

Response to Reviewers

Dear reviewers,

We sincerely thank you for the time and effort devoted to evaluating our manuscript, and for your constructive comments. We have carefully considered each point and revised the manuscript accordingly. The revisions include additional statistical analyses, restructuring of the Material and methods, Results and Discussion sections, more cautious interpretation of our findings, and an expanded comparison with the relevant literature. Below, we provide a point-by-point response. Each response refers to the corresponding line numbers in the clean revised manuscript.

We hope you will be pleased by the revisions and welcome any further comments and suggestions that may improve our research.

Best regards,

Charly Moras, on behalf of all co-authors.

Reviewer 1 Comments

The authors have carefully revised the manuscript by addressing the reviewers' comments and conducting additional analyses (mineral grain size and specific surface area), which is appreciated. The manuscript has improved substantially. However, some methodological details remain unclear or insufficiently described, several interpretations would benefit from more nuanced wording, and I believe that the section linking laboratory results to modelling studies would benefit from restructuring to improve clarity and focus. Detailed comments are provided below.

We would like to thank the reviewer for their additional round of reviews. We carefully considered every suggestion and implemented the requested changes, particularly by removing the new figures from the discussion, adding more calculation details to the material and methods section rather than in the results section, and especially reprocessing the pCO_2 data with the linear mixed-effects models with post hoc comparisons and adjusted p-values.

Line 171: A space is missing between “50” and “mL”. Please carefully check the manuscript for additional spacing inconsistencies.

Thank you for pointing this out, we indeed missed this space in the first review and corrected it in this new version (line 162).

Line 175: Please elaborate on how Meas. TA_{loss} and Max. TA_{loss} were calculated. Based on the values reported in Table 1 and Figure 4 for the 600 $\mu mol\ kg^{-1}$ $Mg(OH)_2$ treatment, the calculations are not immediately clear. For example, for the 0 RPM $Mg600$ treatment, I would expect the measured TA loss to be approximately 770 $\mu mol\ kg^{-1}$ (target addition of 600 $\mu mol\ kg^{-1}$ minus an observed ΔTA of roughly $-170\ \mu mol\ kg^{-1}$), assuming full dissolution of $Mg(OH)_2$. However, Table 1 reports values closer to $\sim 230\ \mu mol\ kg^{-1}$. If Meas. TA_{loss} is instead based on the dissolved fraction of $Mg(OH)_2$, please clarify how this fraction was derived, especially since some precipitation likely already occurred within the first day in the static high-concentration treatments, as suggested by the non-linear ΔTA response with increasing $Mg(OH)_2$ addition.

Thank you for asking for more details, we have corrected the text accordingly. We have added a new section describing the calculations (lines 226-233). The corrected version reports Meas. TA_{loss} as the amount of alkalinity removed from the starting water column concentration, based on TA measurements. Hence, for replicate 1 of 0 RPM, given that the average starting TA was 2325.5 and lowest ΔTA 2153.0, the Meas. TA_{loss} is 172.6 μM . While this is only the removal from the water column (most likely to runaway $CaCO_3$ precipitation), we agree that the actual TA loss is $\sim 770\ \mu M$, which is what we labelled as Max. TA_{loss} (calculated at 769.1 μM in the Table for the same experiment).

Also, we agree with the idea that some precipitation most likely occurred on day 1. We therefore report the Max. TA_{loss} as the sum of ΔTA expected (596.6 μM) and the measured removal from the water column, i.e., Meas. TA_{loss} . This leads to an actual Max TA_{loss} of $596.6 + 172.6 = 769.1\ \mu M$. This represents the total amount of alkalinity lost to incomplete dissolution of material (expected 596.6 μM) and concurrent precipitation. This calculation aims at emphasising that simply measuring TA does not effectively account for losses of efficiency from other pathways.

Materials & Methods: Please include a short subsection describing the statistical analyses performed taking into account my later comment about the statistics.

Thank you for the comment. A section has been added to support the statistical calculations (lines 213-225).

Line 214: Please specify what the error bars represent (e.g. standard deviation, standard error, or range).

Thank you for pointing this out, we have edited accordingly as per the other figures: “Each point represents the mean of the triplicate measurements, with error bars representing the uncertainty based on the standard deviation of these three measurements.” (lines 257-258)

Line 285: For consistency and clarity, also indicate here that values are presented as mean $\pm$ standard deviation.

Thank you for the comment, this has been done as follows: “Each point represents the mean of the triplicate measurements, with error bars representing the uncertainty based on the standard deviation of these three measurements.” (lines 365-366)

Line 336: Add “within one day” after “92% of the expected TA”.

This has been edited as suggested (line 425).

Line 371: I recommend citing your paper Moras et al. (2022) on runaway precipitation here, along with other relevant studies discussing this phenomenon. This would help readers unfamiliar with the concept locate the relevant background literature more easily.

Thank you for the comment, we have added the suggested reference, as well as Hashim et al., 2025, who observed similar patterns at varying alkalinity additions with liquid feedstock as per our NaOH experiments, Moras et al., 2024 observing runaway precipitation with $\text{Mg}(\text{OH})_2$, and Suitner et al., 2025 who characterises the phenomenon based on alkalinity levels (line 448).

Lines 435–437: These statements should be phrased more carefully. Persistent low- pCO_2 waters may still occur in nearfield regions around outfalls with continuous alkalinity dosing (assuming regulations permit such conditions). In addition, dilution/water mixing should play a larger role than atmospheric CO_2 uptake in restoring pCO_2 of an alkalinized seawater patch, since air–sea equilibration is comparatively slow. While dilution is mentioned later in the paragraph, I recommend already mentioning it earlier to provide a more balanced interpretation. A similar comment applies to lines 443–445.

We agree with the comment. We have toned down some formulations and have emphasised in several instances in the manuscript that dilution will play a more significant role (for example, in lines 475 and 553). We also added a limitations paragraph that reads: “Several limitations should be considered when interpreting these findings. First, the experiments only reproduced simplified hydrodynamic conditions and did not fully capture the complexity of ocean turbulence, wave breaking, or horizontal and vertical transport. Second, repeated sampling progressively reduced the volume in each bottle, changing the headspace and potentially influencing gas exchange over time, although all treatments underwent the same sampling schedule. In addition, airflow rate and relative humidity were not measured at the individual outlets, and differences in headspace renewal among bottles therefore cannot be excluded. Nevertheless, disregarding biological processes, particle transport and large-scale dilution allowed

physicochemical processes to be isolated. The results should therefore be interpreted as process-based laboratory observations that inform, rather than reproduce, field-scale OAE deployment.” (lines 623-631)

Line 438: The sentence discussing literature mesocosm studies should be integrated into the preceding paragraph discussing literature results (lines 428–432) to improve coherence. The subsequent comparison with toxicological thresholds could then also be generalized, since the experimental pCO₂ values were higher than the toxicity thresholds (~100 µatm) observed in the other phytoplankton studies as well.

We agree and have reshaped that section to accommodate the reviewer’s comment regarding pCO₂ in our experiments (line 611), while mesocosms are discussed earlier in the paragraph (lines 594 and 606). For example, we now state: “These concentrations remained close to reported optimal values for coccolithophore calcification in a large-scale mesocosm study (Schneider et al., 2026), suggesting, based on previously reported thresholds, that severe biological effects would have been unlikely under our experimental carbonate chemistry conditions.” (lines 611-614)

Line 389 onward + Figure 8: As a general rule, new experimental results should not be introduced in the discussion section. I therefore recommend moving Figure 8, together with a concise description of the DIC results, to the Results section.

This is a fair suggestion. The third reviewer also suggested such a change, hence we have decided to move the CO₂ ingassing rates as well as the wind speed calculations to the Materials and Methods (lines 190-205), and Results sections (lines 331-347).

Lines 495–539: This section currently combines elements of Materials & Methods (detailed equation descriptions), Results (presentation of calculated values), and Discussion (comparison with literature and broader implications). This weakens the overall focus and flow of the Discussion. I therefore recommend moving the calculated k values and DIC fluxes to the Results section, potentially together with Figure 8 and the DIC results. The calculations themselves should be briefly described in the Materials & Methods section, while the full equations can be included either in the Materials & Methods or in the Supplementary Information to improve readability and maintain a clearer narrative structure.

We agree with the reviewer and have proceeded to a large restructuring of the manuscript. All calculation descriptions and equations are now in the Methods section, the calculated fluxes are now presented in the Results with Fig. 8 (now Fig. 6 in the manuscript), and interpretation was kept in the discussion though combined with the environmental and biological effects. This led to a slightly shorter discussion but with a revised structure enhancing the flow.

Line 515: Remove either “first” or “initial”, since both have the same meaning.

We agree and decided to remove “initial” and it now reads: “these gas transfer velocities provide a useful first-order approximation” (lines 576-577).

Lines 555–556: This sentence is somewhat difficult to follow. Consider reformulating it, for example by starting with “For the 125 RPM treatment, ...” to avoid repetition and improve readability.

Thank you for the suggestion. This passage was revised during the restructuring, and this section now reads “Using the parametrisation of Wanninkhof (2014), corrected equivalent wind speeds were estimated to be 1.7 m s⁻¹ and 3.1 m s⁻¹, respectively. Wind speeds in the open ocean vary greatly, from less than 1 m s⁻¹ to over 20 m s⁻¹ during heavy wind and storm periods, with a mean of approximately 6 m s⁻¹ (Ho et al., 2006; Miller et al., 2010; Risien and Chelton, 2008).” (lines 579-582)

Lines 556–558: Here again, dilution likely plays an important role in limiting CAP persistence and should not be overlooked in the discussion.

We have edited to incorporate the dilution factor, with the section now reading “However, their overall influence on the CAP duration will also mostly depend on mixed-layer deepening, dilution, and vertical transport, which will determine the residence time of alkalised water near the surface (Cushman-Roisin and Beckers, 2011; England and Bach, 2025; Flipkens et al., 2023; Tokinaga and Xie, 2011).” (lines 585-588)

Line 558: I would avoid the term “quick” before “CO₂ ingassing”. Several studies describe air–sea CO₂ equilibration as a relatively slow process that may take weeks to years (e.g. Zhou et al., 2025, *Nature Climate Change*, 15(1), 59–65). Using “quick” may unintentionally give the opposite impression to non-specialist readers.

We understand and removed the “quick” statement. The whole section was also restructured and toned down with more cautious formulations. For example, it now reads: “Although highly relevant to OAE applications, such physical processes are poorly represented in bottle experiments. In natural systems, vertical and horizontal transport, tidal mixing and dilution would further reduce local supersaturation and available surface area for precipitation, as may occur in point-source alkalinity release (Lee et al., 2026). While this was not tested here, these processes could shorten the CAP duration more rapidly than atmospheric CO₂ ingassing alone because CO₂ equilibration occurs over longer timescales in the open ocean and varies substantially with latitude and season (Zhou et al., 2025).” (lines 552-556)

Table A1: Given the large number of comparisons performed, please clarify whether p-value adjustment was applied. Conducting many independent two-sample t-tests across multiple concentrations and time points substantially increases the probability of type I errors (false positives). In addition, given the repeated-measures structure of the experiment (multiple observations from the same bottles over time), a linear mixed-effects model with post hoc comparisons and adjusted p-values would likely provide a statistically more robust approach than numerous independent t-tests.

Thank you for pointing this out. We have reprocessed the data following your suggestions and included the following description in the methods section: “Changes in pCO₂ over time were analysed separately for the 0 and 125 RPM treatments using linear mixed-effects models implemented in MATLAB (MathWorks). The fixed effects included treatment, i.e., ΔTA targets, sampling time, and their interaction, while bottle identity was set as a random intercept to account for repeated measurements from the same experimental unit. Sampling time was treated as a categorical factor to avoid assuming a linear temporal response and to enable comparison among individual sampling dates. The control treatment and day 0 were specified as the reference levels for treatment and sampling time, respectively. Overall effects of treatment, sampling time, and their interaction were assessed using F-tests with Satterthwaite’s approximation for the denominator degrees of freedom. Upon significant interaction between treatment and sampling time, planned one-sided post hoc contrasts were performed to compare each treatment with the control at each sampling time, testing the hypothesis that treatment pCO₂ was

lower than that of the corresponding control. To account for multiple comparisons between treatments and the control at each sampling time, the one-sided p-values were adjusted using the Holm procedure. Treatment pCO₂ was considered significantly lower than the control when the estimated difference between treatment and control was negative and the corresponding one-sided adjusted p-value was < 0.05. Model assumptions were evaluated by visual inspection of residual-versus-fitted plots and normal probability plots of the residuals.” (lines 213-225)

Table A2: Holm-adjusted one-sided p -values from planned post hoc treatment-control contrasts of the linear mixed-effects models, testing whether $p\text{CO}_2$ in each treatment was lower than that of the corresponding control at each sampling time. The Holm adjustment was applied across all treatment-control comparisons within each sampling time. Values in bold indicate statistically significant differences (adjusted $p < 0.05$). Adjusted p -values below 0.01 are reported as < 0.01 .

[illegible]

Reviewer 3 Comments

First, I want to highlight, that I came into this review process after the first iteration. This implies that I tried to find a balance between looking at the manuscript with an unbiased opinion, but at the same time trying to acknowledge the already past review process.

Summary:

The study aims to understand the impact of advection in the water column on CO₂ uptake and the subsequent change in mineral dissolution and alkalinity stability during ocean alkalinity enhancement. The major results are that faster equilibration leads to higher amounts of dissolved alkalising material (in the case of solid feed stock) and in a lower loss to brucite and subsequent CaCO₃ formation (in the case of liquid fed stock).

General comments:

Given that this is the second iteration of the script, the state it is in is still poor. The method section is still very messy and difficult to follow. Whilst at some point unmercenary details are presented, major methodologies are not well explained.

The result part needs severe streamlining and reduction to results. Very often the authors mix in discussion-like snippets that do not belong here.

At the same time the authors readily state that their data supports certain ideas without explaining in what way that is the case. This coincides with at least loose if no uninformed statements about diffusive processes around grains.

The discussion needs a clearer line of thought and less attempts to invent a new concept. Additionally, the discussion would greatly benefit from a more comprehensive comparison of this studies new findings with other studies to emphasise the impact and the relevance of the new results. Overall, I want to urge the authors to do a thorough literature research before revising the manuscript. The last iteration was, as far as I saw it definitely not thorough enough.

Dear reviewer, we would like to sincerely thank you for the thorough review you have provided and the valuable suggestions provided. We have proceeded to a major rework of the manuscript, particularly the discussion with a strong focus on flow. We have toned down our formulations and now use cautious language rather than presenting speculative interpretations as established conclusions:

- “The derived wind speeds should therefore be interpreted as gas-exchange equivalents rather than direct hydrodynamic analogues of open-ocean conditions (Frew et al., 2007; Yu et al., 2013; Zhou et al., 2023).”
- “Several limitations should be considered when interpreting these findings. First, the experiments only reproduced simplified hydrodynamic conditions and did not fully capture the complexity of ocean turbulence, wave breaking, or horizontal and vertical transport. Second, repeated sampling progressively reduced the volume in each bottle, changing the headspace and potentially influencing gas exchange over time, although all treatments underwent the same sampling schedule. Finally, biological processes, particle transport and large-scale dilution were not investigated, thereby allowing physicochemical processes to be isolated.”
- “The results should therefore be interpreted as process-based laboratory observations that inform, rather than reproduce, field-scale OAE deployment.”

We also conducted an extensive literature search to fill key knowledge gaps you have pointed out (i.e., diffusive processes around grains, duration of the experiments compared with other experiments in the literature, secondary mineral formation as well as the role of advection on dissolution). Below is the full list of newly added literature, but we would like to highlight the textbook work of Drever (1997) and published research from Lasaga (2014), Levich (1962) and Sulpis et al. (2019) regarding diffusive boundary layer processes, studies by Meysman and Monsterrat (2017), Flipkens (2023) and particularly the works from Fuhr et al (2023, 2024, 2025) which, despite focusing on olivine-rich minerals and sediment interactions, provide fundamental insights on hydrodynamics and CaCO_3 precipitation, and the early works from Haas (1916) and Kapp (1928) who investigated secondary formation of hydroxides following alkalinity addition to seawater.

List of newly added published works:

- Bach, L. T.: The additionality problem of ocean alkalinity enhancement, *Biogeosciences*, 21, 261-277, 10.5194/bg-21-261-2024, 2024.
- Drever, J. I.: *The Geochemistry of Natural Waters*, 3rd, Surface and Groundwater Environments, Prentice Hall, 1997.
- England, P. I. and Bach, L. T.: Influence of Wave Action on Applications of Olivine-Based Ocean Alkalinity Enhancement on Sandy Beaches, *Geophysical Research Letters*, 52, e2025GL114922, 10.1029/2025GL114922, 2025.
- European Parliament and Council of the European Union: Directive 2000/60/EC of the European Parliament and of the Council of 23 October 2000 establishing a framework for Community action in the field of water policy, *Official Journal of the European Communities OJ L 327*, 1–73, 22/12/2000 (consolidated version: 10/05/2026), 2000.
- Fuhr, M., Geilert, S., Schmidt, M., Liebetrau, V., Vogt, C., Ledwig, B., and Wallmann, K.: Kinetics of Olivine Weathering in Seawater: An Experimental Study, *Frontiers in Climate*, 4, 10.3389/fclim.2022.831587, 2022.
- Fuhr, M., Wallmann, K., Dale, A. W., Diercks, I., Kalapurakkal, H. T., Schmidt, M., Sommer, S., Böhnke, S., Perner, M., and Geilert, S.: Disentangling artificial and natural benthic weathering in organic rich Baltic Sea sediments, *Frontiers in Climate*, Volume 5 - 2023, 10.3389/fclim.2023.1245580, 2023.
- Fuhr, M., Wallmann, K., Dale, A. W., Kalapurakkal, H. T., Schmidt, M., Sommer, S., Deusner, C., Spiegel, T., Kowalski, J., and Geilert, S.: Alkaline mineral addition to anoxic to hypoxic Baltic Sea sediments as a potentially efficient CO_2 -removal technique, *Frontiers in Climate*, Volume 6 - 2024, 10.3389/fclim.2024.1338556, 2024.
- Fuss, S., Lunstrum, A., Prütz, R., and Gruner, F.: Chapter 10: Costs and Potentials, in: *The State of Carbon Dioxide Removal 3rd Edition 2026* (eds. Edwards, M. R. et al.), 10.17605/OSF.IO/FBZ4X, 2026.
- Griskey, R. G.: Fluid Flow Basic Equations, in: *Transport Phenomena and Unit Operations: A Combined Approach*, John Wiley & Sons, 23-54, 10.1002/0471722057, 2002.
- Haas, A. R.: THE EFFECT OF THE ADDITION OF ALKALI TO SEA WATER UPON THE HYDROGEN ION CONCENTRATION, *Journal of Biological Chemistry*, 26, 515-517, 10.1016/S0021-9258(18)87433-6, 1916.
- IPCC: Summary for Policymakers. In: *Climate Change 2021: The Physical Science Basis. Contribution of Working Group I to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change* [Masson-Delmotte, V., P. Zhai, A. Pirani, S.L. Connors, C. Péan, S. Berger, N. Caud, Y. Chen, L. Goldfarb, M.I. Gomis, M. Huang, K. Leitzell, E. Lonnoy, J.B.R. Matthews, T.K. Maycock, T. Waterfield, O. Yelekçi, R. Yu, and B. Zhou (eds.)], Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, pp. 3-32, 10.1017/9781009157896.001, 2021.

- IPCC: Summary for Policymakers. In: Climate Change 2022: Impacts, Adaptation and Vulnerability. Contribution of Working Group II to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change [H.-O. Pörtner, D.C. Roberts, M. Tignor, E.S. Poloczanska, K. Mintenbeck, A. Alegría, M. Craig, S. Langsdorf, S. Löschke, V. Möller, A. Okem, B. Rama (eds.)], Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, pp. 3–33, 10.1017/9781009325844.001, 2022.
- Isometric: Ocean Alkalinity Enhancement from Coastal Outfalls, Isometric, Version 1.1, 2026.
- Jones, D. C., Ito, T., Takano, Y., and Hsu, W.-C.: Spatial and seasonal variability of the air-sea equilibration timescale of carbon dioxide, *Global Biogeochemical Cycles*, 28, 1163-1178, 10.1002/2014GB004813, 2014.
- Kapp, E. M.: The Precipitation of Calcium and Magnesium from Sea Water by Sodium Hydroxide, *Biological Bulletin*, 55, 453-458, 10.2307/1536800, 1928.
- Kauk, S., World's first verified Ocean Alkalinity Enhancement credits: <https://isometric.com/writing-articles/worlds-first-verified-ocean-alkalinity-enhancement-credits> (last access: 14 July 2026), 2025.
- Lasaga, A. C., *Kinetic Theory in the Earth Sciences*, Princeton University Press, Princeton, doi:10.1515/9781400864874, 2014.
- Lee, C.-H., Subhas, A. V., Kim, J.-H., and Lee, K.: Ocean Carbon Dioxide Removal and Storage, *Chemical Reviews*, 126, 1110-1144, 10.1021/acs.chemrev.5c00433, 2026.
- Levich, V. G.: *Physicochemical hydrodynamics*, Prentice-Hall, 1962.
- Lunstrum, A., Keller, D., Hartmann, J., and Moras, C. A.: A framework for understanding efficiency losses of Ocean Alkalinity Enhancement, *CDRXIV* [preprint], 10.70212/cdrxiv.2026517.v1, 2026.
- Meysman, F. J. R. and Montserrat, F.: Negative CO₂ emissions via enhanced silicate weathering in coastal environments, *Biology Letters*, 13, 20160905, 10.1098/rsbl.2016.0905, 2017.
- Montserrat, F., Renforth, P., Hartmann, J., Leermakers, M., Knops, P., and Meysman, F. J. R.: Olivine dissolution in seawater: Implications for CO₂ sequestration through enhanced weathering in coastal environments, *Environmental Science & Technology*, 51, 3960-3972, 10.1021/acs.est.6b05942, 2017.
- Oschlies, A., Bach, L. T., Rickaby, R. E. M., Satterfield, T., Webb, R., and Gattuso, J. P.: Climate targets, carbon dioxide removal, and the potential role of ocean alkalinity enhancement, *Guide to Best Practices in Ocean Alkalinity Enhancement Research*, 2-oae2023, 1, 10.5194/sp-2-oae2023-1-2023, 2023.
- Reynolds, O.: XXIX. An experimental investigation of the circumstances which determine whether the motion of water shall be direct or sinuous, and of the law of resistance in parallel channels, *Philosophical Transactions of the Royal Society of London*, 935-982, 10.1098/rstl.1883.0029, 1883.
- Rigopoulos, I., Harrison, A. L., Delimitis, A., Ioannou, I., Efstathiou, A. M., Kyratsi, T., and Oelkers, E. H.: Carbon sequestration via enhanced weathering of peridotites and basalts in seawater, *Applied Geochemistry*, 91, 197-207, 10.1016/j.apgeochem.2017.11.001, 2018.
- Saez Moreno, M., Hartmann, J., Börker, J., Bartsch, P., and Moras, C. A.: The role of temperature and salinity on ocean alkalinity enhancement performance, *EGUsphere*, 2026, 1-30, 10.5194/egusphere-2026-3277, 2026.
- Sharqawy, M. H., Lienhard, J. H., and Zubair, S. M.: Thermophysical properties of seawater: a review of existing correlations and data, *Desalination and Water Treatment*, 16, 354-380, 10.5004/dwt.2010.1079, 2010.
- Suijter, N., Hartmann, J., Varliero, S., Faucher, G., Suessle, P., and Moras, C. A.: Surface area and Ω -aragonite oversaturation as controls of the runaway precipitation process in ocean alkalinity enhancement, *Biogeosciences*, 23, 3965-3980, 10.5194/bg-23-3965-2026, 2026.

- Sulpis, O., Mucci, A., Boudreau, B. P., Barry, M. A., and Johnson, B. D.: Controlling the diffusive boundary layer thickness above the sediment–water interface in a thermostated rotating-disk reactor, *Limnology and Oceanography: Methods*, 17, 241-253, <https://doi.org/10.1002/lom3.10309>, 2019.
- U.S. Environmental Protection Agency, National Recommended Water Quality Criteria - Aquatic Life Criteria Table: <https://www.epa.gov/wqc/national-recommended-water-quality-criteria-aquatic-life-criteria-table> (last access: 14 July 2026), 2026.
- U.S. Environmental Protection Agency: Quality Criteria for Water 1986, U.S. Environmental Protection Agency, Washington, DC, EPA 440/5-86-001, 1986.
- Zhou, M., Tyka, M. D., Ho, D. T., Yankovsky, E., Bachman, S., Nicholas, T., Karspeck, A. R., and Long, M. C.: Mapping the global variation in the efficiency of ocean alkalinity enhancement for carbon dioxide removal, *Nature Climate Change*, 15, 59-65, [10.1038/s41558-024-02179-9](https://doi.org/10.1038/s41558-024-02179-9), 2025.

Overall, though, the results are relevant and certainly worth being published in this journal. Hence, I recommend an absolute major revision of the script and will gladly review the result.

Here the specific comments as occurred upon reading:

Line-by-line comments:

Line 10/11: "...using solid..." please rephrase with "...using sea water amended with solid..."

Thank you for the suggestion, we edited accordingly (line 11, and throughout the text).

Line 13a: Compared to expected values?

Yes indeed, this has been edited as suggested (line 13).

Line 13b: Bottles don't lead alkalinity generation. Please rephrase this.

This is a good point, we reformulated as: "in the bottles subjected to advection, alkalinity generation was nearly twice as high" (lines 13-14)

Line 20: Grammatically incorrect: precipitation does not redissolve.

We agree with the statement which is why we emphasised that CaCO_3 precipitates did not redissolve. We reformulated as: "conditions remained stable and freshly precipitated CaCO_3 did not redissolve" (line 20)

Line 22: Data does not serve as a threshold. Data can support an argument for lower thresholds. Or sth. in these lines.

We agree and rephrased as: “data presented here may be used to derive lower thresholds for OAE implementation in global models” (lines 22-23)

Line 27: I thought that this was already in the 2021 IPCC report. Please check citation

We checked and though it is not directly mentioned, it is implied in the 2021 IPCC report, so we added the reference as suggested (line 28).

Line 28/29: “...the highest carbon capture potential...”. Please provide a citation for this statement, and a rough number.

We understand and added the latest State of CDR edition in line 30 (chapter by Fuss et al.), which reports OAE as having one of the highest CDR potential (Chapter 10, page 236).

Line 32: Some issues is really not what you want to say in a scientific paper. Secondary effects... or „Several processes “. Yes, let’s do that.

This is a valid point and we edited to “Secondary effects such as...” (line 34)

Line 33a: The potential is not decreased. The efficiency is. Please rephrase.

We rephrased as suggested: “and may decrease efficiency” (lines 34-35)

Line 33b: “itself” is redundant here. Just say the ocean or alkalised seawater.

We agree and removed itself and kept “ocean” (line 35)

Line 34: Not sure, whether the formula needs to be introduced since it’s been used in the abstract before.

This is a good question, though to be more conservative, we decided to add the formula here as well for the less familiar readers (line 38)

Line 36: Despite the fact that these authors used olivine or dunite respectively, Fuhr et al. 2022 and Rigopoulos et al. 2018 were the first to report on CaCO₃ precipitation on grain surfaces during OAE. Please add references.

This is a fair point. We have reformulated theses few lines to include suggested works and emphasise the works were published before the others. It now reads: “Early work using olivine-rich minerals revealed that in the presence of particles, increasing alkalinity may trigger CaCO₃ formation on the undissolved particles despite slow dissolution kinetics (Fuhr et al., 2022; Rigopoulos et al., 2018). Studies using more reactive solid feedstocks such as calcium hydroxide (Ca(OH)₂) and magnesium hydroxide (Mg(OH)₂) have revealed CaCO₃ precipitation once alkalinity was increased by 500 µmol

kg-1 (Hartmann et al., 2023; Moras et al., 2022; Moras et al., 2024; Suitner et al., 2024; Varliero et al., 2024).” (lines 36-40)

Line 39: Or you add the mentioned references here after aragonite.

We have incorporated them above as suggested.

Line 41: “...where the precipitation speed increases...”. I would rephrase to sth. like “with increasing precipitation rates”.

This was reformulated as suggested: “it may enter a runaway pattern, with increasing precipitation rate, sometimes reaching TA values lower than the starting conditions” (lines 44-45)

Line 44: “Hence, one could suggest”: Then why does one not simply suggest this, when they could? ;) Jokes aside. Please rephrase this.

This was rephrased as suggested: “Hence, $\text{Mg}(\text{OH})_2$ could be a more favourable alkaline feedstock” (line 47)

Line 47: “...loss to CaCO_3 ”: Please rephrase to “...loss to CaCO_3 formation”

Thank you, this was edited as “the persistence of high alkalinity and ΩA (micro-)environments is expected to enhance the likelihood of CaCO_3 formation” (lines 49-50)

Line 48a: If it’s microenvironments around added feed stock particles, how much of a biological risk can that be? Clarify or cite a respective study, please.

This is a fair point and we believe the sentence was misleading. High TA and ΩA environments, both in the DBL and water column may enhance CaCO_3 precipitation. But also, the concomitant pH increase and CO_2 decrease may affect biological processes. We have included the work of Bach et al., 2019 which discuss such potential risk, and reformulated the section as: “Overall, the persistence of high alkalinity and ΩA 50 (micro-)environments is expected to enhance the likelihood of CaCO_3 formation, particularly through secondary precipitation in the diffusive boundary layer of dissolving particles or on suspended solids. Also, the concomitant high pH and low CO_2 conditions may impact biological processes because CO_2 is essential for photosynthesis (Bach et al., 2019).” (lines 49-52)

Line 48b: “This period of high alkalinity is therefore critical to the overall safe and effective implementation of OAE to limit negative impacts of OAE and maintain high CDR potential, which is later referred to as the critical alkalinity period (CAP).”: This sentence at this point suggests that CAP refers to the time when microenvironments exist. Is that the idea? Or should it also refer to times when the entire water column is critically alkalinised? Please clarify and rephrase if necessary.

Thank you for pointing this out. This refers to immediate conditions after alkalinity addition where alkalinity is high enough to trigger precipitation. This includes the time before dilution, sinking, mixing and CO_2 ingassing. We reformulated as: “This transient period following alkalinity addition, during

which carbonate chemistry is substantially perturbed, is hereafter referred to as the critical alkalinity period (CAP).” (line 52-54)

Line 51: If you say “First”, you must say second, which you do not. Please rephrase.

This is a good point, we changed “Similarly” to “Second” three lines below (line 66).

Line 52: “...weeks.”: Here you need to cite the most relevant lab-based OAE studies that fulfil your description. Keep in mind that you were talking about “OAE-focussed experiments”, which encompasses a broad range of experiments. So, either the list of references here will be very long or you want to rephrase this.

This is a fair point, we edited as: “these experiments were usually conducted over shorter periods of time, from minutes to weeks (Hartmann et al., 2023; Moras et al., 2022; Moras et al., 2024; Shaw et al., 2025; Yang et al., 2023), though longer experiments exist with slower-dissolving materials such as olivine-rich minerals (Flipkens et al., 2023; Fuhr et al, 2022; Fuhr et al, 2024; Fuhr et al, 2025; Montserrat et al., 2017).” (lines 60-64)

Line 53: “One can...”: One should really do sth. rather than stating what one could do. please rephrase.

Thank you for the comment, we reformulated as: “Given the time dependency of CaCO_3 precipitation kinetics, previously reported patterns for weeks may be different once monitored for months.” (lines 64-66)

Line 54a: “these experiments”: Which ones? Please provide references.

This refers to the previously mentioned experiments, but we edited as: “Second, experiments using hydroxides” (line 66)

Line 54b: In what way is a stir bar problematic? Please provide a short explanation and again: references.

This is rather not a problem, as it is a very well described approach for dissolution kinetics. But conducting experiments in closed vessels with a stir bar do not allow for active CO_2 ingassing and most likely leads to higher dissolution kinetics than could be observed in a real-world application. This is particularly evident when comparing results from Moras et al., 2022 and 2024, and the experiment with $\text{Ca}(\text{OH})_2$ and $\text{Mg}(\text{OH})_2$ (lines 66-68)

Line 57: “...what would occur in a natural setting”: How high would that be? Please provide respective references.

Thanks for pointing this out. It is hypothetical hence we do not have a reference to add. We decided to remove the sentence to prevent confusion.

Line 58: “One could...” Please rephrase.

Thank you, we reformulated as: “Hence, we hypothesise that the patterns observed in closed laboratory experiments with stir bar may not fully represent open-ocean conditions” (lines 69-70)

Line 61: How do you define open ocean conditions? What parameters are considered, how are these defined? is it just the mixing? In the abstract you say about 3 ms^{-1} wind speed, but what does that mean? airflow? turbulence? an expected gas-exchange velocity calculated via the Schmidt number?

Thank you for the comment. We have edited the sentence to emphasise that we aimed at generating turbulence conditions similar to open ocean and looked for a correlation based on the Reynolds number calculations provided later on. We reformulated as: “Circular advection regimes were chosen to try and reproduce turbulent conditions similar to those observed in the open ocean, allowing for vertical diffusion but limiting disruption of the water surface, while gas exchange was allowed to occur during the experiments” (lines 71-73)

Line 64: “and gas velocities for CO₂ and equivalent open ocean wind speeds were derived”: This does not really make sense. What gas velocities? What equivalent? Please explain and rephrase.

Thank you for the comment, this was reformulated as: “Carbonate chemistry was monitored throughout, particle analyses were conducted at the end of the experiments, and recorded changes in dissolved inorganic carbon (DIC) were used to calculate the gas transfer velocities. We later derived the equivalent open ocean wind speeds and compared them with values recorded in the open ocean” (lines 73-76)

Line 75: “more appropriate to resuspend”: Compared to what? Please clarify.

Thank you for the comment, this is compared to the linear motion mentioned later. We reformulated the sentences as described below.

Line 76: “without breaking water tension”: What do you mean by that?

Breaking water tension refers to small “waves” or “splashing” inside the bottles. We reformulated the sentences as: “Linear motion produced insufficient particle resuspension and substantial disruption of the water surface, including splashing. Circular motion produced turbulent conditions with effective vertical mixing without visible splashing. (lines 98-100)

Line 77: How did you determine the Reynolds number?

Thank you for pointing this out, it is true we did not include enough details. During our revised literature review, we highlighted key research about Reynolds number and included it. The Reynolds number Re is calculated from the seawater density, its tangential velocity, the bottle diameter (0.09 m) and water viscosity (Griskey, 2002). The fluid’s tangential velocity is calculated as $2\pi \cdot \text{radius} \cdot (\text{RPM}/60)$ with radius being the radius of the shaking table rotation (here 13 mm) and RPM/60 the rotation per second (here $125/60 = 2.08$). The seawater density is calculated at 1023 kg m^{-3} (Millero & Poisson, 1981) for salinity 35.346 and average temperature of $24.6 \text{ }^{\circ}\text{C}$, and viscosity at $9.68 \cdot 10^{-4} \text{ Pa s}$ (Sharqawy, 2002).

Hence, we obtain: $Re = \frac{1023 \times (2\pi \cdot 0.013 \cdot 2.08) \times 0.096}{9.68 \times 10^{-4}} = 1.7 \times 10^4$

The edited section now reads: “Turbulence can be expressed as a dimensionless number which is also used in physical oceanography, known as the Reynolds number (Reynolds, 1883). To compare turbulence in our experimental setup to naturally occurring ocean turbulence, we derived the Reynolds number from the bottle diameter, tangential velocity, seawater density and dynamic viscosity, as per Griskey (2002). Our conditions yielded a Reynolds number approximately 1.7×10^4 , above the threshold considered for turbulent systems (Millero and Poisson, 1981; Thorpe, 2007; Sharqawy, 2010). This setup was used for a total of six months to assess the long-term response of alkalinity addition to seawater.” (lines 100-106)

Line 80ff: “The bottles were cleaned... “: This is oddly detailed. Not bad per se, but also not necessary. Additionally: If you want to keep this sentence, place it after the section with the sea water. That is logically easier to follow: 1: Collect water, 2: Prepare bottles. 3: Set up experiment.

This is a fair point, though we have decided to keep the details as it was requested by a previous reviewer. However, we will follow this reviewer’s comments on the order. It now reads: “About 90 L of natural Indian Ocean seawater was collected from the surface ~400 km southeast of Sri Lanka (2°59.995'N, 77°59.997'E) and stored in five 18 L borosilicate 3.3 glass bottles. The bottles were stored in the dark in a refrigerated container until delivery ~10 weeks after collection. The entire amount of seawater was sterile filtered using a Whatman Polycap 36 AS encapsulated filter with a 0.2 µm pore size. The seawater was stored in a cleaned and MilliQ-rinsed 210 L PP rainwater tank (Garantia rainwater green round barrel). The seawater was bubbled for a week using MilliQ-saturated ambient air following Moras et al. (2023) to allow for the entire content of the tank to be equilibrated with atmospheric pCO₂. The experiments were conducted in 1 L PP bottles that were cleaned in a 3-step process. They were first soaked overnight in a 1% solution of NaOH with dishwashing tablets. The next day, the bottles were rinsed with tap water three times and soaked overnight in a 0.5% solution of HCl. Finally, all bottles were rinsed with tap water and MilliQ (18.2 MΩ) three times each, before being dried overnight in an oven at 60 °C.” (lines 80-89)

Line 84/85: Sea water was collected and filtered: When? Directly after collection? How long was it stored before the experiment? What depth was it collected from?

Thank you for pointing this out, we edited as follows: “About 90 L of natural Indian Ocean seawater was collected from the surface ~400 km southeast of Sri Lanka (2°59.995'N, 77°59.997'E) and stored in five 18 L borosilicate 3.3 glass bottles. The bottles were stored in the dark in a refrigerated container until delivery ~10 weeks after collection” (lines 80-82)

Line 89: “One major issue when it comes to open bottles is evaporation. To compensate for such loss...”: Shorten to “To minimize evaporation,”

This was edited as suggested (line 107).

Line 89/90: “...was provided in all bottles using a similar process to the one described above for the seawater.”: That sounds like a lot of redundant words: Why not just say „In order to avoid evaporation the headspace of the open bottles was continuously replaced with watersaturated air. For this purpose....“. And then a brief explanation of what the setup looked like.

Thank you for the suggestion, we incorporated it as follows: “To minimize evaporation, the headspace of the open bottles was continuously replaced with water-saturated air. For this purpose, air was bubbled

through MilliQ contained in a sealed 1 L Schott bottle. Once the air was bubbled through the MilliQ, it was carried over into plastic airflow dividers similar to those used for aquariums (Fig. 1).” (lines 107-109)

Line 94/95: “allowing for easier comparison of results from bottles as they were all exposed to the same conditions.”: This would also have been true (within error) if the headspace had not been modified.

Also: Have you measured relative humidity in the air? And: Have you measured the flow rate at each outlet? Experience tells me that it is not easy to construct a gas distributor that provides the same flow rate at every outlet. A photo of the device would be very helpful.

We agree that measuring airflow and relative humidity at each outlet would have helped characterise the setup. Unfortunately, these variables were not measured, and the airflow may therefore have differed among outlets. We added such a limitation to the manuscript and included the photograph of the tubing and air splitter configuration in the appendix. The supplied air was intended to renew each bottle’s headspace with atmospheric CO₂ rather than impose a controlled wind speed.



Figure 1: Photo of the experimental setup, showing how the tubing was attached to the bottles, with the air splitter in the background.

Line 96: “to ensure that evaporation was not too important”: This is not an appropriate wording. Please rephrase this to sth. like “...2-3 days). Evaporation-induced weight loss was compensated for by...”

Thank you for the suggestion, the section was edited to: “To compensate for evaporation, each bottle was regularly weighed, and accurate MilliQ additions were performed when a decrease in weight of more than 5% was observed.” (lines 111-113)

Line 98/99: “Another issue that was raised before the start of the experiments was the risk of organisms growing in the bottles. Hence, experiments were mostly conducted in the dark”: Same pattern as before: Many unnecessary words. „To reduce the potential growth of autotrophic organisms, the experiments were conducted...”

Thank you for the suggestion. We followed the recommendations and changed the sentences to: “To reduce the potential growth of autotrophic organisms, two germicidal UV lamps emitting at 254 nm were mounted on top of the setup and placed so that all bottles were exposed to the UV lights. These lamps were automatically switched on for an hour twice a week in the middle of the night, allowing for a long enough exposure to limit microbial growth.” (lines 113-115)

Lines 110-112: I thought this had happened already? Please reorganize the methods thoroughly so that there is a chronological order. Right now, it’s quite a lot of jumping back and forth. How many of each treatment and controls? How much powder? How much solution?

We understand the concerns and have changed the methods chronology. It now introduces the seawater collection, bottle cleaning process and NSW filling. Then it describes the setup, first by detailing the choice of turbulences and corresponding Reynolds number, then by detailing the risk for evaporation from said motion. Once the setup was fully described, the second section addresses alkalinity manipulations and the start of the incubations.

Line 113/114: “+100 $\mu\text{mol kg}^{-1}$ to +600 $\mu\text{mol kg}^{-1}$ with 100 $\mu\text{mol kg}^{-1}$ TA increments for both alkaline powders, and only one set of +600 $\mu\text{mol kg}^{-1}$ using NaOH.”: After the powder had dissolved? why the 125 rpm then?

Thank you for pointing out this confusion, we did not communicate the idea correctly and have edited the section. There have been three treatments:

- one with $\text{Ca}(\text{OH})_2$ and increasing TA amendments from +100 to +600 $\mu\text{mol kg}^{-1}$ (increments of 100)
- one with $\text{Mg}(\text{OH})_2$ and increasing TA amendments from +100 to +600 $\mu\text{mol kg}^{-1}$ (increments of 100)
- one with only 3 bottles NaOH stock solution with one TA amendment of +600 $\mu\text{mol kg}^{-1}$.

As suggested below, we rephrased and incorporated a table. It now reads: “Each 42-bottle experimental set consisted of three control bottles filled only with sterile-filtered seawater, 18 TA-enhanced bottles with $\text{Ca}(\text{OH})_2$ powder, 18 TA-enhanced bottles with $\text{Mg}(\text{OH})_2$ powder, and three TA-enhanced bottles with a stock solution of NaOH (Table 1). Every treatment was conducted in triplicate. Targeted alkalinity increases ranged from +100 $\mu\text{mol kg}^{-1}$ to +600 $\mu\text{mol kg}^{-1}$ in increments of 100 $\mu\text{mol kg}^{-1}$ for both alkaline powders, and the NaOH-amended set consisted of a TA increase of +600 $\mu\text{mol kg}^{-1}$. One bottle was lost from the 125 RPM NaOH treatment after day 6, and one was lost from the 0 RPM NaOH treatment after day 108. Subsequent values therefore represent the means of the two remaining replicates.” (lines 123-128)

Line 114 – 118: This is very confusing, please provide a table or a figure to visualize this.

We followed the advice and provided the following table (line 137):

Table 1: Experimental design for one set of 42 bottles, showing the treatment groups (control, Ca(OH)₂ powder, Mg(OH)₂ powder and NaOH stock solution), amendment levels (expressed in $\mu\text{mol kg}^{-1}$), and the number of replicate bottles per treatment.

Treatment	+0	+100	+200	+300	+400	+500	+600
Control	3	–	–	–	–	–	–
Ca(OH) ₂	–	3	3	3	3	3	3
Mg(OH) ₂	–	3	3	3	3	3	3
NaOH	–	–	–	–	–	–	3

Lines 123 – 125: Please provide an overview of the amounts added to the different treatments.

We reshaped the paragraph for a better flow and incorporated the weights range. It now introduces the alkaline materials and supplier earlier, followed by their grain size distribution and weighing. It now reads: “The alkaline powders Ca(OH)₂ and Mg(OH)₂ and the NaOH pellets used to make the 1 M NaOH solution were all reagent-grade, obtained from Carl Roth (Germany). The Ca(OH)₂ and Mg(OH)₂ powders were analysed for grain size distribution with a Bettersizer S3 Plus, exhibiting similar D50 values, i.e., 31.98 and 29.79 μm , respectively, as well as comparable specific surface areas, i.e., 58.90 and 62.82 $\text{m}^2 \text{kg}^{-1}$. Both powders were dried overnight at 60 °C, and specific amounts for TA amendments were weighed on weighing paper using a Kern ABT 120-5DNM ($\pm 0.01 \text{ mg}$) balance. Ca(OH)₂ additions ranged from 3.67 to 22.26 mg L^{-1} , while Mg(OH)₂ additions ranged from 2.88 to 17.54 mg L^{-1} to reach the desired ΔTA targets. The NaOH solution was added using an Eppendorf Research Plus 1000 pipette. Overall, the weighing uncertainty led to an offset between the targeted and estimated TA of less than 1%” (lines 128-136)

We also added the following table to the appendix:

Table A 1: Amount of material added per treatment and target TA (mean of triplicates $\pm$ standard deviation) in mg L^{-1} .

Treatment	+100	+200	+300	+400	+500	+600
0 RPM Ca(OH) ₂	3.67 $\pm$ 0.06	7.36 $\pm$ 0.11	11.09 $\pm$ 0.03	14.85 $\pm$ 0.05	18.51 $\pm$ 0.07	22.26 $\pm$ 0.09
0 RPM Mg(OH) ₂	2.88 $\pm$ 0.05	5.85 $\pm$ 0.09	8.72 $\pm$ 0.06	11.71 $\pm$ 0.07	14.57 $\pm$ 0.04	17.46 $\pm$ 0.07
125 RPM Ca(OH) ₂	3.70 $\pm$ 0.08	7.47 $\pm$ 0.01	11.10 $\pm$ 0.08	14.81 $\pm$ 0.02	18.51 $\pm$ 0.06	22.24 $\pm$ 0.08
125 RPM Mg(OH) ₂	2.95 $\pm$ 0.04	5.84 $\pm$ 0.10	8.73 $\pm$ 0.06	11.61 $\pm$ 0.05	14.63 $\pm$ 0.12	17.54 $\pm$ 0.10

Lines 126 – 134: This section needs thorough revision: Too much unnecessary information (14-gauge needle), and a lot is missing: How was pH measured? What scale are values reported on? What total amount of sample was collected? Was the volume replaced?

We have removed some unnecessary detailed steps as suggested. Also, following the comments below, we have moved the pH measurements description earlier with all respective information. It now reads: “The pH, temperature and conductivity of each sample were measured before manipulation and one, three and six days after alkalinity addition. Thereafter, measurements took place twice a week for the first two months and once a week afterwards. pH and conductivity were measured using a multiparameter pocket measuring device MultiLine Multi 3630 IDS from WTW, with a SenTix 940 pH probe and a WTW TetraCon 925 conductivity probe. The pH probe was calibrated weekly using NIST-traceable buffers and measured values were converted to the total pH scale (pHT) using measured salinity and temperature and inputting the values into the equation derived by Badocco et al. (2021). The conductivity probe was calibrated before the first measurement to determine the starting salinity of the seawater. Salinity was calculated as 35.346 ± 0.077 on the practical salinity scale of 1978 (Lewis and Perkin, 1981).” (lines 141-148)

Water replacement is also addressed in the material and methods section (“leaving less than 150 mL at the end of the experiment, as the sampled water was not replaced.” at line 156) and in the limitations paragraph (“Second, repeated sampling progressively reduced the volume in each bottle, changing the headspace and potentially influencing gas exchange over time, although all treatments underwent the same sampling schedule. In addition, airflow rate and relative humidity were not measured at the individual outlets, and differences in headspace renewal among bottles therefore cannot be excluded.” at lines 626-629)

Lines 136 – 138: Ahhh here it comes. Why the paragraph above?

See comment above.

Line 138: “as per Badocco”: Quickly describe how that is done. A seawater buffer?

Thank you for the question. The work of Badocco et al. (2021) provides a conversion equation from the measured pH on the NIST scale into pH values on the total scale. The equations are derived for their experimental work and correlate the two scales based on the measured salinity and temperature of the sample. We have emphasised that we used “measured values were converted to the total pH scale (pHT) using measured salinity and temperature and inputting the values into the equation derived by Badocco et al. (2021)” (lines 145-146)

Line 139: What do you mean by “estimated”. Please describe measurements with appropriate references.

Thank you for pointing this out, this is a wrong wording, we changed to “calculated” (line 147).

Line 159: Expectations do not go into the method section. Please just describe what you did.

This is a fair point and removed this sentence.

Line 160: “Scanning electron microscopy (SEM) analyses were conducted on a tabletop Hitachi TM4000 II, connected to a Bruker QUANTAX energy-dispersive x-ray spectroscopy system (EDS)”: This sentence is somewhat misplaced. Please report the methods in a chronological order. What sample was taken in what way and then what was measured on which device.

We understand the confusion. The section has been reshaped to first address the manipulations and the various sampling (“TA manipulations and sampling procedure”) before discussing the measurement procedures in a second subsection (“Carbonate chemistry measurements, mineral analyses and data processing”).

Line 162: What do you mean by remaining?

Thank you for pointing this out. Following a previous comment, we gave estimates of the amount of water remaining in the bottles after successive sampling which resulted in less than 150 mL remaining in each bottle. From these, SEM and F-AAS filters were collected (lines 157).

Line 163: Rinsed for how long? Was the MQ pH neutral? Usually, it’s corrosive.

We thank the reviewer for requesting clarification. The filters were rinsed for several seconds with only a few millilitres of freshly produced MilliQ water under vacuum to remove residual salts. We have added the rinse duration and purpose to the revised manuscript (lines 159-160). However, we avoided assigning a specific pH value to the MilliQ water because it was not measured directly.

Line 165: What does “higher treatment” mean? Please rephrase.

This is a good point and we reformulated as: “water was filtered through Whatman polycarbonate Nuclepore filters (0.2 μm) from bottles amended to a target ΔTA of +600 $\mu\text{mol kg}^{-1}$ ” (lines 158-159)

Line 166a: “could have been used for CaCO_3 precipitation.”: Do you mean that CaCO_3 might have formed around $\text{Mg}(\text{OH})_2$ particles? If so, say that, please.

Yes this was the original idea. We edited as: “The second set of filters stored in Suprapure nitric acid was used to determine the concentrations of solid Ca and Mg, to assess whether CaCO_3 precipitated on undissolved $\text{Mg}(\text{OH})_2$ particles. Analyses were performed using flame atomic absorption spectroscopy (F-AAS) on a Thermo Fisher Solaar S Series.” (lines 186-189)

Line 166b ff.: „the case where CaCO_3 would have precipitated on yet to be dissolved $\text{Mg}(\text{OH})_2$ particles, a clear signal of Mg^{2+} would be observed. Otherwise, if CaCO_3 precipitated from the water column only, the Mg^{2+} signal would be absent.“: This sentence belongs to the discussion section.

We agree and removed this from the methods section.

Line 172a: Why HNO_3 and not HCl ?

This was performed following in house standard operating procedures as well as recommendations from the instrument manufacturer, Thermo Fisher, for sediments and total metal elements analyses. (see page 4, <https://documents.thermofisher.com/TFS-Assets/CMD/Technical-Notes/tn-44483-aas-icp-oes-icp-ms-sample-prep-regulated-test-tn44483-en.pdf>, as per U.S. EPA SW-846 Method 3010A).

Line 172b: “try and”. Delete that.

We agree and did as suggested.

Line 173c: Rather remove *from* the wall?

This is a valid point and edited accordingly (line 163).

Lines 174 – 176: Please clarify what you mean by this, what TA loss is referred to and please provide a formula that was used to carry out the calculations including an explanation of each variable used in the formula.

We understand that this section is confusing. We have fully rewritten this section with a more detailed step by step calculation. The new paragraph now reads: “A TA generation efficiency metric was calculated by dividing the measured ΔTA after 24 hours by the targeted ΔTA . This %TArealised can be used to compare the dissolution efficiency of the minerals based on the treatment. After six months, a TA budget was derived from the carbonate chemistry measurements and mineral analyses. Measured TA loss (Meas. TALoss) was calculated as the difference between the TA value at day 0 and the lowest measured TA value. Similarly, a theoretical maximum ΔTA (Max. ΔTA_{theo}) term was calculated using the mineral molar mass and the amount of material added in mg L⁻¹ (assuming complete dissolution). From both estimates, a maximum TALoss term (Max. TALoss) was calculated from the difference between the Max. ΔTA_{theo} and the lowest recorded ΔTA . An estimate of particulate Ca concentration was derived from Max. TALoss with each mole of TA loss corresponding to 0.5 mol CaCO₃” (lines 226-233)

Line 179/180: “Throughout the experiments, quality control of the carbonate chemistry measurements was conducted using the TA, DIC and pH_T data from each set to prevent loss of data to unexpected instrument misbehaviour.”: This sentence is redundant. Please delete.

We agree and removed the sentence as suggested.

Line 180/181: “The DIC measurements from months 1 and 2 were likely underestimated, while the DIC measurements from month 6 were likely overestimated (Fig. 2).”: This sentence belongs to the discussion along with an explanation why the authors come to this conclusion.

We agree and have moved it to the relevant section.

Lines 180 – 190: This entire section belongs to the discussion. The method section should only describe what has been done. Figure 2 Belongs to the results section.

We agree and have created a new subsection in the results section.

Line 197: This section along with the next one should be clearly marked as only referring to the solid phase experiments.

Thank you for pointing this out, the subsection heading has been edited accordingly (“Differences between expected and measured alkalinity using solid feedstocks”, line 235) and more details added in

the subsection itself, such as: “Alkalinity additions for each experiment were performed identically with the two solid feedstocks, with ΔTA targets varying from +100 to +600 $\mu\text{mol kg}^{-1}$ ” (lines 236-237)

Line 199/200: “Within these first 24 hours, stark differences emerged between the material used and the advection regime.”: In what way can there be a difference between a material and a reaction regime. Please rephrase this sentence.

We understand and edited as: “Stark differences emerged in realised TA based on the material used and the advection regime within the first 24 hours.” (lines 238-239)

Line 199/200: “The $\% \text{TA}_{\text{realised}}$, i.e., net measured ΔTA in $\mu\text{mol kg}^{-1}$ divided by the targeted ΔTA in $\mu\text{mol kg}^{-1}$...”: This term needs explanation and the formula for its calculation needs to be presented and explained.

We agree and introduced the concept of efficiency term and described the calculation in the material and methods section: “A TA generation efficiency metric was calculated by dividing the measured ΔTA after 24 hours by the targeted ΔTA . This $\% \text{TA}_{\text{realised}}$ can be used to compare the dissolution efficiency of the minerals based on the treatment.” (lines 226-227)

Lines 202 – 203: “Advected bottles led to...”: How much of the added material has hence dissolved? How did the saturation state with respect to the materials change? After what time is $\text{TA}_{\text{realised}}$ calculated? Is incomplete dissolution or already precipitation? In addition: Bottles do not lead to anything. In bottles with advection higher values were observed.

We have edited the formulation as suggested, and added examples of calculations, such as: “In the advected bottles, TA generation was higher [...]. For example, in the advected bottles with a ΔTA target of +100 $\mu\text{mol kg}^{-1}$, TA increased by 55-58 $\mu\text{mol kg}^{-1}$ with $\text{Ca}(\text{OH})_2$ and $\text{Mg}(\text{OH})_2$, respectively, corresponding to $\% \text{TA}_{\text{realised}}$ values of 55-58%. However, TA generation in static bottles with a ΔTA target of +600 $\mu\text{mol kg}^{-1}$ only yielded TA increases of 20-45 $\mu\text{mol kg}^{-1}$ with $\text{Ca}(\text{OH})_2$ and $\text{Mg}(\text{OH})_2$, respectively, corresponding to $\% \text{TA}_{\text{realised}}$ values ranging from 3 to 8% (Figs. 2 and 3).” (lines 239-244)

These % term therefore indicates how much of the material dissolved within the first 24 hours. This subsection only focuses on the efficiency and does not present the absolute values yet. These are presented in the next subsections, i.e., “Long-term stability of alkalinity” and “CO₂ fluxes and derivation of equivalent wind speeds from DIC measurements”. It is intended to only present the short-term alkalinity generation efficiency, i.e., within the first 24h. As mentioned by the reviewer, these values cover both incomplete dissolution of the feedstock and secondary precipitation. These are later discussed using the 24h DIC values, initially presented in the discussion but now moved up into the results section (see: “CO₂ fluxes and derivation of equivalent wind speeds from DIC measurements”). Given the complex processes operating in opposite directions (i.e., dissolution and ingassing increasing TA and DIC, and precipitation decreasing them), we decided to only refer to a net efficiency term. However, these observations have been added and discussed in more details in the discussion to incorporate the reviewer comment.

Lines 205/207: “It appears that the measured TA...”: Either it is lower or not. It does not appear to be something. Such formulations can be used in the discussion section, certainly not in the results part.

We agree and removed the start of the sentence.

Line 214: Figure caption: "...after 1 day.": This explanation needs to appear in the main text.

We agree and have emphasised it earlier, such as: "Stark differences emerged in realised TA based on the material used and the advection regime within **the first 24 hours.**" (lines 238-239)

Figure 4: Please add the control here. That makes it easier to understand pCO₂ values.

The controls are represented by the dotted black line at 0 as they are subtracted to all the values to only report measured Δ TA. We have incorporated this information to the caption (lines 284-285) to address the reviewer comment: "Changes in TA (Δ TA) as a function of time and expressed in $\mu\text{mol kg}^{-1}$, calculated by subtracting the same-day control TA from measured TA (the controls are represented by the black dotted line).".

Lines 272/273: "While both setups are meant to be identical...": They are clearly not. Recent research that some of the authors were involved with have demonstrated that liquid addition differs significantly from solid phase addition.

This is a fair point, though this sentence only refers to the NaOH additions. They were performed identically but shows different patterns whether they were advected or not. We understand the confusion and removed the confusion starting sentence. It now reads: "NaOH additions were performed identically in static and advected bottles. However, a clear difference in Δ TA emerged." (line 350)

Line 273: "clear difference in Δ TA emerges": Difference between what and what? The two solid state experiments were already different. Please rephrase for clarity.

We apologise for the misunderstanding. This only refers to the NaOH experiments. We removed mentions of the experiments with solid feedstocks.

Lines 291/292: "particles were used as surface area for CaCO₃ precipitation": Why not simply say whether CaCO₃ had formed on Mg(OH)₂ particles? „Used“ sounds odd here.

Following recommendations, we edited as: "to determine whether CaCO₃ precipitated onto Mg(OH)₂ particles." (line 370)

Line 292/293: "The sample for the third replicate of the 0 RPM Mg(OH)₂ treatment was lost during analysis, and not all interpretations could be computed in Table 1 for this sample in particular": This sentence appears out of nowhere. transfer it to the table caption or a point in the main text after Table 1 has first been mentioned for the data it contains.

We agree and added it to the caption as suggested "The third replicate of the 0 RPM treatment was lost during analysis. Hence, not all values could be calculated for this sample." (lines 382-383).

Line 293: "The data": Which data?

We understand and ultimately deleted it following the comment below.

Lines 293 – 297: This is all interpretation. Only report results in the result part, please. Move it to the discussion or delete this section.

We agree and reformulated to only discuss results, such as: “The Max. TA loss values for the 0 RPM treatment were higher than those for the 125 RPM by about 194 $\mu\text{mol kg}^{-1}$ (Table 2). The results obtained from F-AAS reveal higher Ca^{2+} and Mg^{2+} concentrations in the 0 RPM treatments, with values about 7.2 and 0.4 mg L⁻¹ higher, respectively.” (lines 370-373)

Line 298: “the 0RPM”: The 0 RPM what? a revolution per minute cannot really have Mg^{2+} . Please use precise scientific language.

We have reformulated as: “in the 0 and 125 RPM **treatments**, respectively. Finally, another major difference emerged from the measurements of Mg^{2+} , where the 0 RPM **treatment** yielded nearly twice as much particulate Mg^{2+} as in the 125 RPM **treatment**” (lines 374-375)

Line 301: Table1: What does this table have to do with the section title? This should go to a respective discussion part. And please explain every parameter listed in the table and provide formulas how they were calculated in the method section.

We agree with the calculations formulas and have provided them in the methods as suggested earlier. This table refers to the TA loss terms described in the last subsection of the methods (“Carbonate chemistry measurements, mineral analyses and data processing”) as well as the results from the particulate analyses conducted with F-AAS which allowed us to have measured particulate Ca^{2+} and Mg^{2+} concentrations. We have changed the subsection title to: “TA loss budget, particulate analysis and scanning electron microscopy”

Line 323: “Considering real-world applications for OAE is of high interest but is yet to be fully addressed.”: This sentence as it is, is completely redundant. Please delete or fill with meaning. Addressing a consideration does not make much sense.

We followed the reviewer comment and deleted the sentence.

Lines 324/325: “more classical experimental setup available in the literature”: What on earth is “classic” supposed to mean here? Likewise “available in the literature” is a weird wording. On top of that the authors should know that there are very well long-term experiments on OAE published in peer reviewed journals.

We understand the wrong formulation and have avoided any emphasis on “replicating real world conditions” but proceeded with a more careful and appropriate formulation. It now reads: “A first clear outcome is that the alkalinity generated through $\text{Ca}(\text{OH})_2$ and $\text{Mg}(\text{OH})_2$ dissolution using a shaking table was substantially lower than reported in the literature (Hartmann et al., 2023; Moras et al., 2022; Moras et al., 2024; Varliero et al., 2024; Yang et al., 2023). Such an outcome was expected as the weaker turbulence generated by the shaking table compared with that produced by a stir bar, for example, could have decreased fluid exchange around the dissolving particles and enhanced their settling.” (lines 401-405)

Regarding the literature comment by the author, we are not aware of many long term (several months) abiotic laboratory experiments for OAE in particular. Below are 5 of interest 2 were in duration, i.e., ~4 and ~12 months, though it is important to note that the first one was run with artificial seawater and partly open to the air, while the second was a benthocosm experiment with sediments where processes such as bioactivity and oxidation affect dissolution/precipitation kinetics. Though hard to compare to abiotic focused research, they have been incorporated in the text as they provide key process understanding for our research (lines 63-64 and 538 for example):

- Fuhr et al., 2023: <https://doi.org/10.3389/fclim.2023.1245580> ~60 days
- Flipkens et al., 2023: <https://doi.org/10.1016/j.gca.2023.09.002> ~70 days
- Montserrat et al., 2017: <https://doi.org/10.1021/acs.est.6b05942> ~88 days
- Fuhr et al., 2022: <https://doi.org/10.3389/fclim.2022.831587> ~130 days
- Fuhr et al., 2025: <https://doi.org/10.1038/s43247-025-02079-6> ~345 days

Lines 325/326: “intentional atmospheric CO₂ invasion.”: Was the unnaturally high pCO₂ in the atmosphere as indicated by the control pCO₂ intended?

This is a fair point. The “high” pCO₂ was somewhat intentional or at least unavoidable as it was the laboratory pCO₂. Given it was conducted in a closed room, the pCO₂ was slightly higher than in the main laboratory which usually varies around 500-600 µatm. Nevertheless, it does not affect the interpretation of the results.

Line 329: Expectations do not go into the results part. Delete this sentence.

We did accordingly.

Lines 329 – 330: Clarify this statement: How does this affect the diffusion speed? To my knowledge the actual diffusion speed is always the same. The flux is just higher if ΔC is higher or the diffusive distance is shorter if the dbf is thinner.

This is a good point, and we did not express our ideas correctly. We wanted to emphasize that the turbulence generated by a stir bar promotes stronger particle resuspension. Consequently, dissolution is likely to be less limited by mass transfer at the particle surface than in experiments conducted on a shaking table, which may explain the higher dissolution efficiencies reported in studies using stir bars. We reformulated this section to incorporate the concept of mass transfer and it now reads: “In our case, the low (125 RPM, Reynolds number of $\sim 1.7 \times 10^4$) or absent (0 RPM) turbulence coincided with a decrease in TA generation efficiency and suggest mass-transfer limitations (Drever, 1997; Saez Moreno et al., 2026). [...] Taken together, these findings and previous studies using hydroxide and olivine-rich feedstocks suggest that hydrodynamic conditions are a first-order control on realised alkalinity generation (Flipkens et al., 2023; Fuhr et al., 2025; Meysman and Montserrat, 2017). Expected alkalinity generation should therefore not be simply inferred from the alkaline material properties but rather a combined result of material properties and surrounding hydrodynamic regime. Environments with strong advection and/or physical disturbance, such as strong porewater exchange or water flow generating high bed shear stress, may therefore be more suitable, as previously suggested for olivine-rich materials (Flipkens et al., 2023; Meysman and Montserrat, 2017).” (lines 409-423)

Lines 331/332: “Hence, TA generation was up to 50% lower at 0 RPM for the lower additions, due to the slower dissolution and higher saturation in the...”: A thicker dbf explains a time lack in alkalisation,

not a lower final alkalinity. Please rephrase this. Add: Depending on the conceptual model of a dbf, the saturation state in the dbf is always the same. The gradient is just steeper in this dbf.

We agree and added: “In our case, the low (125 RPM, Reynolds number of $\sim 1.7 \times 10^4$) or absent (0 RPM) turbulence coincided with a decrease in TA generation efficiency and suggest mass-transfer limitations (Drever, 1997; Saez Moreno et al., 2026). At 0 RPM, TA generation was up to 50% lower for the lower additions. At the same time, the persistence of such concentrated micro-environments increased the risk of heterogeneous and pseudo-homogeneous CaCO_3 precipitation (Marion et al., 2009). The competing processes of reduced dissolution and increased CaCO_3 precipitation likely explain the substantially lower measured alkalinity increase, at most half of reported values in literature, especially in the 0 RPM incubations. Although we did not directly measure such chemical gradients and micro-environments, the dissolution and precipitation patterns are consistent with the behaviour expected from thicker diffusive boundary layers surrounding dissolving particles (Lasaga, 2014; Levich, 1962).” (lines 409-417)

Line 332: The concept of a diffusive boundary layer needs to be explained and backed up with an appropriate citation.

This is a fair point and we added a definition before: “Consequently, the dissolution was likely limited by mass transfer through the diffusive boundary layer surrounding particles (Lasaga, 2014; Levich, 1962; Saez Moreno et al., 2026; Sulpis et al., 2019). The reduced advection could have increased the thickness of the diffusive boundary layer surrounding particles, leading to micro-environments enriched in Ca^{2+} , Mg^{2+} and OH^- . These could have reduced the concentration gradient driving diffusion and ultimately decreased dissolution rates.” (lines 405-409)

Line 333a: What disturbance to you refer to?

We have added more information and linked it to existing literature, which now reads: “Taken together, these findings and previous studies using hydroxide and olivine-rich feedstocks suggest that hydrodynamic conditions are a first-order control on realised alkalinity generation (Flipkens et al., 2023; Fuhr et al., 2025; Meysman and Montserrat, 2017). Expected alkalinity generation should therefore not be simply inferred from the alkaline material properties but rather a combined result of material properties and surrounding hydrodynamic regime. Environments with strong advection and/or physical disturbance, such as strong porewater exchange or water flow generating high bed shear stress, may therefore be more suitable, as previously suggested for olivine-rich materials (Flipkens et al., 2023; Meysman and Montserrat, 2017).” (lines 417-423)

Line 333b: Flipkens 2023 proved that grain to grain interaction can foster olivine dissolution, but the conceptual idea is older. Meysman & Montserrat 2017 should be cited here.

We have incorporated the citation as suggested (lines 419 and 423).

Lines 338/339: Strictly speaking this is not an open ocean scenario. To argue in that direction, you need to refer to the Ω values in your experiments.

We agree and removed “open ocean”

Line 342: "...likely led to...": Without evidence in the form of particles or at least a drop in Mg concentrations, I would rephrase this to something like „Mg(OH)₂ formation and subsequent redissolution would therefore explain...”

We agree and reformulated similarly to the reviewer's suggestion, as: "Similar patterns were observed in the literature and attributed to Mg(OH)₂ formation following NaOH addition, both experimentally at pH above ~10 (Haas, 1916; Kapp, 1928) and in the context of OAE (Hashim et al., 2025; Shaw et al., 2025; Suitner et al., 2024; Varliero et al., 2024). The progressive increase in TA to 100% after three months is consistent with gradual dissolution of temporarily precipitated Mg(OH)₂ (Haas, 1916; Hashim et al., 2025; Kapp, 1928)." (lines 427-431)

Line 343: Which implications would that be?

We introduced the concept of efficiency loss, and the difference between temporary and permanent losses: "Secondary mineral formation has recently been considered a loss in TA efficiency for OAE (Lunstrum et al., 2026). However, this transient Mg(OH)₂ formation may represent a temporary reduction in alkalinity generation rather than a permanent loss of CDR potential" (lines 431-433)

Lines 347 & 348: Repeating "runaway fashion". Please rephrase.

This is a good point, we reformulated as: "The concomitant decrease in TA and DIC suggests that CaCO₃ precipitation after NaOH addition became the dominant process, likely obscuring the signal of subsequent Mg(OH)₂ dissolution (Fig. 7)." (lines 435-436)

Line 350: "Like in the NaOH experiments, CaCO₃ precipitation was suspected in the experiments with solid feedstocks": This sentence is redundant. Expectations make our lives easier, but are not an argument for or against anything. Please delete.

We agree and removed the sentences

Lines 350/351: "The creation of highly alkaline microenvironments led to inevitable secondary CaCO₃ precipitation.": As long as you cannot measure or visualize these microenvironments you should use a more careful wording here.

Thank you for pointing this out, we reformulated as: "The reduced advection could have increased the thickness of the diffusive boundary layer surrounding particles, leading to micro-environments enriched in Ca²⁺, Mg²⁺ and OH⁻. [...] At the same time, the persistence of such concentrated micro-environments increased the risk of heterogeneous and pseudo-homogeneous CaCO₃ precipitation (Marion et al., 2009)." (lines 407-413). This was also emphasised later, such as: "Although we did not directly measure such chemical gradients and micro-environments, the dissolution and precipitation patterns are consistent with the behaviour expected from thicker diffusive boundary layers surrounding dissolving particles (Lasaga, 2014; Levich, 1962)." (lines 415-417)

Lines 351/352: "Furthermore, the low to inexistent advection led to longer CAP, allowing for more CaCO₃ to be formed": Why exactly? Please explain this in more detail.

We understand and addressed the comments at the same time as the previous reviewer's comments, lines 407-417.

Line 352/354: „The presence...“ : This sentence just repeats what has just been said.

This is a good point, we removed the sentence

Lines 354/355: “In all experiments, CaCO₃ formation was suspected, with a clear decrease in DIC after addition in the 0 RPM experiments, and a loss in DIC for all levels above 300 μmol kg⁻¹ at 125 RPM.”: What do you mean by "was suspected"? Either there is evidence supporting it or not.

This is true, we changed it to a more cautious formulation such as “The concomitant decrease in TA and DIC suggests that CaCO₃ precipitation after NaOH addition became the dominant process, likely obscuring the signal of subsequent Mg(OH)₂ dissolution (Fig. 7).” (lines 435-436)

Line 355/356: „This was confirmed by SEM imaging (Fig. 7) and particulate analysis (Table 1).”: Please explain in what way that was supported by these data.

We agree and edited later in the text as: “SEM-EDS analyses were consistent with the formation of CaCO₃ and revealed differing morphologies depending on the alkaline material used (Fig. 8).” (lines 481-482)

Line 357: Which data support that and in what way?

We reformulated the section to improve the flow, and now reads: “In contrast, the slower dissolution of Mg(OH)₂ seems to favour slow-growing, denser broccoli-shaped structures. The higher Ca²⁺ and Mg²⁺ concentrations measured in the precipitates from the 0 RPM experiments support the interpretation that CaCO₃ coated Mg(OH)₂ particles (Table 2). Such coatings could simultaneously reduce dissolution efficiency while increasing particle density and thereby promoting export of undissolved Mg(OH)₂ particles from the mixed layer.” (lines 545-549)

Line 358: What do you mean by the signal of Mg? Please clarify.

It referred to the concentration of magnesium in the precipitates. We reformulated to “The higher Ca²⁺ and Mg²⁺ concentrations measured in the precipitates” (lines 546-547)

Line 359: If it was not confirmed via SEM it is rather possible than likely.

We agree and reformulated to: “support the interpretation that CaCO₃ coated Mg(OH)₂ particles” (lines 547-548)

Line 363: Which external factor? Just name it, please.

We have reworked the whole discussion and argue such concept later on: “Once CO₂ ingassing occurs, the resulting DIC increase hides this stoichiometric relationship, making the quantification of realised

TA more difficult. This represents an important challenge for monitoring, reporting and verification (MRV) efforts during field-scale OAE deployments.” (lines 518-520).

Lines 363 – 367: These sentences are stating the absolute obvious. In addition, the citation does not really make sense here. Furthermore, the ratio of 2:1 would be realistic in a scenario without a source for TA. The loss term is not clearly defined as it consists of two terms: inhibited dissolution and precipitation. This ratio is therefore not to be expected in these experiments anyways, except from the liquid addition.

We have reshaped this part, which now reads: “In principle, incomplete dissolution can be differentiated from CaCO₃ precipitation by measuring both TA and DIC in the system. Limited TA generation without a DIC decrease may indicate incomplete dissolution, while a concomitant decrease in TA and DIC in an approximate 2:1 $\Delta\text{TA}:\Delta\text{DIC}$ ratio may indicate CaCO₃ precipitation. Once CO₂ ingassing occurs, the resulting DIC increase hides this stoichiometric relationship, making the quantification of realised TA more difficult. This represents an important challenge for monitoring, reporting and verification (MRV) efforts during field-scale OAE deployments.” (lines 515-520)

Line 375/376: Please explain that further. Why would the CAP be responsible for processes in the dbf?

We agree with the comment and reviewed the concept of the CAP. The CAP itself is not a process but rather a concept or indicator. We now clearly explains what the CAP refers to (lines 52-59) and expanded on its link to CO₂ ingassing as: “The decrease and halt in the precipitation process coincided with CO₂ invasion. Under advected conditions, CO₂ invasion occurred more rapidly. When CO₂ dissolves in water, it reacts with a carbonate ion, CO₃²⁻, to form two bicarbonate ions, HCO₃⁻ (Zeebe and Wolf-Gladrow, 2001). This consumption of CO₃²⁻ decreases ΩA , which directly influences CaCO₃ formation rates (Suitner et al., 2026). These observations suggest that rapid CO₂ ingassing may have reduced the risk of runaway CaCO₃ precipitation. Consequently, more rapid CO₂ ingassing likely shortened the CAP, reducing or, in some cases, preventing runaway precipitation. Conversely, slower CO₂ ingassing in the 0 RPM experiments was associated with a longer CAP and runaway CaCO₃ precipitation (Fig. 4). Hence, the risk of loss of TA through CaCO₃ precipitation appears to depend not only on the initial amount of alkalinity and form of alkalinity added, but also on the rate of CO₂ ingassing.” (lines 454-461)

Lines 377/378: This study has not looked at dbf processes, which are relevant here. Please provide more detail on your ideas on what has happened in the experiments.

We agree and given the new information added before, we explicitly say: “Although we did not directly measure such chemical gradients and micro-environments, the dissolution and precipitation patterns are consistent with the behaviour expected from thicker diffusive boundary layers surrounding dissolving particles (Lasaga, 2014; Levich, 1962).” (lines 415-417).

Line 380/381: “Under such considerations...”: Under what consideration? and why? Please be more specific.

We agree and went straight to the point as suggested by the reviewer earlier. It now reads: “Importantly though, these observations resulted from batch and mesocosm experiments with limited CO₂ ingassing” (line 606)

Line 389a: "...in all experiments.": Did you find precipitates in your NaOH experiments?

That is a good point and it has been removed during the rewriting of the discussion.

Line 389b: What DIC fluxes? Into the bottles? Figure 8 shows concentrations, not fluxes. Can the actual fluxes even be calculated?

Yes, it refers to DIC fluxes, but more precisely CO₂ fluxes. Following earlier recommendations, we reshaped some parts of the methods and discussion. There is now a new methods section "CO₂ fluxes and wind speed derivation from DIC measurements" which addresses this comment. We also changed "DIC fluxes" to "CO₂ fluxes"

Lines 393/394: Why is that expected in Seawater? Please provide a respective reference.

This is a good point, we cited Morse et al., 1997, who states that in warmer water, aragonite is dominant. We also emphasised "All crystal morphotypes were morphologically consistent with previously reported aragonite precipitates formed in comparable seawater conditions, i.e., natural seawater at 25 °C, while EDS analyses did not reveal significant incorporation of other measured elements (Morse et al., 1997)" (lines 482-484)

Lines 416/417: Again. in the one hand side you talk about dbp processes, on the other here it's about impacts on organisms. Microbes? What effects are those? Please restructure this, so that the reader can better follow your thoughts.

We understand the comment. Initially we focused the discussion around CO₂ ingassing and its impacts on geochemical and biological processes. We have restructured the Discussion as follow:

- Effects of advection on alkalinity generation, CaCO₃ precipitation and CO₂ ingassing
- Role of alkaline feedstocks and CO₂ invasion on CaCO₃ precipitation and morphologies
- Impacts on the CDR potential and implication for MRV
- Resulting CO₂ uptake potential and environmental impacts

Line 418: What changes in the carbonate system? You can shorten sections like this one, by getting to the point directly.

We agree and directly cited "Biological responses to OAE are generally expected to arise primarily from associated changes in carbonate chemistry, including increased pH and reduced dissolved CO₂ concentration, rather than from TA increase in isolation (Bach et al., 2019)." (lines 591-592). We also shortened the three sentences by getting to the point directly.

Line 420a: Which effects? Please explain this.

We compiled the three sentences into a single one which is cited in the previous comment.

Line 420b: “Hence, the duration...” : This sentence contains the same information as the second sentence of this section, but is at the same time just as non-informative. Please restructure this section to "Observed effects, where and why do they occur and then how your new (!) findings shed a new light on the current state of research".

We agree and it was removed during the rework of this section

Line 444: “see below”: Where below? It is always a pain to have to jump back and forth in a script to understand it.

This was removed during the rework

Line 446: “One could argue that...”: I'm not going to highlight this anymore. Replace throughout the manuscript. Please. Especially, because it does semantically not make any sense. "One could argue that... , but at the same time this and that speaks against that argument".... that would be a valid construction.

We understand and have removed this formulation throughout the text as suggested

Line 463: What does usually mean here? in which studies was that done?

Such an approach can be used to identify whether dissolution was incomplete, or CaCO_3 precipitation occurred by knowing the starting conditions and the amount of alkalinity added. If the final TA is lower than expected but no change in DIC is observed, it may indicate incomplete dissolution. Otherwise, such a missing TA linked to a decrease in DIC may indicate CaCO_3 precipitation. This is only applicable for purely abiotic setups with filtered water as we have here, and without other processes changing TA or DIC. We have included such information and reformulated as “Although these processes are more readily quantified in a controlled laboratory setting, they become increasingly difficult to distinguish under open ocean conditions. In principle, incomplete dissolution can be differentiated from CaCO_3 precipitation by measuring both TA and DIC in the system. Limited TA generation without a DIC decrease may indicate incomplete dissolution, while a concomitant decrease in TA and DIC in an approximate 2:1 $\Delta\text{TA}:\Delta\text{DIC}$ ratio may indicate CaCO_3 precipitation. Once CO_2 ingassing occurs, the resulting DIC increase hides this stoichiometric relationship, making the quantification of realised TA more difficult.” (lines 514-519)

Lines 450 – 468: For this section: I am missing the comparison to real world applications. Planetary in Halifax is already using $\text{Mg}(\text{OH})_2$ for alkalising the water column. How do they manage these questions? There should be at least some citable studies out there.

This is a fair point. We have added a reference to the Isometric protocol which is the only protocol to our knowledge providing the requirements and procedures for calculating the actual CDR from OAE via coastal outfalls. The protocol does not require direct measurement of CO_2 ingassing but states the need for upscaling and modelling. It only requires the alkalinity dosing rate, the discharge rate and the discharge duration. Here we clearly show that simply assuming the amount of alkalinity added is not sufficient to quantify CO_2 fluxes. Simply modelling them may not be sufficient either as these may hide efficiency loss processes such as lack of dissolution and CaCO_3 precipitation or underestimate the contribution of CO_2 ingassing to buffer the system faster and decrease loss pathways. Recommendations are now added and it reads: “This represents an important challenge for monitoring, reporting and

verification (MRV) efforts during field-scale OAE deployments. Current MRV protocols recognise loss terms, such as biotic calcification, additionality and secondary mineral formation, but the distinction between incomplete dissolution and CaCO_3 precipitation becomes more difficult to constrain once atmospheric CO_2 ingassing alters DIC (Bach, 2024; Isometric, 2026; Lunstrum et al., 2026). To date, the first independently verified carbon credits generated from coastal OAE deployment were issued under the Isometric protocol to the Halifax-based company Planetary (Isometric, 2026; Kauk, 2025). The protocol requires monitoring of effluent characteristics, particularly pH, to ensure compliance with regulatory thresholds that must not be exceeded. In the United States, historical Environmental Protection Agency aquatic life criteria established an upper pH guideline of 8.5 and 9.0 for saltwater and freshwater ecosystems, respectively. However, the European Union does not prescribe a single pH limit for surface waters (European Parliament and Council of the European Union, 2000; U.S. Environmental Protection Agency, 1986, 2026). Our observations support the need for such a process-based approach and identify processes that should be considered in a future iteration of MRV protocols.” (lines 519-530)

Line 470: Again: Name them. Unwanted changes can be anything.

We reformulated this part and it now reads: “As mentioned earlier, current MRV protocols account for loss of TA to secondary mineral formation through a loss-term (Isometric, 2026). Hence, reliably quantifying secondary CaCO_3 precipitation remains one of the major uncertainties for OAE efficiency and MRV. Although precipitated CaCO_3 may contribute to long-term CDR, its formation in surface waters consumes alkalinity that would otherwise remain available for atmospheric CO_2 uptake, and therefore reduces the near-term CDR potential of OAE” (lines 531-535)

Line 478: Here you would also need to cite Fuhr et al. 2023 and 2024 where these authors actually showed that added calcium carbonate dissolves under various geochemical conditions.

We have added the suggested citations (lines 538).

Line 480: In which bottle experiments. In general: The citations in this script are very scarce. The authors should definitely step back and redo their literature research on the various aspects discussed here. And despite the one answer to a previous reviewer: There are studies out there that have investigated OAE and e.g. effects on advection, grain interaction or secondary minerals. Despite the fact that some of them used materials other than $\text{Mg}(\text{OH})_2$ and $\text{Ca}(\text{OH})_2$ I am lacking a comprehensive comparison with their results.

We understand and have reworked the manuscript to 1) describe the concept of diffusive boundary layer, concentration gradient driving diffusion and mass-transfer limitation (from Drever, 1997; Lasaga, 2014; Levich, 1962; Sulpis et al., 2019), 2) incorporate previous OAE works discussing the role of porewater exchange and water flow to enhance dissolution, though they mostly report olivine-based research with different setups and research questions which makes direct comparison trickier (from Flipkens et al., 2023; Fuhr et al., 2022; Fuhr et al., 2024; Fuhr et al., 2025; Montserrat et al., 2017), and 3) compare our observations to fundamental work published on secondary mineral formation, in particular for $\text{Mg}(\text{OH})_2$ formation in seawater (from Haas, 1916; Kapp, 1928, as well as more recent ones from Hashim et al., 2025; Shaw et al., 2025; Varliero et al., 2024)

Line 485a: Which consideration? How can one actually use a consideration?

We have reformulated as “Based on modelling considerations from Suitner et al. (2026)” (lines 543-544)

Line 485b: “one could” see above.

We removed it.

Line 487: “However such needles may...”: Based on what is this assumption made? Please provide a reference.

This section was reworked and now reads: “The morphology of secondary CaCO_3 precipitates may also influence such transport. Based on modelling considerations from Suitner et al. (2026), the porous needle-like structures observed in the $\text{Ca}(\text{OH})_2$ -amended bottles are unlikely to inhibit dissolution because seawater can seep between the needle structures and reach undissolved particles. In contrast, the slower dissolution of $\text{Mg}(\text{OH})_2$ seems to favour slow-growing, denser broccoli-shaped structures. The higher Ca^{2+} and Mg^{2+} concentrations measured in the precipitates from the 0 RPM experiments support the interpretation that CaCO_3 coated $\text{Mg}(\text{OH})_2$ particles (Table 2). Such coatings could simultaneously reduce dissolution efficiency while increasing particle density and thereby promoting export of undissolved $\text{Mg}(\text{OH})_2$ particles from the mixed layer. Conversely, rapid particle export could significantly decrease the surface area available for further precipitation, limiting the loss of TA to CaCO_3 (Baltruschat et al., 2026; Suitner et al., 2026).” (lines 543-551)

Line 489: „...allows for a higher CAO in the dbl”: This is still contradictive. How do you define CAP in a dbl? At the grain surface, the A would always be C for long P (as long as the grain exists). Please clarify this at the respective point in the manuscript.

The CAP definition has now been provided earlier with a clear concept described in lines 52-59.

Line 490: “This was identified by...”: In what way do these results support that? Please explain.

We have added some additional information, which now reads: “The higher Ca^{2+} and Mg^{2+} concentrations measured in the precipitates from the 0 RPM experiments support the interpretation that CaCO_3 coated $\text{Mg}(\text{OH})_2$ particles (Table 2).” (lines 546-548)

Line 494: “These statements are only hypotheses...”: Please rephrase this to something like “Understanding the precise behaviour of the different particles, though, will need dedicated experimental work.”

Thank you, we have reformulated in those lines. It now reads: “Dedicated experiments incorporating realistic hydrodynamics, atmospheric CO_2 equilibration and particle transport would improve estimates of realised CDR and provide valuable constraints for future MRV protocols.” (lines 558-559)

Line 496: “Current understanding of the global CDR potential...”: Anything to back up this statement? Otherwise, I wonder where the authors take this information from.

We have removed this statement during the rework.

Line 498: “DIC fluxes”: I haven't seen a single flux calculation so far, let alone a resulting value.

We agree with this statement. We have added a new section in the results that focuses on flux calculations: “CO₂ fluxes and derivation of equivalent wind speeds from DIC measurements” (line 330)

Line 499: “ours”: Our what? results? Please rephrase this sentence.

This has been removed during the rework of the discussion.

Line 499b: “first steps after”: if you need to, at least say time steps, please.

This is a good point, we edited as suggested throughout the text.

Lines 502 – 505: Please provide an informed guess on how much CaCO₃ has formed in each experiment and account for that in your flux calculations. I will therefore skip the rest of this section, until this has been performed.

We performed the requested flux corrections. We have estimated CaCO₃ precipitation between days 1 and 3 from the decrease in TA, subtracting the day 3 TA from the day 1 TA. Assuming all of this loss of TA was to CaCO₃, half of it would have resulted in a DIC loss. Hence, such loss was added to the corresponding measured DIC increase. Because the correction was applied to the individual flux estimates before calculating the mean fluxes used to derive $k_{0 \text{ RPM}}$ and $k_{125 \text{ RPM}}$, we report the resulting means and standard deviations for each advection regime. The detailed section with corrected calculations now reads: “During the first few days after alkalinity addition, DIC measurements yielded estimated net CO₂ fluxes inferred from DIC changes of -4.7 ± 21.2 and $38.2 \pm 25.3 \text{ } \mu\text{mol kg}^{-1} \text{ d}^{-1}$ for the 0 and 125 RPM experiments, respectively. However, these rates coincided with a decrease in TA during that time, most likely resulting from CaCO₃ formation, as indicated by the simultaneous decrease in TA and the negative apparent flux of CO₂ in the 0 RPM treatments. This simultaneous removal of DIC likely biased the inferred CO₂ fluxes. Estimated DIC loss to CaCO₃ can be inferred from TA loss, where for each mole of TA lost to CaCO₃, 0.5 mole of DIC is removed. This resulted in about 10.9 ± 6.3 and $4.8 \pm 4.0 \text{ } \mu\text{mol kg}^{-1} \text{ d}^{-1}$ in the 0 and 125 RPM treatments respectively, corresponding to about 1.1 and 0.5 mg CaCO₃ kg⁻¹ d⁻¹.” (lines 563-570)

Line 540: Likewise, I will skip the conclusion part for now. There are too many issues in the script that may or may not change the conclusion. Hence, I will gladly have closer look at it, once the other issues reported above have been fixed.

We have reviewed the rest of the conclusion and hope that the changes made will satisfy the reviewer's comments.