We would like to extend our heartfelt gratitude for the reviewer's thoughtful feedback regarding our manuscript. The time and commitment conferred to providing constructive criticism are truly appreciated. We believe the suggestions made have played a decisive role in refining the content of our manuscript, and we have incorporated all the specific revisions to improve its clarity and coherence. Should there be any elements that warrant additional clarification, we welcome the opportunity for continued discussion.

Lines 82-85: "Additionally, the type…" –This sentence is a bit convoluted; it also needs additional citations. For example, I don't think including "biogenic" when citing Moras et al. 2022 is accurate as the referenced study examines runaway $CaCO_3$ precipitation when using proposed ocean-liming minerals (e.g., CaO and $Ca[OH]_2$) – and they hypothesize that precipitation likely occurred on the surface of the undissolved ocean-liming minerals used in their study; while this offers support to abiotic particles affecting OAE efficacy – at least when using the noted ocean-liming minerals – it seems the authors are also implying that the presence of biogenic $CaCO_3$ could induce similar precipitation. If so, additional citations should be included. Citations for the other potential influences on OAE efficiency noted in this sentence would also be helpful.

We thank the reviewer for this insightful comment, and we agree. Two new references providing support for precipitation occurring in the presence of particles of biogenic origin have been added right after the word "biogenic". The sentence the reviewer considers convoluted has been divided in two.

Line 120: What the authors mean by "air-equilibrated…stock solutions" is not entirely clear here (e.g., did they bubble their solutions?). Although further explanation is provided later in the manuscript, a brief explanation along the lines presented in Federer et al. (2022) would provide clarity early on. Additionally, the method of alkalinity addition being simulated in this study is not entirely clear: if the goal was to simulate an ocean-liming scenario (as stated elsewhere in the paper – e.g., line 370), why weren't calcium concentrations also increased? Would these results also apply to aqueous hydroxide addition? For example, the use of NaOH – after its reaction with seawater – is effectively alkalinity enhancement through sodium carbonate addition. The authors also use "carbonate-based" (line 370) to describe the form of alkalinity addition being simulated – would these results apply to the use of other carbonate minerals (e.g., dolomite) as well?

Yes, agreed, this is confusing. Air-equilibrated solutions refers to them containing carbon in accordance with the targeted TA level. The order of this sentence was changed by moving air-equilibrated to before alkalinity gradient. Thus, attributing air-equilibration to the nature of the alkalinity addition, rather than the solutions themselves, which were not bubbled.

Sure, the calcium concentrations would need to be increased. However, and as a first step in the direction of evaluating impacts of ocean liming (meaning since no previous work had been carried out at the time), only alkalinity was increased. Adding calcium to the system, instead of or together with Na, would have been a confounding variable that, with no prior information would have complicated the system's response interpretation.

In this study equilibration was attained by adding carbon through the Na2CO3 and NaHCO3 solutions in proportion. This brings me to answering the two last questions. We do not think our results apply to an addition of aqueous hydroxide because the specific chemical pathways are different. It may be comparable if after the addition, thorough bubbling is undertaken to ensure pCO2 level restoration. We believe though this study provides a baseline that will help the interpretation of results from other experiments set out to increase alkalinity together with Ca or CaMg (like in the set-out example, dolomite), or basically simulating any other specific carbonate mineral additions.

Line 295: The spike in GP:CR in the Δ1200 treatment (Figure 2D) just before phase II is interesting – especially as differences were seen in the contribution of the micro size class and PER% during phase II for this treatment relative to other treatments (Figures 3 and 5). Do the authors have a hypothesis as to what may have caused the spike in this treatment?

We thank the reviewer for pointing out the spike in GP / NCP in the Δ1200 treatment. With the available datasets, we have not been able to explain why this treatment behaved differently. Microplankton community composition and structure is the focus of a complementary publication that is currently in preparation. They will hopefully be able to better address the difference between the D1200 and the rest of the treatments.

Line 314 and throughout: Using "community composition" seems a bit misleading as we can't deduce how relative species abundances or phytoplankton functional group (PFG) relative abundances might have been affected within each size class, especially seeing as how there is PFG overlap among size classes (e.g., Pierella et al. 2020). As such, stating that "only minor changes in species composition were observed" (line 531) seems a bit premature. If the authors wish to use "community composition" to describe their results, they should note that potential changes in the relative abundances of species or PFGs within size classes might be masked.

We want to thank the reviewer and we agree. The title of the section was changed to Pico- and Nano- eukaryote abundances.

Lines 315-316: It's not clear what criteria were used to differentiate nanoeukaryote (1) and nanoeukaryote (2) populations. Figures 3 and 4 have the nano community as one group, but two populations are discussed in Section 3.3 and presented in Figure 6.

Further details describing this differentiation have been added to section 3.3. Figures 3 and 4 refer to size-fractionated 14C and Chla results. Since both Nano populations could be included within the Nano size range, no differentiation can be inferred using the two latter datasets.

Minor:

Line 51: "Process that is…" – this sentence is not complete and would flow better if it were joined with the previous sentence.

Amended

Lines 54 and 59: Should "carbon dioxide removal" and "negative emissions technologies" be capitalized here?

Amended

Line 55 and throughout: "…hard to abate emissions…" should be written as "…hard-to-abate emissions…". Here – and throughout the manuscript (e.g., line 61: "…carbonate- or silicate-based alkaline…") – phrasal adjectives are often incorrectly written.

Amended

Line 175: Incubation time is represented as "h_D" and "h_L" in the two equations rather than "T" as noted in the text (line 182).

Amended

Line 187: I couldn't find Carmeño et al. 2012 in the reference list – check that all references are included.

Amended

Line 284 and following: Panel D in Figure 2 is labeled "E" in the caption. GP:CR is also shown as "GCP over CR". The authors should verify that figure captions match plot labels and in-text references.

Line 290: The axes in Figure 3 are difficult to read.

Amended. The size of the figure was increased to fit a whole page.

Line 400: At the beginning of this section, the authors' use of "these results" make it a bit difficult to determine to which study they are referring. For example, is the sentence beginning at the end of line 405 referring to this study or the Ferderer et al. 2022 study?

Amended.

Line 410: Why is Figure 7 presented here instead of in the Results section?

Amended. The figure has been cited and moved up to the "3.4 Non-linear response vs no response"

Line 420: The sentence beginning with "In addition…" should be combined with the previous sentence.

Amended.

References

Ferderer, A., Chase, Z., Kennedy, F., Schulz, K. G., & Bach, L. T. (2022). Assessing the influence of ocean alkalinity enhancement on a coastal phytoplankton community. *Biogeosciences, 19(23), 5375–5399*.

Pierella Karlusich, J. J., Ibarbalz, F. M., and Bowler, C. (2020). Phytoplankton in the Tara ocean. *Annu. Rev. Mar. Sci., 12, 233–265*.