Detailed response to RC2:

1. Major Comments

Comment 1 (lines 625-630, line 140)

Issue: The authors repeatedly emphasise that “Phaeocystis bloom developed regardless of Si supply” (L625-630) and use this to challenge the view that Si availability is a key determinant of Phaeocystis vs. diatom dominance. However, the authors themselves state (L140) that initial Si concentrations in the fjord water were already below detection limit at the time of mesocosm filling. This means that all mesocosms started under severely Si-depleted conditions, which likely suppressed the initial recovery and division of Chaetoceros, indirectly favouring Phaeocystis. This is a legacy effect of pre-experimental Si limitation, not a true “no effect” of Si. Consequently, the claim that Si does not govern initial competition is over-stated and does not adequately account for initial conditions.

We thank the reviewer for this detailed comment. As stated in the manuscript we agree that initial Si concentration in the field resulting in Si limitation of diatoms potentially contributed to the initial Phaeocystis dominance. And we acknowledge that individual sentences make strong statements about the missing role of Si supply on the Phaeocystis bloom. However, while the key reason for the initial Phaeocystis bloom dominance is likely associated with the prior conditions in the fjord, we do not agree that pre-experimental Si limitation is the main reason to favor Phaeocystis. Silicic acid limitation does not affect the physiological health of the cellular machinery the same way as N and P (or Fe) limitation, which allows diatoms to rapidly grow upon Si resupply, particularly if we consider boom-and-bust-type species like *Chaetoceros*. Therefore, silicic acid resupply at the onset of the experiment should have resulted in a strong response by diatoms. The delayed response of diatoms by 8-10 days suggests that other reasons contributed to this delay including other bottom up factors as well as top-down factors (i.e. grazing and viral pressure). We have streamlined and reorganised the corresponding discussion section and toned down the specific sentences stating full independence of Si supply, to acknowledge the potential contribution of Si limitation, while highlighting the complex interaction of bottom up and top down factors in regulating Phaeocystis vs diatom growth.

Comment 2 (lines 251-260, Fig. 12)

Issue: The authors honestly acknowledge (L251-L260) that because isotope enrichments exceeded the recommended 10% level, the measured C, N and Si uptake rates represent potential rather than in situ rates. This is a critical methodological constraint. However, in the results (especially Fig. 12, single-cell uptake) and throughout the Discussion, these “potential” rates are treated as if they reflected actual competitive performance. For example, Fig. 12 shows higher Chaetoceros uptake on Day 6 than Phaeocystis, but this may simply reflect the maximum potential of Chaetoceros under enriched conditions, rather than its real performance under natural nutrient fluctuations. The current presentation risks over-interpreting species-specific differences in nutrient acquisition.

We thank the reviewer for catching this. However, although not stated clearly in the current method section of the manuscript, only isotope enrichments for N and Si (in those mesocosms and days where nitrate and silicic acid were very low) exceeded the recommended 10% level. This is not the case for bicarbonate stable isotope additions, for which 13C-DIC measurements confirmed that the final enrichment after the incubation was 9.9±0.6 Atom% across all mesocosms and incubation days. Correspondingly neither the bulk rates of C uptake nor the species-specific single cell rates in Fig. 12 constitute an overestimation, but can be considered in situ rates. We added the measured enrichment level to the method section and clarified the description of the isotope enrichments.

Comment 3 (lines 162-164)

Issue: To prevent CO_2 limitation, the authors added $NaHCO_3$ and HCl to all mesocosms (L162-L164). While this helped maintain pH, it artificially altered the dissolved inorganic carbon (DIC) pool and its carbon isotopic composition. The added HCO_3^- dilutes the ^{13}C-DIC label, potentially biasing the calculation of carbon fixation rates derived from ^{13}C uptake. Furthermore, whether this manipulation affected the physiology of Phaeocystis (e.g., its preference for HCO_3^- utilisation) remains unknown.

We thank the reviewer for raising this issue. However, the 13C-DIC label was not added directly to the mesocosms but was only added to subsamples for bioassays. Within these bioassays the ^{13}C-DIC isotopic composition was measured, ensuring that the carbon fixation rates are accurate. Additionally, the addition of bicarbonate occurred together with the nutrient addition i.e. on separate days from the sampling, including the sampling for the bioassays, ensuring that the carbonate system had time to respond to the bicarbonate addition and stabilise before the subsampling. Of course, effects of the manipulation for the key species can not be fully excluded. However, a preference for bicarbonate should have a stimulating effect on *Phaeocystis*. Instead the bicarbonate additions as a response to decreasing DIC/increasing pH started shortly before the crash of *Phaeocystis* (coinciding with the strongest nitrate depletion). While this could have potentially contributed to the collapse of the *Phaeocystis* bloom, previous studies (e.g. Trimborn et al. 2013) have directly examined the response of Phaeocystis to different carbon concentrations and did not find a sensitivity of *Phaeocystis* growth rates to CO_2, making it unlikely that DIC limitation or bicarbonate addition played a significant role in the species transition, while nitrate limitation is a more likely candidate. Since it can however not be excluded as a contributing factor we added a potential contribution of DIC to changes in community composition to the discussion.

Comment 4 (line 546, Table 2)

Issue: The text notes that “Phase II N:Si uptake ratios showed extreme treatment dependence (0.1-15.7), with low values at high Si treatments (10-40 µM: 0.2-0.3)” (L546). This is striking because the classical Redfield N:Si ratio for diatoms is ~1. Values as low as 0.2-0.3 under Si-replete conditions suggest either a very low N demand, internal nitrate storage, or that an unmeasured N source (e.g., ammonium) contributed to growth. However, this key observation is not discussed in the relevant section (L660-L680).

These are all interesting suggestions to consider. We included a brief discussion of the significance of low N:Si rates to the discussion. As these measurements refer to rates and not to biomass, the taken up nutrients are not necessarily used for growth. A surplus of Si could also have been taken up and effluxed out again. Such surge and efflux behavior may be in response to the experimental perturbation (e.g. Thamatrakoln & Hildebrand 2008), but not necessarily associated with growth. That being said, if the diatom assemblage was kinetically limited by the ambient Si, then such apparently excess Si uptake could be due to release from Si limitation and a lack of corresponding release from N limitation.

Minor Comments

Line 75-80: The authors cite Ardyna et al. (2020) and Assmy et al. (2017) as evidence for the “low Si favours Phaeocystis” hypothesis, but they do not acknowledge alternative or complementary hypotheses that have been proposed to explain Phaeocystis dominance, such as: (1) differential grazing pressure (microzooplankton preferentially grazing diatoms), (2) colony formation as a defence mechanism, (3) temperature optima differences, or (4) light/shade adaptation. By presenting only the Si-limitation hypothesis, the authors set up a straw-man argument that their experiment then “challenges,” without giving due credit to the complexity of the existing literature.

While these different contributing factors are assessed in detail in the discussion, we acknowledge that they are not presented in corresponding detail in the introduction. We have expanded the corresponding section to better encompass the variety of reasons that have previously been suggested to affect growth of diatoms and *Phaeocystis*.

Line 232 The reference Liguori et al., 2020 is not present in the bibliography (only Bracher, Chen, etc. from 2020 are listed).

We thank the reviewer for catching this. The reference for Liguori et al., 2020 was added to the bibliography.

Line 247**:** The parentheses contain an author's internal note ("get details for stockholm mass spec") that should never appear in a submitted manuscript.

We apologise profoundly for this mishap. The correct information for the mass spectrometer was added to the method section.

Lines 251–260: The Methods clearly state that because isotope enrichments exceeded recommended levels, the measured rates reflect potential rather than in situ uptake. However, in the Results (especially Fig. 12) and throughout the Discussion, the figures and text are labelled simply as "uptake rates," without emphasising the "potential" qualifier. This risks over-

We addressed the issue in detail in the major comments. For N and Si we added a "potential" qualifier throughout the manuscript.

Line 630-640: While the authors later acknowledge that "Si limitation prior to the experiment" may have constrained Chaetoceros recovery (L860–865 in the Conclusion), this acknowledgment comes far too late and is not integrated into the main discussion of competition. The central conclusion—"Si availability does not govern initial Phaeocystis-diatom competition"—is stated repeatedly (L625–630, L858–860) without adequately weighing the counterargument that pre-existing Si depletion may have been the reason diatoms did not compete effectively at the start. This is a major logical gap.

This issue is addressed in detail in the corresponding major comment and we contextualised the conclusion to reflect the potential contribution of Si limitation, while also including the evidence for other relevant drivers.

Line 630-640: This conclusion is based on literature showing similar optimal temperature ranges, but the experiment experienced a rapid temperature drop from ~2.3°C to -0.2°C around Day 15 (L390–392). The authors argue this drop did not affect productivity, but they provide no analysis of species-specific temperature sensitivity at sub-optimal temperatures. The literature cited (Hegseth et al., 2019; Simo-Matchim et al., 2017) may not address the effects of rapid cooling on division rates. Furthermore, temperature affects nutrient uptake kinetics (Lomas & Gilbert, 1999), so the interaction between temperature and nutrient limitation is complex and cannot be dismissed based on optimal growth ranges alone.

We acknowledge that the response to a drop in temperature can not be appropriately understood based on optimal temperature ranges. We included a correlation analysis to demonstrate that there was no significant correlation of phytoplankton growth rates and temperature.

Line 720-730: The authors acknowledge (L745) that "viral lysis rates or other pathogens were not assessed within the framework of our experiment," yet they spend significant space (L720–745) constructing a narrative about viral and pathogen control that is entirely speculative. This is particularly problematic because the key evidence they cite (Kranzler et

al., 2019) comes from culture experiments showing that Si limitation facilitates viral infection in diatoms—but they extrapolate this to a field community without direct measurements. The Discussion section would benefit from much more cautious phrasing.

We thank the reviewer for this comment and agree that, in the absence of direct measurements of viral abundance or lysis rates, our previous wording overstated the role of viral and pathogen control. We have substantially shortened this section and rephrased it to clarify that viral lysis is presented as one possible, untested explanation rather than a conclusion. We have nevertheless chosen to retain a brief discussion of this mechanism due to the observation (described in detail in major comment #1) that diatom growth rates responded more slowly to Si resupply than would be expected if Si limitation was the primary factor impending diatom growth. Furthermore we now explicitly highlight that our conclusion is based on the fact that bottom up processes (as estimated by rate measurements) did not explain biomass dynamics, which makes top-down controls (despite their specific mechanisms being unclear) the other likely explanation.

Line 820-830: The authors excluded large grazers (>200 μm) from the mesocosms (L132), yet they claim the dynamics were in “good agreement” with the fjord. Large grazers, especially calanoid copepods, can exert significant top-down control on Phaeocystis and diatoms (Nejstgaard et al., 2007). If these were excluded, the mesocosm community likely experienced reduced grazing pressure, potentially inflating Phaeocystis biomass relative to the natural fjord. The authors’ claim of “good agreement” is unsupported by quantitative data, and the logical implication is that the mesocosms may not accurately represent natural top-down regulation—a point that is critical for extrapolating the findings to the broader Arctic.

We thank the reviewer for raising this issue and acknowledge that the phrasing might have been somewhat misleading. While we used a mesh to pre-filter, when we started our experiment and for the duration of the mesocosm experiment, abundances of larger grazers in the fjord were very low. While we can’t show quantitative data to support this, our original experimental setup was supposed to include grazer additions at a later point during the experiment (to address the question of top down control by larger grazers) but we were unable to do so, as even repeated sampling trips did not yield sufficient abundances of copepods. Accordingly, larger grazers are unlikely to have exerted a significant control on the phytoplankton population in the fjord, making conditions between the fjord and the mesocosms comparable. This is furthermore supported by the transition of the community in the fjord, which was comparable to the species dynamics observed in the mesocosms, supporting that large zooplankton is unlikely to have been the major driver for the observed transition in the fjord. We clarified this reasoning in the discussion section.

Figure 4：The x-axis shows Si treatments (0, 1, 2, 3, 5, 10, 20, 40 μM) at equally spaced intervals, even though the actual concentrations are non-uniform (e.g., 0→1 vs. 10→20). This may visually distort the concentration-response relationship.

While the actual concentrations are not uniform, this figure does not assess dose-response relationships but assesses the duration of exponential growth across the mesocosms over time. In this context each mesocosm represents a distinct water body, which in our view justifies the factorial representation.

Figure 5. The caption does not indicate which day (0, 7, 14, or 22) corresponds to which panel
Following the review comment,we clarified this by adding "Day" to the header of each panel. Additionally, we described this in the caption.

Tab2 should be formatted as a three-line table

We now used the three-line table design to improve the visualization of this table.