

Biogeosciences Discuss., author comment AC2
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Reply on RC2

Aaron Ferderer et al.

Author 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-AC2>, 2022

Dear referee,

Thank you for your comments on our manuscript. We appreciate the time and effort you have dedicated to providing valuable feedback on our manuscript. Here are our point-by-point responses to your comments.

Comment 1:

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.

Response 1:

We would like to thank the reviewer for highlighting this point. We agree that the spring bloom was most likely caused by the enclosure of nutrient-rich water and exposure to sufficient light to enable a bloom. We acknowledge that enclosure experiments are never capable of representing the full complexity of a real aquatic system, which may compromise the assessment (Carpenter, 1996; see also our response to comment 1 from reviewer 1). From a technical perspective, all microcosms were kept in near-identical conditions, thus the initiation of the spring bloom and the mechanism responsible for this has likely not influenced the outcome of our results and the key message of this manuscript.

Figure 6 of the manuscript illustrates that phytoplankton communities began to diverge approximately two-four days into the experiment. This is with the exception of

picoeukaryotes which are known to be significantly affected by pCO₂ concentrations, as well as cryptophytes and *Synechococcus* which are known to be significantly impacted by enclosure (Schulz et al., 2017). We agree that in hindsight sampling of the Derwent Estuary, (location of initial sampling) would have been useful to assess bloom development at the sampling location. Unfortunately, due to the nature of the experiment and timing, there was only one person collecting data each day, thus we were unable to sample from the estuary as well as the microcosms but will undoubtedly consider this in future experiments.

The reviewer also asks the question "Did all the community acclimate to the new conditions at the same rate?". We are not sure how to measure acclimation but based on the flow cytometry data collected we can argue that most if not all assessed groups acclimated at similar times. Figure 6 illustrates an increase in the major phytoplankton groups (picoeukaryotes, nanophytoplankton and microphytoplankton) on day 2. Bacteria and cryptophytes appear to have had a greater starting stock explaining the increasing abundance trend seen for these groups since the initiation of the experiment. Finally, *Synechococcus* abundance appears to have been declining in response to increasing abundance of other groups, before rapidly declining during the bloom phase. Thus, we believe that the changes observed in the communities at the beginning of the experiment are within normal ranges and follow expected outcomes based on the surplus of nutrients. Furthermore, any differences relative to the natural bloom initiation would not affect our interpretation here as all microcosms were treated in the same manner (apart from the enhancement of alkalinity).

Comment 2:

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?

Response 2:

We would like to first advise the reviewer that although 55 L is indeed a small volume, we removed no more than 15 L from any microcosm over the experimental period for sampling. As we were working with relatively small volumes from the beginning of the experiment, we endeavoured to remove no more than needed each day to minimise potential effects on the phytoplankton community. Bottle effects always affect the outcome in experimental ecological studies, regardless of the size of the container (Carpenter, 1996; Duarte et al., 1997; see also response to comment 1, reviewer 1). Furthermore, to the best of our knowledge, there is no evidence to suggest that a specific duration is an ideal timeframe to minimise bottle effects (Duarte et al., 1997). Three weeks were chosen as we were able to cover a nutrient-induced phytoplankton bloom and the subsequent post-bloom phase with sufficient data points to obtain meaningful insights. We do not deny that bottle effects likely had an impact however bottle effects occur in all micro and mesocosms. To address the reviewers concerns we will add a statement

emphasizing this: (also see response to comment 1, reviewer 1)

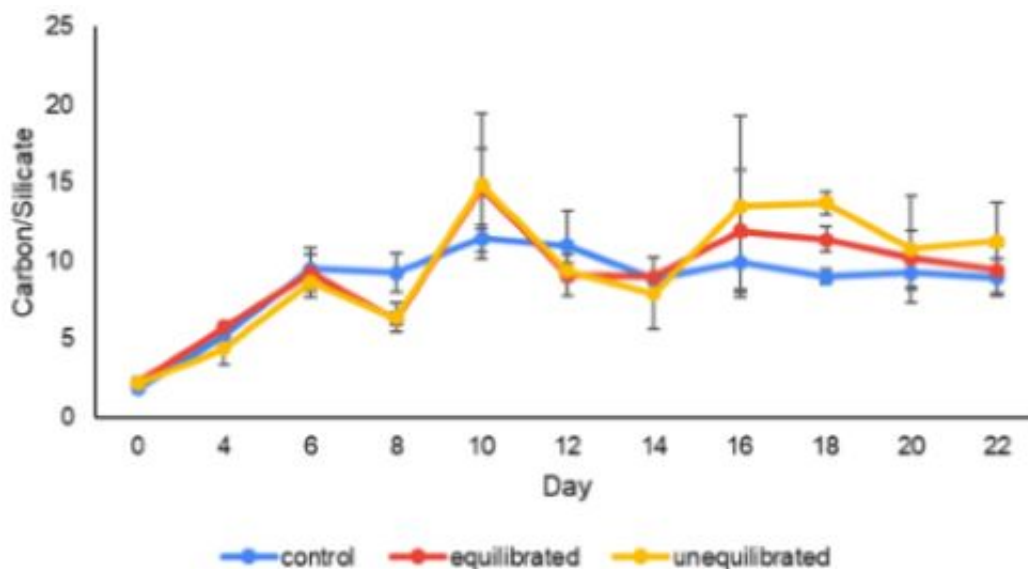
“Nevertheless, despite some potential advantages, we acknowledge and are fully aware that our microcosm setup cannot reproduce the full physical (or chemical/biological) complexity of nature (Carpenter, 1996). Enclosures of any type will very likely induce so-called bottle effects (Bach and Taucher, 2019), which can alter the observed community succession and therefore affect the transferability of the outcome to natural (non-enclosed) communities (Carpenter, 1996). While this is a general limitation of these kind of experimental studies, we stress that bottle effects would occur in all replicates so that the comparison between control and treatments (as done in our study) is valid.”

Comment 3:

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.

Response 3:

Thank you for highlighting this point. We did look at the stoichiometric ratios between carbon and silica and found no significant differences or discernible patterns between the treatments (see figure below). We thought at the time it would therefore be better to exclude this figure from the article as we already had a large number of figures and did not wish to add any figures depicting insignificant differences. However, we will add a statement to illustrate that we have considered the ratio of C:Si. “Furthermore, ratios of carbon to silica did not differ between treatments across the experimental period supporting SEM count data (data not shown)”.



Comment 4:

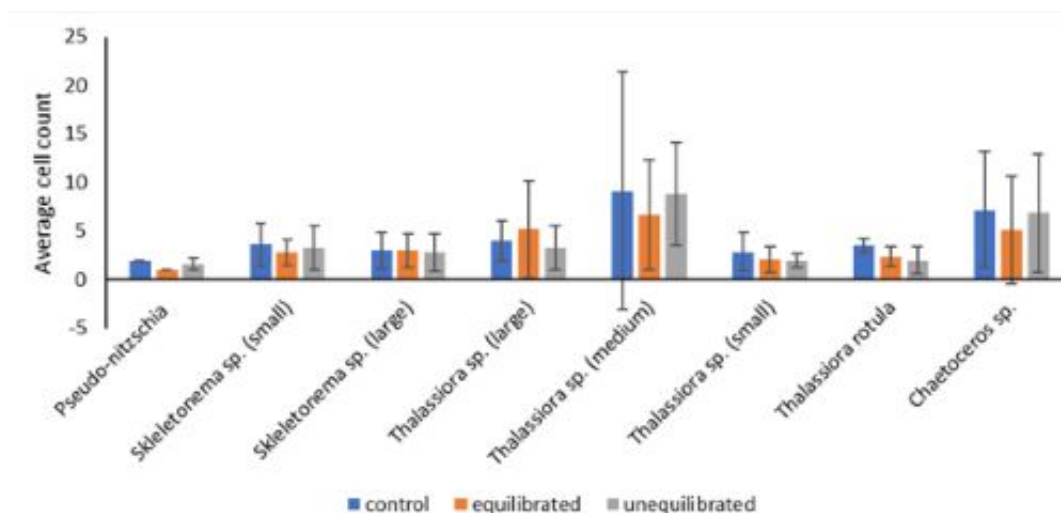
Also, note that Figure A5 is biovolume (μm^3) rather than biomass and does not make it

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?

Response 4:

Thank you for highlighting this, we have now changed Figure A5 to Biovolume, not Biomass. We did assess SEM images for compositional differences (peak bloom only) and found little to no differences (see below). At the time of analysis, it was decided that further investigation into the differences was not necessary for this manuscript as our primary goal was to identify the presence or absence of an effect of OAE on a functional type level (not yet in full taxonomic detail, which would have required more resources that were not available to the project). We will clarify this in the discussion: “However, there were no clear differences in the composition or biovolume of the diatom community between the control and alkalinity treatments on day 6 (Fig. A5)”.

Thank you for the recommendation of including images, we agree that they can be very informative and an appealing way of presenting the information. As we took SEM images to assess biovolume and cell counts/ml many of the images are not (in our opinion) visually appealing to the readers. Furthermore, we have already included five supplementary figures and a supplementary video. As such we decided in this instance not to include any SEM images as we believe they will not add a substantial amount of information to the manuscript. Therefore, we currently have not included any SEM images in the revised manuscript.



Comment 5:

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.

Response 5:

We agree and will add the following conclusion to account for this valid concern as the last sentence of the abstract: "We note, however, that more detailed and wide-spread investigations of plankton community responses to OAE are required to confirm or dismiss this first impression."

Comment 6:

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.

Response 6:

Thank you for your comment. We agree that this final sentence could be seen as somewhat controversial, especially due to the word "justifiable", which has policy implications. As such we will make subtle changes to the wording of this sentence "Altogether, the inadvertent effects of increased alkalinity on the coastal phytoplankton communities appear to be rather limited relative to the enormous climatic benefit of increasing the inorganic carbon sink of seawater by 21%." We stand to the point that the climatic benefits of a 21% enlargement of the marine CO₂ sink are incredible, and that the effects on the plankton community must therefore be put into perspective.

References

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