



EGUsphere, referee comment RC2
<https://doi.org/10.5194/egusphere-2022-683-RC2>, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on egusphere-2022-683

Anonymous Referee #2

Referee comment on "Limits and CO₂ equilibration of near-coast alkalinity enhancement" by Jing He and Michael D. Tyka, EGU sphere, <https://doi.org/10.5194/egusphere-2022-683-RC2>, 2022

This is an interesting, well-written article that explores a number of the potential geophysical limits on ocean alkalinity enhancement (OAE) using a high-resolution global circulation model. The authors find that imposing conservative limits on the change in pH and aragonite saturation station due to coastal OAE still permits potential global carbon dioxide removal (CDR) on the scale of several GtCO₂/yr under current climate conditions. Their simulations further highlight how the CDR efficiency differs regionally due to equilibration kinetics and surface water residence time. My main reservations with the manuscript are given below. If these can be adequately addressed, I would be happy to recommend publication.

Main points

- The choice of pH and omega anomalies as opposed to absolute values when constraining OAE rates: Given the authors justification that the CO₂ system constraints they impose on potential OAE are to avoid calcite precipitation, why use pH and omega anomalies as opposed to absolute values? I find this choice problematic given both the log-scale of pH and the non-linear response of the CO₂ system (including omega) to alkalinity enhancement. For example, higher pH waters exhibit a smaller change in pH for the same change in [H⁺] just due to the log-scale. While lower omega waters will exhibit a smaller change in omega for the same increase in alkalinity (and the opposite is true of pH). I don't want to necessarily force the authors to rerun computationally expensive simulations but feel this decision to use anomaly thresholds needs to be better justified. Perhaps the authors could assess the extent to which permissible OAE is dependent on background omega and pH values. I also feel that they are missing a proper comparison between the use of the pH versus omega threshold.
- The dependence of results on current climatic conditions: The authors say little about how dependent their results are on current climatic conditions. I would be particularly interested to see how the permissible OAE rates and associated CDR differ under a much higher atmospheric CO₂ concentration. Presumably permissible OAE rates are higher (at least when using the delta omega threshold) and the impact on pCO₂ (and in turn CDR) will be far greater per unit alkalinity added. Such a simulation may be beyond their scope but I still think some discussion of how their results might change

as the anthropogenic carbon content of the ocean increase is required.

- Figures: The current content and layout of many figures in the manuscript makes for quite painful reading. So much detail is often squeezed into the multi-panel figures that often nothing is legible/understandable without using a high zoom in a pdf viewer. I suggest the authors simplify their figures, reducing the number of panels and repetitive information. All figure text should conform to a minimum font size.

Minor comments

L7-14 The first time I read this I thought you were confusing OAE and CDR. I think you need to introduce the concept of OAE thresholds (and why they may be important) before you mention the steady state OAE rate which most readers (myself included) will assume you are free to choose.

L21 Isn't it the CDR potential that is lost not the OAE potential? Is the transport of alkalinity to depth actually facilitates higher surface addition of alkalinity addition without exceeding a given threshold.

L32 CO₂ doesn't dissolve as bicarbonate (although most ocean DIC is in the form of bicarbonate).

L66 "moieties" is not the correct term to use here as it typically refers to part of a molecule. You just mean the sum of the aqueous forms of inorganic carbon.

L91 lat-lon grid?

L104 avoid "moieties"

L119-120 provide the units of DIC and Alk used otherwise it's unclear that this is a molar ratio.

L121-122. Can the authors say more here about this 0.8 value and what effects it?

L144-145 The movement of DIC is controlled by the ocean physics not these 5 biogeochemical tracers which are presumably used to compute the ocean CO₂ system.

L147 Is this freshwater flux from rivers?

L150 uatm are units of partial pressure. Do you hold the concentration or partial pressure of atmospheric CO₂ constant ie can local changes in atmospheric pressure influence gas exchange?

L154 More detail on how the imported wind speeds are used to calculate gas exchange would be useful here.

L234 Any rationale for January pulses?

L240-241 Not sure this sentence should be a distinct paragraph.

L265-277 Perhaps make it clear that you're only considering the financial and not carbon cost of transport.

L278 Results "and discussion" – there is a lot more than results in this section.

Figure 1. A very busy figure. Fonts are too small to be legible. Axes labels are missing on panels e/f/g.

Figure 2. Another very difficult figure to read. Suggest reducing the number of panels and enhancing font size. Some alkalinity flux axes labels for are missing in panels a/b.

Figure 2 legend, Line 2- I would call this CDR potential not OAE potential Given the different research questions this manuscript addresses I think this distinction needs to be clear.

Figure 3. Panel labels b/c are incorrectly ordered. Panels e/f/g are not interpretable at their current size.

L326 Largest per unit area fluxes. The globally aggregated fluxes are shown to be higher for wider strips.

L331-332 This sentence is unclear, please clarify.

L335 Unclear where this 25% comes from. Fig 3e/f/g appear to show 8% and 30% declines in the 200km and 400 km simulations.

L341. Do the authors expect this length scale to vary considerably regionally?

L351-352 clarify these are molar ratios.

Figure 4. Reiterate the units are mol/mol.

Figure 5. Another busy figure. The "surface pCO₂" label is unnecessary and conflicts with the surface pCO₂ deficit label.

Figure 5. legend. I think some of this legend may be misleading. Presumably change in the surface pCO₂ over time is also influence by mixing/circulation/biology (and not only gas exchange).

L371 Some local deficits >1uatm are shown in Figs 5bii and 5cii.

L405 mol/kg?

L413 I would call this a high-resolution global ocean model (particularly compared to most global obgc models and papers that have simulated OAE previously).

L421 Shouldn't the R in MRV be for Reporting?