Review – Faucher et al. Growth response of *Emiliania huxleyi* to ocean alkalinity enhancement

The manuscript of Faucher et al. investigates the response of the coccolithophore *Emiliania huxelyi* (strain B92/11) to increased alkalinity (and associated decreased CO_2 and HCO_3^- concentrations) in a culture experiment. The motivation for this study is very timely and important, given that the urgency of the climate crisis is fuelling intense focus on emissions mitigation technologies. One such approach is Ocean Alkalinity Enrichment, which may also mitigate ocean acidification. As these OAE experiments are already in trial phases, it is important to build an evidence base for the biological impacts of alkalinity enrichment on key marine organisms. The coccolithophore species *E. huxleyi* is often used as a model species for calcifying phytoplankton and is a common species in some of the regions where OAE trials have already been proposed or undertaken, e.g., in the North Atlantic shelf seas, and is therefore a very suitable candidate for the study objectives.

The data presented by Faucher et al. specifically address alkalinity perturbations rather than manipulation of other aspects of the carbonate system, which have been the focus of previous work. They identify a growth rate response to alkalinity enrichment that leads to decrease POC and PIC production, identifying a threshold value above which the response is greatest. The methods, resultant data and the manuscript are of high quality and suitable for publication following minor revisions. I have made some general suggestions for additional details that can be included to provide more context for the magnitude and rate of alkalinity enhancement that may be realistic in OAE experiments. I also think that the discussion of the manuscript would benefit from a section discussing in a bit more detail the implications of the results for guiding recommendations for alkalinity enhancement trials and/or monitoring the impacts of OAE on the biosphere and/or highlighting future research priorities.

General comments:

The Introduction gives a good overview of previous studies that have investigated the response of phytoplankton and coccolithophores specifically to changes in carbonate chemistry conditions. But given the wide range of carbonate chemistry parameters that have been manipulated across published studies, I think it would be helpful in the introduction to very explicitly state what (if any) impact an increase in alkalinity has on pH, pCO_2, DIC, and saturation state, in addition to ion concentration, so that it is very clear to the reader why pCO2-only experiments or pH-only experiments do not capture the same response as an OAE scenario.

The experiment is designed to inhibit equilibration of the culture media with the atmosphere after NaOH addition. In the field, how quickly would the surface ocean equilibrate with the atmosphere following OAE? I.e., what is a likely duration of time for the phytoplankton community to be exposed to increased alkalinity conditions, such as imposed in your experiment? Would there be a progressive decrease in alkalinity following OAE such that populations would be exposed to a gradient of alkalinity over a period of time? Or would phytoplankton populations be more likely to experience an abrupt and large increase in alkalinity that progressively equilibrates over some time? Whilst the specifics of these real-world OAE test settings are beyond the scope of your experimental study (and the details may not be available for industry reasons), some brief context of the reality of (proposed) OAE in terms of timescales and magnitude of alkalinity change (prior to the discussion where it is mentioned generally in Lns 185-189 but without specifics) would be useful context for your choice of experimental conditions and scaling from your results to real-world applications.

You mention in the Introduction that calcification consumes alkalinity but is also a CO_2 source, thereby offsetting CO_2 drawdown through OAE (Lns 51-52). However, you don't mention this process further in the discussion. Is it not relevant at the cell concentrations you are using (although this might therefore mean it is not relevant outside of the lab either as cell concentrations in the 'real world' are also low much of the time), because you have no headspace equilibration, or because the PIC:POC in your experiments in <1? Perhaps you can loop back to this aspect in the discussion, especially as calcification as a net sink or source of CO_2 is a common theme in coccolithophore work.

Given the obvious need for lab experiments with different phytoplankton (and other marine organisms) to provide evidence to guide safe OAE practices (which is a motivation for the research) and monitor the biological impacts of any OAE project, I feel that the manuscript should include at least a brief section in the discussion that interprets these data in the context of what is currently planned for OAE (e.g. the magnitude of alkalinity changes, timescales involved, spatial extent of OAE treatments that are likely, geographical regions of focus and how they overlap with where *E. huxleyi* is most abundant in phytoplankton communities) so that you can make some initial recommendations. This could expand on Lns 225-230, which are currently very generalised and brief. It might be that this sort of discussion is planned for future manuscripts but interpreting the dataset beyond solely discussing the response of growth rate, POC and PIC production to elevated alkalinity would link back to your initial motivation for the experiments and make the article of greater interest to a wider audience. For example, is the threshold of 600 µmol kg^{-1} addition comparable to the expected change in alkalinity likely to be used in OAE, or is that much larger or smaller than likely to be used? Would your recommendation therefore be that because we now have evidence that additions beyond 600 µmol kg-1 will alter the productivity of *E. huxleyi*, OAE trials should not exceed this? Or that changes in *E. huxleyi* productivity/abundance should be monitored during OAE trials, especially in areas like the UK shelf seas and the North Atlantic where *E. huxleyi* is a very common component of the phytoplankton community? Do your results have any relevance for OAE that uses something other than NaOH (maybe other alkali that have the same impact on carbonate chemistry), or does that require further experimentation, at least for confirmation? Are your results useful for constraining biogeochemical models that investigate the impacts of OAE on the marine system?

In your PIC:POC data, there are significantly lower values for TA conditions 2433, 2482, and 2499 but you don't discuss this in the text at all – is there a likely explanation for this?

Specific comments:

Ln 26: I think "adapted" should be "adopted" here?

Ln 34-35: you could specify one or two examples of which minerals you are referring to here.

Ln 40: could you contextualise "gigatonnes of carbon" with respect to typical annual emissions or oceanic carbon uptakes, or similar, for context? e.g., CO_2 absorption equivalent to 2% current annual anthropogenic CO_2 emissions, or whatever it actual is.

Ln 76: I think this conclusion that *E. huxleyi* is the 'sole coccolithophore species' not showing a neutral effect to alkalinity, whilst accurate based on their analysis, comes across as a bit too decisive considering that the study in question only synthesised a limited amount of data (5 studies I think) from 5 species. I would suggest rephrasing to something more like "Of the five coccolithophore species included in the meta-analysis of Bednaršek et al. (2024), *E. huxleyi* was the only species where calcification did not show a neutral response to increased alkalinity."

Ln 87: it would be useful to mention where this strain was isolated, in addition to the culture collection information.

Ln 95 (also Ln 79 in the Introduction): NaOH is presumably one of several options for changing alkalinity chemically (I have seen proposals using magnesium hydroxide for instance). Was there a reason that you used NaOH specifically over other options?

Ln 205: should be "CCMs", plural

Ln 205: Based on carbon isotopic composition of alkenones, there is evidence that CCMs may have evolved in coccolithophores as early as the Miocene (Bolton and Stoll 2013), i.e., significantly earlier than the first appearance of *E. huxleyi*. I would suggest replacing *E. huxleyi* with Noelaerhabdaceae and rephrasing the sentence so that the evolution of CCMs sound less like a sentient choice, e.g. something along the lines of "The timing of CCM evolution may have been a response to declining atmospheric CO_2 concentrations during the Neogene, thus enabling Noelaerhabdaceae like *E. huxleyi* to maintain competitive growth rates under lower CO_2 levels".

Ln 213-214: there is an incorrect placement of a comma and a missing comma in this sentence. It should read "In principle, the alkalinity additional increased $\Omega_{calcite}$ (Table 2), which…"

Ln 216: typo – exchange "weather" with "whether".

Ln 217-219: Can you clarify what you mean here, linking coccosphere formation with growth rate and reduction in cellular PIC? With a lower growth rate, there would be on average a longer duration between successive cell divisions for the generation of new coccoliths – might this not then reasonably be expected to increase cellular PIC because there is more time for more coccoliths to be produced each cell division cycle? Or here do you refer to PIC production rather than cellular PIC?

Ln 226: can you suggest here how long this period might be – are we talking hours/days/months etc. Are there any foreseeable implications of this conclusion for when alkalinity enrichment could/should be carried out (i.e. time of year) to minimise the impact on community composition?

Ln 227: is there evidence from the literature to indicate which other phytoplankton groups, e.g., diatoms, might have a competitive advantage then in this scenario? E.g., the study of Gately et al. (2023) that also investigated *Chaetoceros*.

References: I haven't proof-read the references in any detail but can see that many need final formatting and you should check the correct referencing format for EGUsphere pre-prints.

References mentioned in this review:

Bednaršek, N., Pelletier, G., Van De Mortel, H., García-Reyes, M., Feely, R., and Dickson, A.: Unifying framework for assessing sensitivity for marine calcifiers to ocean alkalinity enhancement identifies winners, losers and biological thresholds – importance of caution with precautionary principle. EGUsphere [preprint], https://doi.org/10.5194/egusphere-2024-947, 2024.

Bolton, C. T and Stoll, H. M.: Late Miocene threshold response of marine algae to carbon dioxide limitation, Nature, 500, 558-562, https://doi.org/10.1038/nature12448, 2013.

Gately, J. A., Kim, S. M., Jin, B., Brzezinski, M. A., and Iglesias-Rodriguez, M. D.: Coccolithophores and diatoms resilient to ocean alkalinity enhancement: A glimpse of hope?, Science Advances, 9, eadg6066, https://doi.org/10.1126/sciadv.adg6066, 2023