Reviewer comments – egusphere-2025-4887 – The carbon dioxide removal potential of cement and lime kiln dust via ocean alkalinity enhancement

Overview:

The manuscript from Flipkens et al. explores the suitability of two by-products of the cement industry, cement and lime kiln dusts (CKD and LKD, respectively). Using laboratory experiments, they report first estimates on the dissolution kinetics of such feedstocks in natural seawater, both using short- and long-term experiments, while expanding on the risks for $CaCO_3$ precipitation and potential trace metals release. Finally, CDR estimates are given, assuming the use of all available CKD and LKD, and considering inorganic thresholds and water quality and safety guidelines.

Overall, I found the manuscript very enjoyable to read. While some people may express reluctance to consider such by products for OAE, their significant alkalinity release makes them suitable candidates, and the presented data support this. The introduction is rather comprehensive, and most required information is already provided. The material and method section is also mostly complete with some smaller comments pointed out below. The results section is rather dense, yet all required information is well presented. Finally, the discussion is convincing and reports well on the implication of kiln dust based OAE. Some further points could be added to the discussion, especially when it comes to the alkalinity generation. While LKD was following estimates, the nearly doubling alkalinity generation from CKD is yet to be fully addressed. I believe a more detailed discussion could benefit the paper. It would be beneficial to try and characterise the amorphous phases that clearly seem to be the responsible factor. I do not request further analyses but rather consider whether using the elemental composition of CKD (Table A1), an estimate of the amorphous phases' composition could be derived. Another point that could benefit from further interpretation is the fate of KD and especially the potential secondary minerals precipitated. It is clear that non soluble phases such as $CaCO_3$ would eventually sink and as described, potentially dissolve on the seabed. It would also be the case for secondary $CaCO_3$ formed from the high alkalinity generated. One could consider discussing the dissolution on the seabed of both the $CaCO_3$ from KD and the freshly precipitated $CaCO_3$ in the water column. The overall potential would probably increase, and if it is significant, it would be worth mentioning. Finally, a smaller point regarding the turbidity discussion. Discussing such environmental guideline is a great idea and of high importance. Unless I am mistaken, the turbidity is reported for 24h. In line 343, it is mentioned that the KD particles would remain in the surface ocean mixed layer for about one hour. Therefore, one could safely assume that after 1h the turbidity is back to low enough levels that new KD could be added. Under such assumption, the overall KD release could be increased, ultimately increasing the CDR potential. If it is correct, it could be quicky discussed.

Considering the minor comments mentioned above, and once they have been addressed, I am fully supporting the publication of the manuscript.

Comments:

Line 6: I am not sure whether “ocean liming” is a suitable keyword here, considering it investigates kiln dust potential for OAE rather than lime. But this can be up to the authors.

Line 34: I believe brackets are missing and should read “brucite ($Mg(OH)_2$)”.

Line 42: one could argue the use of “rapid” here, as it can take month to years before the alkalised water equilibrates with atmospheric $CO_2$. Please edit accordingly.

Line 57: consider moving “hence” at the start of the sentence.

Line 69: has this method been tested and proven to be reliable and efficient at dissolving such product? If so, a reference (or in-house quality control) could be inserted.

Lines 87-88: how long was the seawater aerated for? Was there any check to confirm equilibration? (pH measurements, $pCO_2$, etc.).

Line 111: how were DIC samples taken? Given the gas sensitivity of these samples, it would be interesting to report whether a peristaltic pump was used or any other apparel.

Line 125-126: since the pH has been measured and later reported on the total scale, it would be beneficial to report it as $pH_T$ throughout the text

Line 150: salinity has no unit, please remove the “ppt”. alternatively, you could report the ionic strength instead, but it is not required

Line 164: I believe PHREEQC provides saturation indexes, not saturation states. The term $\Omega$ is usually used only for $CaCO_3$ if I am correct

Lines 164-169: I am not sure I understand correctly. First it is said that saturation states were calculated with PHREEQC (line 164). Then, it is said that $\Omega_A$ and $\Omega_C$ were not computed in PHREEQC. Why that?

Line 246: I believe that “for CKD is missing after the 1.3 mmol $g^{-1}$.

Line 264: it would be great to have the 0 on the y axis of tile B.

Lines 293-294: how can turbidity be slightly greater yet significantly greater? Do you mean statistically significantly greater?

Line 310: it would be great to have the 0 on the y axis of tile B.

Line 330: I believe “be” should be deleted.