

Review

With the presented manuscript the authors address an important question about the feasibility of various Ocean Alkalinity Enhancement (OAE) efficiency matrices. For that they deploy a fully coupled Earth System Model and simulate OAE in various European near coastal regions under the SSP3-7.0 emission scenario. One of the key findings is that the efficiency defined as the ratio of globally integrated increase in dissolved inorganic carbon (DIC) and total alkalinity is not a feasible metric, because of high uncertainty caused by natural variability in the simulations. They have also explored some interesting OAE efficiency definitions based on plume tracking of alkalized water masses and determining the domain where the increase in oceanic CO₂ flux is statistically significant. They conclude that efficiency calculated based on such metrics are of much more robust nature if the oceanic conditions at the deployment regions, natural variability of various parameters in these regions, and impacts of global climate modes are considered.

While the manuscript provides a much-needed insight in regards to the various OAE efficiency metrics, there is still scope of improvement. The text can be streamlined a lot and proper reasoning and logical explanations can be added at the same time. Currently, I find certain conclusions drawn in the paper to be not well supported or well written. Hence, I would recommend major revisions after which the manuscript can potentially be considered for publication. I have noted major and minor comments line-wise in the text below.

Comments:

Line 4-6: There seems to be a bit of disconnect between the sentences here. You end the sentence at line 5 with “global ocean inventories for dissolved inorganic carbon and alkalinity are not a feasible approach”. Immediately the next question which comes to a reader’s mind, then what is feasible and why is alkalinity and DIC not feasible. You explain that in the text below, but the connectivity between the sentences is not that explicit. It reads more like listing one finding after the other from the manuscript. I would suggest to restructure the part and improve the logical flow of the sentences here.

Line 14: Update to the latest version of the Global Carbon Budget.

Line 21-22: I totally understand the argument about the limiting warming below 2°C above the preindustrial levels. But in my opinion, it should also be made clear somewhere in the introduction that current emission trajectories project that the warming will potentially cross the 1.5°C (coming few decades) threshold and limiting it to 2°C is a substantial challenge. With the current generations of OAE research, simply stating that it is an augmentative method for achieving the goal of the Paris Agreement, is too simplistic and can be misleading with the background made clear first.

Line 25: Add proper citation here.

Line 26: I think it should be “sequestration of **additional** CO₂”.

Line 29-30: Here you describe the OAE efficiency as ratio of volume-integrated increases in the DIC and cumulative added alkalinity. But some literature that is cited in the next lines (e.g. Nagwekar et al., 2024), do not use cumulative added alkalinity, but the increase in alkalinity as simulated by the model. Please cross verify and amend the text accordingly.

Line 43: increases → increase

Line 52-53: It would be good if you already mention here which high CO₂ emission scenario is being used for the simulations.

Line 80-81: It would be good to mention the per m² of added alkalinity. Is it the same for all the sub-regions in the European coastal setup?

Line 87: Ca(OC)₂ → Ca(OH)₂

In the Methods section in general, I missed the information about the spin-up strategies you followed to carry out the simulations. I would suggest to discuss it briefly here.

Line 82-83: Discussion about the emission scenarios can be grouped together, line 72-73 can be moved here.

I understand having simulations under a high emission scenario (in this case SSP3-7.0) gives a useful insight, but nonetheless as the authors have mentioned in line 82-83, that lower emission scenarios (SSP2-3.4 or SSP2-4.5) are closer to the reality. Hence, in the text here as well as in the discussion section, I do not find any good explanation or rationale as to why SSP3-7.0 emission scenario was chosen to begin with. It would be good to make that point very clear.

Furthermore, as this is an ESM setup it is a good tool to investigate emission scenario sensitivity of OAE, and would like to make a suggestion to also carry out simulations in the SSP2-4.5 emission scenarios if possible. Such simulations would potentially also strengthen your arguments in the result section about the effect on phase shifts in the large-scale climate pattern El Nino. As the manuscript discusses the very relevant questions about feasibility of various OAE efficiency metrics, in my opinion this serves as a right opportunity to add in more insights and confidence to the results if an closer to reality emission scenario was considered for OAE simulations, as this will be a value input in context of Monitoring, Reporting, and Verification (MRV) for OAE.

Line 89-90: The sentence can be moved above where you describe all other OAE simulations.

Line 95 and Equation 1: Based on the equation and the definition of the OAE efficiency her, the ΔAlk is the difference between model simulation alkalinity between OAE and the BASE simulation. But in the introduction, you mentioned it to cumulative added

alkalinity (line 30). I would suggest it to make it clear here and, in the text, further wherever applicable.

Line 99: Regarding the threshold-based budgets, why do you only consider the horizontal transport of the alkalized waters? What about vertical mixing? It would be good to add an explanation about this and potentially discuss the implications of considering just the horizontal transport of the waters.

Line 120-121: Please specify the sign convention for CO₂ flux used in your model setup. Usually, I have come across OAE having higher CO₂ flux than BASE, hence at first it was a bit confusing here.

Line 138-139: From Fig. S1, it looks like positive is increase in the parameter for OAE compared to the BASE and negative is decrease in OAE than in BASE. But previously as mentioned above (line 120-121) it seems to be other way round (i.e. increase in CO₂ flux for OAE was negative). I suggest to be very consistent with sign conventions and use uniform sign conventions for all the parameters that are investigated here. It will improve comparability of the parameters in the manuscript.

Line 146-148: I don't think this is true. Even without feedback effects (e.g. in ocean only models), the biogeochemistry will result in difference between added alkalinity and alkalinity further simulated by the model. I would like to revise this statement or give proper citations for the statement.

Line 149-150: Why exactly does the EU addition of alkalinity result in ΔAlk being almost identical to ALK_{ADD} and ΔDIC being proportional? I mean compared to background variability on a global scale still the EU addition is a smaller area. And why is the effect of climate feedback not apparent in this case? Is it solely dependent on the amount of alkalinity added? A bit more discussion and clarification would be needed here.

Line 154 onwards: This text about ENSO is a bit confusing to me. In general, El Nino occurs when weakened trade winds allow warm water to shift eastward, raising sea surface temperature in the central and eastern tropical Pacific and suppressing upwelling. Although warmer water absorbs less CO₂ and reduced nutrients weaken biological uptake, the reduced upwelling prevents carbon-rich deep water from reaching the surface, so tropical Pacific CO₂ outgassing generally decreases. So, the upwelling effect matters more than solubility. I would strongly suggest to rephrase the text here and improve the clarity to give the correct scientific message.

Also, in line 161 you mention the exception of EU simulation. So, based on text so, if there really is a direct impact of regional OAE on large scale climate pattern like ENSO, then why exactly is the EU simulation an exception? Is it then related to the amount of alkalinity you add which will affect the CO₂ uptake? A better explanation is expected here.

Furthermore, in my opinion, if you want to draw out conclusion regarding the effect of regional OAE on ENSO using an ESM, it would not be enough to have only single set of

simulations, rather an ensemble simulation setup is much more desirable. I understand that it is currently out of the scope of the presented manuscript (as mentioned in line 350-351), but given the large-scale uncertainty which comes inherently with an ESM, such conclusion backed up ensemble setup can increase the confidence much more. Otherwise, I would still think it is a signal-to-noise issue between the OAE and model internal variability.

Line 163-175: In general, I missed the discussion about the EU addition on biology here. Given the full EU coastal addition is the largest, does it have any significant impact on biology?

Line 166-168: I would like to see a figure of nitrate content in the supplement. And do you mean global nitrate content? If OAE is happening regionally, how stronger of an impact due to nitrate content will there be?

Line 181-183: Is the difference in the efficiencies only due to amount of added alkalinity in each region or are there other coastal biological/physical dynamics in action? A bit more explanation is needed here.

Line 191-193: Please cross check once the numbers in the text and the table. There is a bit of a mismatch.

Line 185 onwards: This comment is in general about the threshold selection for spreading of alkalized water masses:

Would there be any sort of relationship between the threshold level and amount of added alkalinity? How will these threshold levels change for seasonal or pulsed addition? In terms of model grid, would it also matter the per m² amount of added alkalinity and then corresponding threshold levels? Some more clarification and discussion is needed in this regard.

Line 206-211: Here a bit of explanation is needed regarding how the choice of domain, based on threshold/statistically significant CO₂ flux emergence matters. Also, in terms of areal coverage and how it relates to diagnosed uptake efficiencies.

Line 214: Fig. 6c → Fig. 6b

Line 218-219: What does much smaller mean?

Line 227: Please cross check the numbers in the text and the table.

Line 237-238: Again, this cannot be so easily generalized. In the next sentence you mention that there be biogeochemical differences, so these biogeochemical differences will also affect alkalinity and DIC. So, there is a bit of contradiction in the logical flow of sentences here. Please rephrase.

Line 244: What does vlg mean?

Line 251-255: Seems like a mixed-up sentence. Please rewrite.

Line 258-259: I would have liked to see some figures (in supplement perhaps) regarding the physics of the deployment regions (e.g. MLD and/or winds).

Line 260: The term “DIC-enriched” sounds like you follow the equilibrated OAE method. Maybe stick to Alk-enriched water instead.

Line 317-320: I see you have Supplement Fig. S6 about land carbon flux. It would be good to mention and discuss it here about the effect of land carbon flux and how it behaves in your OAE setup to make a more backed up statement.

Line 344: Again, some analysis of physics in the deployment region would be good to see what is happening in the model and how well it agrees with observations and then consequently its effect on various criteria you used to define efficiency.

Line 348: What exactly do you mean by secondary climate effects? Please clarify.

Line 369-371: Just a preference, but this can be together with the previous paragraph, no need of new paragraph here.

Regarding all figures in the manuscript:

The figures can be improved in terms of resolution, proportions, and sizes (make the figures big. Right now, with so many details in it, it is really difficult to see things clearly) with respect to its subplots.

In multiple time-series figures, it is difficult to discern the lines. The legends of such figures (time-series) can be placed outside at the bottom of figure and make it bigger. The legend is easily lost in so many lines in all the subplots.

Adding grid lines to panels would good.

The captions for all figures need to improved and should be more descriptive of the respective figure.

