

Biogeosciences Discuss., referee comment RC2
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Comment on bg-2022-17

Anonymous Referee #2

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-RC2>, 2022

The manuscript by Ferderer et al. describes a microcosm based experiment examining the impact of alkalinity enhancement on development of the spring bloom in coastal temperate waters, documenting differences between control and two alkalinity treatments. Differences between treatments are subtle, with the starkest impact of enhanced alkalinity on the diatom community. The authors carried out a 'simplistic' assessment of the plankton community using flow cytometry, and it seems that compositional and dynamical changes in the community may be at the heart of the response of the community – something the authors speculate on in the discussion as there are no clear trends or insights available without a deeper level of understanding of what was going on at the level of species and trophic interactions. The authors should be commended on their goal to further investigate the impact of alkalinity enhancement, with the results and recommendations for future studies of considerable value but also characteristic of an avenue of oceanic research which is only just beginning. However, there are some general comments to be addressed before this valuable study can be considered for publication.

What caused the spring bloom in the experiment? In the natural environment the spring bloom is initiated by increasing light availability in a slowly stratifying, nutrient replete water column as day length and mixing length scales change. However, the authors removed these factors from the experiment and placed their microcosms in static irradiance and mixing conditions – what happened in those first few days (1-3 days) in the microcosms? Did all the community acclimate to the new conditions at the same rate? At what point did the composition from flow cytometry analysis between the treatments differ or differentiate? It is a shame that the authors did not assess the compositional changes that occurred at the sampling site over a similar length of time to compare and contrast with their experimental treatments – acknowledging that the sampling site would not have encountered such static conditions.

Please do not misinterpret my comments, no experiment is without its problems or inherent assumptions and bias. Here the authors need to consider how their treatment of the community may have impacted on the initial dynamics of the organisms present.

Rather than discount the observations and insights made, the authors should caveat these in the wider context of the different 'stages' of the experiment and bloom development. Microcosms lasting three weeks are of considerable length, especially considering the 'small' volume involved (starting at 50 L with ~50% removed over time) – do the authors need this entire length of observations to confirm their conclusions about differences in community dynamics through the spring period of replete nutrient drawdown under enhanced light conditions and enhanced alkalinity?

Have the authors considered looking at the particulate C:Si ratios to further elucidate their point about compositional differences in the diatom component of the community as being important between treatments. Also note that Figure A5 is biovolume (μm^3) rather than biomass and does not make it clear whether there were clear species differences in the communities present – Scanning Electron Microscopy is more than adequate for assessment of compositional differences which would be enlightening in the context of the paper. Maybe some exemplar images could be included to highlight differences or an absence of differences?

The manuscript concludes that a deeper assessment of the community and its trophic dynamics is needed to reveal more of the impact and eco-physiological drivers of the responses of the community – this is an important point that should appear clearly in the abstract. Many perturbation experiments simplify their assessment of impact based on generalist bulk perspectives of the community (e.g., chlorophyll, particulate elements) only to conclude that a deeper understanding of the species present is actually needed – it would be beneficial if this was the starting perspective for future studies to ensure that the inner details needed are examined at the right scale.

The last line of the abstract is surprising and appears on first reading as a rather controversial conclusion, especially considering the negative impacts on diatom productivity observed and the potential for this to translate into negative impacts for marine ecosystems reliant on their provision of organic matter and essential elements. However, when reading the full manuscript this conclusion was put into much better context – this balanced and fully informed assessment of the statement should appear in the conclusion to ensure that no one reads the abstract (only) and takes home an unbalanced message.