

Reply to Review by Justin Ries

Reviewer comments are displayed in italics, our responses in roman font.

Reviewer comment:

I commend the authors for writing a concise and informative introduction to their Best Practices Guide For Ocean Alkalinity Enhancement Research, and for organizing the production of a guide that will undoubtedly prove useful for researchers, as well as practitioners, in this nascent field. My comments and suggestions on this introductory chapter are below.

We thank the reviewer, Justin Ries, for his helpful and constructive review and its positive assessment.

Line 29: 'net-zero requirement for avoiding further temperature rise' – should clarify that this is 'further temperature rise beyond the 1.5 – 2 deg IPCC target' (not beyond the present-day mean global temperature)

This sentence has been rewritten.

Line 34: The discussion and corresponding figure showing how to achieve IPCC's target of net zero CO₂ emissions by 2050 is useful for illustrating the need for CDR in addition to emissions reductions, but it would be helpful (and potentially compelling for skeptics) to see the corresponding mean atmospheric pCO₂ and mean global temperatures that correspond to that target emissions trajectory, since those relationships are the basis of the authors' rationale for pursuing CDR in the first place. Otherwise, the non-specialist may not grasp what achieving 'net zero by 2050' means in terms of global climate change. Perhaps include another panel above the net zero figure showing the corresponding changes in mean global pCO₂ and temperature over the same interval.

Good point. We will modify the figure and consider adding a panel with the corresponding temperature changes.

Line 77: should clarify here and elsewhere whether the 2/1.5 deg C warming target is relative to pre-Industrial mean global temp or present-day mean global temp.

Thanks, this clarification is now included.

Line 100: The authors should differentiate between particulate inorganic carbon and dissolved inorganic carbon when referencing 'inorganic carbon' in this sentence (presumably they are referring only to DIC): 'The ocean holds more than 50 times as much inorganic carbon (in the form of dissolved inorganic carbon) as the pre-industrial atmosphere'

Thanks, this is now clarified.

Line 104: should 'marine CDR' instead be 'marine CDR by OAE' here?

In this section 2, we still refer to all marine CDR and only focus on OAE in section 3. Therefore we decided to leave the statement as is.

Line 125: should differentiate between the time needed for pCO₂ of air and seawater to fully vs. partially equilibrate. It is true that air/sea can take years to fully equilibrate, but the equilibration will be an inverse exponential function of time, meaning a disproportionate share of the equilibration will occur in the beginning of the equilibration interval. This is an important but often overlooked distinction that has important implications regarding the perceived challenges of quantifying CDR by OAE.

Good point, thanks! The text has now been reformulated to read 'Air-sea gas equilibration of CO₂ can take months to years (Jones et al., 2014) and may pose specific challenges to MRV (He and Tyka, 2023). However, along the path to equilibration, air-sea CO₂ fluxes approach zero following, for otherwise constant environmental conditions, an inverse exponential function, and a disproportionate share of the total CO₂ flux typically occurs at the beginning of the equilibration period.'

Line 131: It seems that the definition of marine CDR should be expanded from 'OAE qualifies as marine CDR if CO₂ is transferred directly from the atmosphere into seawater' to 'OAE qualifies as marine CDR if CO₂ is transferred from the atmosphere or seawater into stable carbonate or bicarbonate ions in seawater', as both processes will result in the eventual drawdown of CO₂ from the atmosphere. Otherwise, we will miss an important and efficient pathway in CO₂ removal. Likewise, CO₂ removal from the atmosphere alone is not sufficient for CDR, as increasing the pCO₂ of the atmosphere through increased CO₂ emissions would increase the flux of CO₂ from the atmosphere to the ocean, but surely this should not constitute marine CDR (as the alleviation of the atmospheric CO₂ pressure would cause off-gassing of the dissolved CO₂ back to the atmosphere unless balanced by alkalinity addition). This point is also illustrated by the author in an earlier paragraph, where they state: 'Alkalinity enhancement results in the consumption of protons, a corresponding increase in the pH, which results in a decrease of the partial pressure of CO₂ in seawater. If applied to the surface ocean, and depending on the initial air-sea CO₂ gradient, it would promote CO₂ uptake from - or lessen CO₂ release to - the atmosphere, in both cases leading to a net reduction in atmospheric CO₂ at the expense of an increase in the oceanic carbon pool.' In the case that the flux of CO₂ from the ocean to the atmosphere is lessened by OAE, this would not satisfy the authors' current requirement that 'CO₂ is transferred directly from the atmosphere into seawater', but would instead reduce the rate that seawater CO₂ is released to the atmosphere, thereby resulting in a theoretical 'net reduction in atmospheric CO₂' – which should qualify the activity as successful marine CDR. Perhaps a more useful framing for CDR is the transfer of C from shorter residence time reservoirs (atmospheric CO₂, seawater CO₂, terrestrial biomass, marine biomass in mixed layer etc.) to longer residence time reservoirs (bicarbonate ion reservoir, carbonate ion reservoir, terrestrial and marine biomass transported to deep ocean below mixed layer, etc.) (c.f., Prentice, I. C., 2001, The carbon cycle and atmospheric carbon dioxide. Climate change 2001: the scientific basis, Intergovernmental panel on climate change. hal-03333974) or, more colloquially, transferring C from the 'fast C cycle' to the 'slow C cycle', as this encompasses the ultimate goal of marine CDR – i.e., net reduction of atmospheric CO₂, regardless of the strict and not necessarily relevant 'transfer of CO₂ between ocean and atmosphere'.

Many thanks for this comment! We agree that the wording was sloppy and that CDR has to be defined via 'additional' CO₂ that is removed from the atmosphere, and into the ocean in the case of marine CDR. We do not agree that CO₂ removal from seawater automatically qualifies as CDR, which is only the case when a CO₂ flux from the atmosphere to the ocean is induced. We have added the word 'additional' to exclude the CO₂ that transfers naturally from the atmosphere to the ocean from counting as CDR.

We decided to separate the discussion of residence times from the definition of CDR, as there is a continuum of residence times (e.g. Siegel et al., ERL, 2021) and with a risk of introducing ambiguities when partitioning into 'slow' and 'fast'.

Line 143: 'leakage' by OAE-induced precipitation (or reduced dissolution) of CaCO₃ at the seafloor (or really anywhere in the water column below the mixed layer) is probably not relevant over climate-relevant timescales because it will take 100-1000s of years for those waters to return to the surface and re-equilibrate with (i.e., offgas CO₂ to) the atmosphere, just as shoaling of the carbonate compensation depth in response to CO₂-induced OA will not sequester anthropogenic CO₂ over fast enough timescales to prevent warming (hence, the bind we are in).

We agree that the leakage referred to here is a slow process, but time scales of 100s or 1000s of years are still climatically, and hopefully societally, relevant and need to be considered in decisions made today. We have added the following text to describe the situation in a more comprehensive way:

'Possible leakage effects via impacts of OAE on pelagic calcifiers are uncertain (Bach et al., 2019), and feedbacks via changes in dissolution and preservation of carbonates on the sea floor operate on timescales of hundreds to thousands of years (e.g. Gehlen et al., 2008). While there is little indication that leakage is a major concern for OAE on shorter than centennial timescales, a quantitative assessment of leakage across the spectrum of timescales is lacking.'

with references to

- L. T. Bach, S. J. Gill, R. E. M. Rickaby, S. Gore, and P. Renforth. CO₂ removal with enhanced weathering and ocean alkalinity enhancement: Potential risks and co-benefits for marine pelagic ecosystems. *Frontiers in Climate*, 1:7, 2019.
- M. Gehlen, L. Bopp, and O. Aumont. Short-term dissolution response of pelagic carbonate sediments to the invasion of anthropogenic CO₂: A model study. *Geochemistry Geophysics Geosystems*, 9, 2008.

Line 150: should probably add 'so long as any CO₂ emitted in their production (e.g., Ca(OH)₂ or Mg(OH)₂ produced through calcination of CaCO₃ or MgCO₃, respectively) is accounted for'

We added the sentence 'Employing these for OAE would require proper accounting of any CO₂ emitted in their production (e.g., Ca(OH)₂ or Mg(OH)₂ produced through calcination of CaCO₃ or MgCO₃, respectively).'

Line 161: Was Albright et al (2016) the first ocean acidification field experiment (see Hall-Spencer et al. 2008 field experiments using volcanic vents, etc.)? Or just the first field experiment to modify seawater pH through alkalinity addition rather than direct pCO₂ manipulation? May also be worth mentioning that insight into impact of OAE on marine organisms can be gained from past research by the shellfish industry investigating the utility of so-called 'sweetening' the water through addition of mainly soda ash (Na₂CO₃), a practice utilized in shellfish hatcheries for decades, and also in the academic and industrial fields of 'river liming', which dissolved primarily CaCO₃ and dolomite in higher latitude watersheds to offset the effects of acid rain (due to NO_x and SO_x emissions) in the 1960s and 1970, but is still practiced today in Canada and some Scandinavian countries, among other places.

Thanks, very good points! We rephrased the Albright et al. (2016) experiment as 'first OAE field experiment carried out in the context of ocean acidification research' as this was, to our knowledge, the first experiment where alkalinity was added for a scientific experiment in the field.

We also took up the reviewer's suggestion and added the following text: 'Insight into possible impacts of OAE on marine organisms can be gained from past research by the shellfish industry investigating the utility of so-called 'sweetening' the water through addition of mainly soda ash (Na₂CO₃), a practice utilized in shellfish hatcheries for decades, and also in the academic and industrial fields of 'river liming', which dissolved primarily CaCO₃ and dolomite in higher latitude watersheds to offset the effects of acid rain in the 1960s and 1970, but is still practiced today in Canada and some Scandinavian countries, among other places.'

Line 173: need more concise phrasing than 'enhancing technological readiness'; perhaps 'developing (or implementing) scalable methodologies'

Thanks. We added 'start-ups working on enhancing technological readiness and developing scalable methodologies'

Line 184: perhaps change 'in a situation where' to 'at a time when'

done

Misc:

Confirm whether 'Ocean Alkalinity Enhancement' and 'Carbon Dioxide Removal' should be capitalized in title and throughout manuscript.

Agreed and capitalized throughout the text.

Use of term 'monitoring, reporting and verification' in abstract without defining the term may be confusing to readers, as the phrasing really only has meaning when the three terms are defined and understood in aggregate.

Agreed that this might be confusing, in our view primarily the 'reporting' part. Monitoring and verification should be self-explanatory, and we have kept these two terms in the abstract. In addition, there is now a new more detailed section defining MRV included in the manuscript.

Respectfully submitted,
J. Ries