lux, not advisable, we removed the described Lux values and added a sentence about 24 h light availability due to polar day.

[Line 401] 4.2 $\mu\text{mol kg SW}^{-1}$? Please check the unit.

The unit is correct, but we added brackets around (kg SW) to clarify.

[Line 405] ...during days "3.5"-10... I don't understand what does 3.5 mean. (The number appears again in Line 419)

In line with the major request above to ensure that reported analysis are meaningful and to shorten the manuscript, this section was significantly streamlined largely removing accumulation rates based on particulate organic matter from the results section and only retaining the group-specific accumulation rates in section 5.7. To calculate the onset of exponential accumulation of biomass, we

used an average across all mesocosms. This resulted in “days”, that were not integers, as exponential growth started in some mesocosms on day 3 and in others on day 5. We clarified this in the text and rounded to full integers, in the few remaining instances.

[Line 423–436, 457] The values of Spearman’s correlation coefficient are got from sample rather than population. Please use r_s rather than p . Similar issue is also found in Line 428 (“ r ” = 0.63) and Line 509–510. Besides showing the r_s numbers, I encourage the authors present the scatter plots of Spearman correlations and the later model I linear regressions (in Supplementary is ok).

We thank the reviewer for clarifying this. As outlined above, Spearman rank correlation were replaced with mixed linear models. The scatter plots of the daily linear regressions were added to the supplementary.

[Line 475–549] I suggest combining the sections of 3.4 & 3.5 and then streamlining the contents. The original texts seem superfluous and some information could be reorganized and moved into Supplementary.

We acknowledge that this section is currently lengthy. To correct this, we combined the paragraph about nutrient availability with the text on uptake and drawdown rates. The paragraph was rearranged to discuss one nutrient after the other to avoid repetition across different parameters. Additionally, we added plots of Si effect size to the time series plot which reduces the amount of statistical information that is reported in the text.

[Line 559–575] I suggest presenting regression equations rather than just Pearson’s r or +0.23% per μM Si, for example.

We presented the linear regression equations from the mixed linear model, where relevant or linked to the effect size plots in the revised figures.

[Line 572–573] “...with budget closure improving significantly at higher Si additions (-0.30% residual per μM Si added, $p = 0.048$, $R^2 = 0.50$, Fig. 10b).” I don’t get it, specifically Fig. 10 shows 10 μM Si mesocosm, but “High Si additions ($\geq 20 \mu\text{M}$)” (Line 561) is noted.

We understand that the choice of the 10 μM mesocosm for figure 10 was insufficiently explained. Treatments 1–20 μM looked very similar in terms of budget closure, hence the 10 μM mesocosm was chosen as an example to illustrate budget closure over time. We clarified this in the figure caption and added the figures for all mesocosms to the supplementary.

[Line 585] Should be 3.7. I think this section (with Fig. 11, and Fig. 5) might be one of the most important results, while the other results could be used to explain how the succession happens.

We thank the reviewer for highlighting the importance of these findings. We rearranged the sections to move this section to an earlier position in the results to highlight their importance.

[Line 588–589] Wilcoxon rank-sum test is not mentioned in the Methods. Sample sizes of *Chaetoceros* and *Phaeocystis* biovolumes should be noted.

We added the Wilcoxon rank-sum test to the methods and added the sample sizes for the respective biovolumes.

[Line 591–592] The differences in average growth rate could be examined using randomized block design of three-way ANOVA (Si and species as treatments, and phase as block).

We appreciate the suggestion to use statistical tests to assess the differences in average growth rate. However, to our understanding a three-way ANOVA would be unsuitable as the resulting model would be too complex for the available amount of datapoints, thus not having the necessary degrees of freedom. However, we applied the linear mixed model described in the earlier comments also to the average growth rates, including species as a factor, to assess the joint effects of species, phase and potentially Si on the growth rate.

[Line 604–612] But soon I got confused in these statements, as small flagellates were included. I wonder if it would be better to show the results when the flagellates data was removed.

While we believe the flagellate data to be meaningful to get a better understanding of the overall community activity, we acknowledge that *Phaeocystis* and *Chaetoceros* are at the center of the publication. We therefore removed the flagellate data from the text and Figure 12 and adjusted the method section accordingly. We furthermore clarified the contrasting responses of *Chaetoceros* and *Phaeocystis* over time, especially with regards to the C:N ratio.

V. Discussion:

[Line 660] The cited reference was not found in the Bibliography list.

We added the correct reference to the bibliography.

[Line 664–666] I got confused about the statements and Fig. 13. What are the thresholds to decide the type of limitation during the experiment? Furthermore, as samples were collected every two days, how to present the data point each day in Fig. 13?

The thresholds represent nutrient concentrations that are generally considered limiting in these environments. In terms of sample collection, only nutrient samples were collected daily to inform the regular nutrient additions. We, however, removed this figure from the publication and are referring to Figure 6 instead, when describing the nutrient limitation and co-limitation.

[Line 670–717] It seems there are too many details in this section. Please consider to streamline and reorganize the section. Similar issue in the following sections.

The discussion of potential reasons driving the competition of *Chaetoceros* and *Phaeocystis* is complex and multifaceted. We acknowledge that the corresponding section therefore got too detailed. We streamlined and restructured section 4.1 of the discussion to reduce the amount of literature review contained in the discussion and to create shorter, more concise paragraphs throughout this section.

VI. Conclusion:

[Line 858–860] Please consider to rewrite the sentence or reconstruct it into two sentences.

We changed the text and created shorter sentences, while also addressing concerns of reviewer 2.

VII. Tables and Figures:

[Table 2] Should be Table 1. Please revise the statistical symbols following the abovementioned comments..

We renumbered and adjusted the table to appropriately report the result of the linear mixed model..

[Figure 1] Please reorganize the layout. Add a latitude line to show the boundary of arctic region. Add new labels or enlarge existed labels to clearly show important seas, lands, and spots (such like Kongsfjorden and Svalbard). A combination of Figure 1 and Figure 2 might work.

We revised Figure 1 and combined it with Figure 2 using OceanView to regenerate the maps for Figure 1 and added more legible labels.

[Figure 5] Add “Day” in front of the experiment day for each panel.

We adjusted the panel header to include “Day”.

[Figure 12] The label of the X-axis should be Si availability. Maybe move the data of small flagellates into Supplementary?

We corrected the x-axis label and remove the data of the small flagellates (see response to result comment)

[Figure 13] Please see the comment of [Line 664–666].

We removed Figure 13 (see response to comment [Line 664-666])

VIII. Bibliography:

[Line 1007] I don’t see the reference of “De La Roche & Passow, 2004 L&O 49:245-255” listed here (cited in Line 660).

The reference was added to the bibliography.