RC1 Specific comments addressed:

The authors presented a comprehensive mesocosm study in a mid-latitude fjord that employed two types of ocean alkalinity enhancement (OAE) techniques to study the changes in carbonate chemistry on metabolic rates in the experimental mesocosms. Different levels of OAE were tested and delays in phytoplankton bloom compared to control conditions were revealed. This study did not use the common OAE minerals (hydrated lime and olivine) directly but employed chemical additions that mimicked the outcome of applying these minerals hence other confounding factors such as trace metal release can be avoided, which is a clever design.

The manuscript is mostly well written, but it can be verbose in places. First, here is a technical question that I hope the authors can address. In this study nutrient sample collection and processing, the authors used 0.45 µm filters for nutrient sample collection and then the samples were kept in the dark at ambient temperature until further processing (did you mean analysis)? See Line 165-166. Given the fact that nutrient stoichiometry is important in discussing metabolism in the Ca vs. Si based OAE schemes, this nutrient collection technique needs further clarification, and the authors should affirm that the pore size and sample preservation had not inadvertently altered nutrient concentrations. See below for a reference.

Reed, M.H., Strope, E.K., Cremona, F., Myers, J.A., Newell, S.E. and McCarthy, M.J., 2023. Effects of filtration timing and pore size on measured nutrient concentrations in environmental water samples. Limnology and Oceanography: Methods, 21, 1-12.

The article provided mentions that, in terms of pore size, all nutrient samples should be filtered through maximum 0.45 µm. Our samples were collected in triplicate and filtered through 0.45 µm 1-2 hours after collection (added to the manuscript) and stored in the dark until they were analysed. They were actually stored in the fridge after filtration, which has been included in the manuscript. A piece of information missing relevant to this method's reliability is the time between filtration and analyses, which was of less than 6 hours. This information has also been added to the manuscript. The method employed to measure Si(OH)4, NO3, NO2, and PO4 concentrations, and the one followed to measure NH4, come from two publications, specifically from Hansen & Koroleff (1999) *and* Holmes et al. (1999)*, which have been cited over 1200 and 1400 times, respectively.*

Additionally, Reed, et al. (2023) evaluate how higher pore sizes and longer times until analysis reduce the PO4 and NH4 determination reliability, particularly when baseline nutrient concentrations are very low. Our discussion section on nutrient uptake focuses on Si to N (derived from NO3 uptake, not NH4) ratios, after a significant nutrient addition. Besides, after the nutrient addition, the measured concentrations were consistent with the theoretically intended ones.

Below are some minor comments:

1. Be consistent with the descriptions of the duration of the experiment. 10-weeks (Line 92), 53 days (Line 127), and three-month (Line 369) were all used.

   Addressed. They were all changed to 53-day.

2. There are many places where the words "said", "mentioned", "aforementioned", "particular", "present" etc were used and in most cases these words are either unnecessary or confusing. Please remove or reword.

   Addressed. These terms were mostly removed across the entire manuscript. And were present is used as in "the present study" (referring to ours), we now use "current".

3. Throughout the context, while it is understandable that a calcium-based chemical alternation was made to the experimental system, using “calcium” appears a little misleading because both OAE approaches intend to increase concentrations of carbonate species in the water. Silicate weathering leads to an increase in carbonate ion concentration, and hydrated lime is essentially a direct base addition, not adding calcium per se. I would suggest that the authors to reconsider the term usage.

   This terminology was mainly used to differentiate between the calcium-based and silicate-based sets of treatments, as these elements were added to test the 'green vs. white ocean' hypotheses proposed by Bach et al. (2019). *In this experiment, TA was adjusted using NaOH, thus bases were added directly in both scenarios. Therefore, the main relevant difference between the two sets of treatments is the addition of calcium and silicate. This is the key difference because even though, in the silicate-based treatments, Mg was also added to simulate a forsterite addition, this element is already found in high concentrations in seawater due to its long residence time* (Foster et al., 2010)

   Furthermore, this terminology will be consistent across many publications about different parameters measured during the same mesocosm campaign that are currently in preparation. Therefore, it will aid in the intercomparison of all these publications to get the whole story of what happened in this very large and collaborative experiment.

4. Line 44-45, improper punctuation.

   Addressed.

5. Line 90, for an uncommon chemical/mineral, explain forsterite.

   This was addressed by adding the following information: However, olivine is comprised of forsterite (Mg_2SiO_4) and fayalite (Fe_2SiO_4) in a 9:1 ratio. An iron (Fe) addition may have a fertilizing effect on phytoplankton in the photic zone (Bach et al., 2019; Hauck et al., 2016; Renforth & Henderson, 2017)*, and it is the Mg end member of olivine that, as it weathers, naturally consumes atmospheric CO_2* (Köhler et al., 2013; Renforth & Henderson, 2017).

6. Line 115, what’s in this brine solution.

   Addressed by specifying it was a NaCl brine solution

7. Line 162, provide more details on how pH was corrected and how the comparison looked like.

   An article focusing on the carbonate chemistry from this experiment is in preparation. Nonetheless, a reference to an article on seawater carbonate system considerations in the context of OAE research, which explains this process in more detail, has been added.

8. Line 178 vs. Line 190, clarify whether the “initials” were already fixed before the incubation.

   Addressed.

9. Line 180, what’s “blackout”? Please use proper term/description.

   Blackout was changed to opaque.

10. Section 2.5, more details on Chl-a processing and analysis is needed. What’s the purpose of using the 200 µm mesh?

We want to extend our gratitude to the referee for this comment because an error was detected. Samples were not pre-filtered using a 200 µm mesh (this referred to the sample processing of another parameter that is not included in the current study). We specified that samples were filtered through GFF with a 0.7 µm pore size and stored at – 80 ºC until they were analysed fluorometrically the following day.

11. Line 239-244, the sentences read awkward and confusing. Please restructure and clarify, explain what's the "controls" mean in the context of the experimental design.

    Addressed. This sentence was changed to: "Therefore, the pH and pCO_2 in the mesocosms where TA was manipulated did not reach ambient levels throughout the experiment."

12. Line 251-252, remove "significantly", and did the experimental timing coincide with post bloom period in this fjord? If so, this needs to be mentioned in the method section.

    To address this comment, significantly was removed, and the requested information was added in the first sentence of the methods section: "The experiment (KOSMOS Bergen 2022) was carried out in Raunefjorden, 1.5 km offshore from the Espegrend Marine Research Field Station, of the University of Bergen, Norway, under post-bloom conditions, starting on the 7th of May 2022."

13. Line 263, subtracted "from"?

    Addressed.

14. Line 284, "slightly almost", what does it mean?

    We want to thank you for noticing. It was a typo. Slightly was removed

15. Line 286-287, "little under" as "slightly below"?

    Corrected.

16. Line 304-307, this sentence needs to be reworded as the current form is quite confusing.

    The sentence was re-written, and we hope it is clearer this way: Therefore, the increase in GP in the silicate based, highest treatments coincided with when GP peaked in the low TA calcium ones. Hence, the delay in the community's response to the nutrient addition was longer for the calcium than the silicate treatments, in both cases following the TA gradient (Figure 3A and B).

17. Line 326-327, the sentence "Nonetheless …" is not clear.

    Addressed by specifying what pattern we were referring to: "Nonetheless, negative slopes obtained from daily linear models peaking on day 41 and that reversed on day 47, as observed in terms of GP and Chla in the calcium treatments, can be partially inferred (Figure 5B)." The following sentence was also altered to: "CR in the low TA treatments increased around the same time as in terms of GP and, the calculated CR rates that followed showed a slight recovery in the high TA treatments."

18. Line 331, define "metabolic balance"

    After metabolic balance "GP:CR" was included in parenthesis.

19. Line 343, "latter parameters" meaning?

    "Latter parameter" was changed to GP:CR

20. Line 347-353, this paragraph appears fragmented and difficult to follow. Please revise.

To clarify the content of this paragraph, it was re-written and divided in two separate paragraphs: "Furthermore, to see if the observed pattern also translated to some extent to the community composition, assimilation numbers based on the GP rates were calculated. GP was chosen due to the low and relatively constant contribution of CR, especially during the second phase. Additionally, because the NCP was positive throughout the experiment, the actual production must have been at least as much as the CR.

The GP normalization using Chla as a biomass proxy (GP:Chla) yielded assimilation numbers that remained reasonably constant throughout the experiment and overall unaffected either by the mineral treatment or by the TA gradient. Differences between phases were not apparent either."

21. Line 366-367, "persistent increase in pH and decrease in pCO2" needs proper context, it reads like these trends should correspond to the level of OAE, but not the duration of each experiment.

By specifying that the increase in pH and decrease in pCO_2 were persistent, we aimed to state that these conditions stayed relatively stable (the gradient remained) and different to ambient levels throughout the experiment. However, we explained this further by changing the sentence to: "These entailed a persistent increase in pH and decrease in pCO_2 when compared to ambient levels, since full natural equilibration throughout the duration of the experiment did not occur."

22. Line 372-374, "addition" with quotation marks, I'd make it more explicit that the experimental technique used surrogate of chemical mixers instead of direct mineral additions.

Instead of addition in quotation marks, we stated that we undertook addition simulations and specified "with compounds containing..." the key elements present in the two minerals, in parentheses: "Especially in terms of community composition with calcium-based OAE treatments through the hydrated lime addition simulation (with compounds containing Ca^{2+} and OH^- separately), potentially increasing the abundance of pelagic calcifiers, and silicate-based OAE through the forsterite one (with compounds containing Mg^{2+}, SiO_3^{2-}, and OH^- independently), favoring diatom proliferation."

23. Line 409, this 1% fraction of DIC as CO2 (which should be aqueous CO2) is salinity and temperature-dependent, so some context is needed.

True. We added "~"1% to specify that this is an approximation and stated that we are referring to the DIC in seawater.

24. Line 415-421, this discussion needs to be placed in the context of the study region to make it the case.

In parentheses we reiterated that Ruanefjorden is our study site's location.

25. Line 435, the ratio of observed Si and N uptake hinges upon the nutrient handling methods. Hence the ratio needs to be taken with a grain of salt.

The inorganic nutrient determination methods that are in question have been used over 1200 and 1400 times since 1999. Additionally, in the article put forward (Reed, et al., 2023) they evaluate how higher pore sizes and longer times until analysis reduce the PO4 and NH4

determination reliability, particularly when baseline nutrient concentrations are very low. In this part of the discussion section on nutrient uptake, we focus on Si: N ratios, in which N is derived from NO3 uptake (not NH4, nor NOx), during phase II. Thus, after a significant nutrient addition.

26. Line 451, "in terms of", meaning?

    Here "in terms of growth rates" was removed.

27. Line 455, clarify what the statement means.

    Thank you for noticing. This sentence was re-written as follows: "This would therefore explain why, when GP is normalized to Chla (assimilation numbers), no differences between mineral treatments can be inferred, even if they occurred, in absolute GP:Chla temporal development, nor in their overtime response to the ΔTA gradient (Figure 7)."

28. Line 465, remove "herein".

    Removed.

29. Line 467, the fact that Daphnia is a zooplankton needs to be mentioned here.

    Specified in line 474 ("…Daphnia, the keystone herbivorous zooplankton species…").

30. Line 484, "stronger" should be replaced with something like higher levels of chemical modification of seawater.

    Addressed by changing "stronger OAE deployments" to "higher TA levels"

The supplemental materials could use more help with higher resolution figures.

Addressed. We want to thank the reviewer for noticing.

References

Bach, L. T., Gill, S. J., Rickaby, R. E. M., Gore, S., & Renforth, P. (2019). CO2 Removal With Enhanced Weathering and Ocean Alkalinity Enhancement: Potential Risks and Co-benefits for Marine Pelagic Ecosystems. *Frontiers in Climate*, *1*(October). https://doi.org/10.3389/fclim.2019.00007

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