

Biogeosciences Discuss., referee comment RC1
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Comment on bg-2022-17

Anonymous Referee #1

Referee comment on "Assessing the influence of ocean alkalinity enhancement on a coastal phytoplankton community" by Aaron Ferderer et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-17-RC1>, 2022

REVIEW: "Assessing the influence of ocean alkalinity enhancement on a coastal phytoplankton community" authored by Ferderer, Chase, Kennedy, Schulz and Bach.

The study reported in this manuscript explored the effect of enhancement of seawater on a phytoplankton community in a coastal environment. The motivation for this study presumably originated from the possible increase in coastal alkalinity if enhancement of ocean alkalinity is in future used to remove carbon dioxide. As far as I aware, the effect of alkalinity increase on biology has not been experimentally tested, and thus is worth studying. To test their hypothesis, Ferderer et al. used a 55 L microcosm containing a coastal phytoplankton assemblage, and investigated its response to various alkalinity treatments over 22 days. I note their painstaking efforts over the course of experiment, and hope these come to fruition.

After carefully reading this manuscript I believe that providing technical comments to the authors may not be useful at the current phase of review. So I instead provide several major comments, which I hope the authors will consider seriously and address in any revision.

I have several reservations about the microcosm facility, and the interpretation of results obtained from experiments conducted in it. However, the microcosm facility did appear to be adequately managed in terms of vertical mixing and control of seawater temperature during execution of the experiment.

The first reservation I have is the duration of the reported experiment, which appears to have been too long given the size of the bottle. The experiment lasted for more than 20

days. Did the authors consider possible biases in species composition because of the bottom effect? I am not convinced that it is possible to interpret with confidence biology data obtained after 10 days of experimentation, and any conclusion drawn about changes in phytoplankton communities based on such data may well be erroneous. The bottle effect is well known, and can seriously bias results. The authors should provide convincing evidence that this effect has not influenced their results.

The second reservation I have is the validity of phytoplankton composition data obtained after the complete depletion of nutrients. Nearly all the N and P was depleted at day 6 (Si was depleted at day 8), but the authors reported phytoplankton composition changes for 20 days after nutrients had completely run out. I think the effects of alkalinity increase on phytoplankton composition should be evaluated under the ample nutrient conditions. Once the nutrients were depleted the phytoplankton would have been affected by both alkalinity change and nutrient constraints. These two variables may have in combination contributed to phytoplankton composition change. The authors should explain how they differentiated the effect of alkalinity change from the effects of nutrient constraints.

My third reservation is the validity of the three experimental treatments used. Case 1 was a control (no alkalinity change); Case 2 involved an alkalinity increase without equilibration with atmosphere CO_2 and so an initial pH > 8.6; and Case 3 involved an alkalinity increase and equilibration with atmospheric CO_2 , and so an initial pH was closer to the pH of the control. I am concerned about the design of the treatment cases 2 and 3. The goal of this experiment was to assess the effects of alkalinity increase on phytoplankton composition and elemental ratios during photosynthesis. To assess this the initial pH in cases 2 and 3 and the control should have been the same or at least similar. The authors needed to make sure that the effect of pH on phytoplankton composition and elemental uptake ratios in their experiment was minimized. I suspect that the difference in results between Case 1 versus Case 2 or Case 3 may have been because of pH differences rather than the alkalinity differences. In the reported experimental setting, the differences among the three cases might have arisen from pH differences rather than the alkalinity differences, or have been affected by their interaction. This is potentially a major problem with the study and interpretation of its results, so the authors should provide appropriate justification for their experimental design.

My fourth reservation concerns the C:N ratio. The authors measured the C:N ratio throughout the experiment, presumably to enable investigation of the effect of the three treatments on the elemental C:N ratio. If this is the case, only those measurements made under conditions of ample N and P availability are relevant. Once nutrients were depleted, nutrient limitation would more strongly constrain the C:N ratio rather than any change in alkalinity. Although the C:N ratio under the nutrient depleted conditions deviated considerably from the Redfield ratio, its impact is likely to have been minimal as the uptake of C and N by phytoplankton under ample nutrient conditions would have far exceeded those under the depleted conditions.

I appreciate this microcosm experiment was difficult and involved much efforts and time. However, to ensure that their efforts have produced correctly interpreted results that contribute to this important aspect of climate science, I believe they need to carefully consider and address the comments above, and those of other reviewers.

