EGUsphere, referee comment RC1
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Comment on egusphere-2022-1505

Anonymous Referee #1

Referee comment on "Considerations for hypothetical carbon dioxide removal via alkalinity addition in the Amazon River watershed" by Linquan Mu et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-1505-RC1, 2023

This study performs a thought experiment to explore the CO_2 removal effect via ocean alkalinity enhancement (OAE) by the hypothetical quicklime addition in the Amazon River watershed. The calculation results suggest that the total carbon uptake in the Amazon River Plume is ~ 0.07-0.1 $MtCO_2$/month when . A Monte Carlo simulation is made to assess the detectability of alkalinity perturbation, which shows that the detectability depends on the perturbation strength, the sample sizes and background alkalinity variability. This paper also discusses other potential issues related with the OAE deployment, including secondary mineral precipitation, ecological consequence, and heat release during quicklime dissolution. In summary, the authors argue that the proposed thought experiment could serve as a great starting point for investigating further the feasibility of using OAE for CO_2 removal.

I find this study interesting, and the argument is well organized and convincing. I do have however several concerns that need to be addressed first, which I summarize in the following.

- Carbonate system calculation for excess CO_2 uptake estimate. The author applies a subtle method to estimate the CO_2 uptake via the quicklime addition. One fundamental assumption is the constant DIC before any significant air-sea equilibrium occurs (Line 123). It is supported by that the air-sea equilibration takes weeks in the study region while CaO dissolution happens on hour-scales. Correct me if I am wrong, but I think this assumption needs more justification. The CO_2 uptake estimate is based on the whole Amazon River plume region. Although CaO dissolves fast, the timescale for spreading of alkalinity perturbation along the plume may be comparable to that required for the air-sea equilibration. It is reasonable to expect a DIC increase from the river gateway to the oceanic part. With an increase of DIC, the pCO_2 in the distal plume seawater will increase, which will reduce the CO_2 uptake in this region. According to Figure 5, the plume at near-oceanic salinity level contributes to the majority of CO_2 uptake due to the large area. Thus, I suggest the authors discuss or test the sensitivity of CO_2 uptake estimate to the CO_2 exchange between ocean and atmosphere along the plume route.

- The pCO_2 baseline derived from the mixing model is essential for the reliability of CO_2 uptake estimate. However, the robustness of pCO_2 baseline lacks discussion in the paper. I suggest plotting a figure (may be put in the extended figures) to compare the pCO_2-baseline with the pCO_2-empirical, which will make the performance of a simple mixing model more accessible to the reader.

- As the authors said, the detectability experiment results depend on the background variability. There are six available alkalinity measurements for each gateway near the Amazon River mouth, which could be potentially applied to constrain the natural alkalinity variation. However, only data from North Macapa is used in this paper, which is "for a concise demonstration" according to Line 97. I did not get the meaning of concise demonstration here. So please elaborate the data choice here. Also, more discussions on the background variability from other regions (in addition to North Macapa) might also be helpful.

- The theoretical saturation state for aragonite after the alkalinity perturbation is quite important to assess the performance of OAE, since secondary mineral precipitation will increase the oceanic pCO_2 and reduce the CO_2 uptake. The authors need to explain more about the calculation method (for instance, the Ca and CO_3^{2-} concentration, aragonite dissolution equilibrium constant for the calculation) for results in Table 3, rather than simply give the reference (Line 249). This is particularly important when salinity is low, because a lot of omega calculation is focused on seawater (with a high salinity). At low salinity (close to river), the equation to calculate omega might not hold.

Below are my minor comments

Two time periods, Sep-2011 and Jul-2012, shows distinct TA background (Figure 4a), empirical pCO_2 values (Figure 1), and responses to alkalinity perturbation (Figure 4 and 5). These are good and I suggest the author adds more description and explanation of the differences between these two time periods, which will offer more insights into the seasonal dynamics of the carbonate system and how this will affect the OAE effects.

Line 78 TA is tracked by the addition of extra Ca^{2+}, instead of using the carbonate species in Equation 1. Thus, I suggest changing the TA equation to the one that relies on the charge imbalance between major cations and anions.

Line 81 & 85 Ocean endmembers are not given explicitly in this manuscript, including the oceanic alkalinity and salinity.

Line 94 The authors state “measurements were made in both the river and throughout the Amazon River plume”. What kinds of measurements are made? In the later part of same paragraph, the authors explain the TA and DIC measurement strategy for the Amazon River mouth. However, the measurements for the Amazon River plume are not explained clearly.

Line 115 The way to parameterize α (solubility of CO_2 in seawater) is not clearly explained.