REVIEWER 1

General comments:

This study assesses the long-term alkalinity generation and CO_2 ingassing following the addition of $Ca(OH)_2$, $Mg(OH)_2$, and liquid NaOH to seawater under slow advective and stagnant conditions. The experimental design and findings are novel and provide a valuable contribution to the growing body of literature on ocean alkalinity enhancement (OAE), particularly regarding the role of hydrodynamic conditions in determining alkalinity realization and CO_2 uptake. The results are relevant for informing implementation strategies and parameterization in larger-scale models. However, the manuscript contains numerous minor grammatical errors and some unclear sentence constructions, which make parts of the text, especially outside the Discussion, difficult to read fluently. In addition, the Materials and Methods section lacks a clear and logical structure, making it challenging to easily understand the experimental setup and workflow. I believe that moderate revisions are therefore required before the manuscript can be considered for publication in Biogeosciences.

Thank you for the time and efforts put into the review, as well as the good words. We will address the comments as suggested unless otherwise stated.

Specific Comments

Abstract & Introduction

Line 4: “Long term modelling studies have focused on this strategy, but most laboratory experiments focus on shorter term with strong advection…”. This sentence is strangely formulated. Shorter-term what? Experiments? Timescales? Please clarify and reformulate.

We understand the confusion. We suggest reformulating as follows: “Long term implementation has been assessed in modelling studies, but most laboratory experiments focused on shorter timeframes with strong advection, which may not be representative of natural systems.”

Line 10: Replace “have been” with “were” to be grammatically correct, since the six-month experiment has presumably been terminated. The abbreviation TA is not introduced in the abstract. Please introduce total alkalinity before using TA.

Thank you, this will be done.

Line 14: What is the critical alkalinity period (CAP)? This is not a commonly used term in OAE research and should therefore be explained in more detail.

We understand. To address the comment, we suggest: “The concept of critical alkalinity period (CAP) is introduced here to represent the critical period of high alkalinity where unwanted secondary calcium carbonate ($CaCO_3$) formation may occur, significantly altering the capture potential of the approach.”

Line 18: "Using NaOH revealed buffering by $Mg(OH)_2$ precipitation, which ultimately redissolved in the bottles exposed to advection.". The meaning of this sentence is unclear. Please rephrase for clarity.

Thank you, we suggest reformulating as: "In addition, when using NaOH as alkaline feedstock, $Mg(OH)_2$ precipitation was observed and eventually redissolved in the bottles exposed to advection"

Line 19: "Finally, the bottles exposed to advection represented open ocean conditions with wind speeds of ~3 m s^{-1}…". This is a somewhat unusual concluding sentence. I suggest mentioning earlier in the abstract (around line 11) that slow advection represents open ocean conditions (~3 m s^{-1} wind speeds). You can then conclude that advection is important to consider and that these data may serve as lower thresholds for inclusion in global models.

Thank you for pointing this out. We will edit the abstract as requested by adding the following text earlier: "Bottles were either kept stagnant or exposed to slow advection, similar to open ocean conditions with wind speed of ~3 m.s^{-1}". We also suggest concluding the abstract as follows: "The study emphasise that water advection is important to consider and the data presented here may serve as a lower threshold for implementation in global models.".

Line 21: Either "carbon dioxide" is missing before (CO_2), or the brackets should be removed.

This is indeed a mistake from our side, we will edit it as suggested.

Line 22: This sentence does not read fluently and is missing a verb before "the" and after "atmosphere." I suggest: "While strong efforts to reduce CO_2 emissions are required, current projections report that these efforts are insufficient to slow global warming, with active CO_2 removal from the atmosphere being the only solution."

Thank you pointing this out, we suggest: "While strong efforts to reduce CO2 emissions are required, current projections report that these are insufficient to slow down global warming, with active CO_2 removal from the atmosphere being the only solution"

Line 26: Insert "seawater" before "total alkalinity" so it is clear to non-experts where alkalinity is added.

This is noted and will be added.

Line 36: Does precipitation always lead to a runaway pattern? For example, if dilution occurs (leading to a decrease in Ω_a) after precipitation has started, would precipitation continue or stop? Please rephrase this statement more cautiously if necessary.

We agree with the suggestion, we therefore suggest a more cautious formulation such as: "Once precipitation starts, it may enter a runaway pattern where the precipitation speed increases, sometimes reaching TA values lower than the starting conditions"

Line 43: Since CAP is not a commonly used term in OAE research, I miss a clear definition of its meaning here.

We understand and suggest reformulating as: “Overall, the persistence of high alkalinity and Ω_A (micro-)environments will dictate the likeliness of alkalinity loss to $CaCO_3$ and potential impacts on biology. This period of time 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)”

Line 48: Is the word “rates” missing after “dissolution”? The sentence reads awkwardly.

We noted the comment and suggest: “While these settings allow for easy and standardised comparison between the various dissolution rates of alkaline minerals, and possible precipitation of $CaCO_3$, they usually omit the role of CO_2 ingassing on the stability of alkalinity.”

Line 49: “More important” is not the right word here. I believe “stronger” would be more appropriate.

Thank you, we will edit accordingly.

Lines 50–54: The research aim is not explicitly mentioned. Please clearly state the objective of the study. The rationale for selecting a specific rotation speed would likely fit better in the Materials and Methods section.

We understand and suggest reformulating as: “In a new approach, the dissolution of $Ca(OH)_2$ and $Mg(OH)_2$ in natural seawater were addressed both in static and rotating reaction vessels for 6 months. Circular advection regimes were chosen to try and represent open ocean conditions, while gas exchanges were allowed to occur during the experiments. Carbonate chemistry was monitored throughout, particle analyses were conducted at the end of the experiments, and gas velocities for CO_2 and equivalent open ocean wind speeds were derived, allowing direct comparison between laboratory data and global modelling studies.”

Materials and Methods

The Materials and Methods section starts in a very unconventional way, with the duration of the experiment mentioned in the first sentence and the actual setup described only later. Please rewrite this section in a more logical, chronological order reflecting how the experiments were conducted. This will make it easier for readers to understand the experimental setup, sampling, and analyses. Furthermore, adding a figure of the experimental setup would improve clarity.

We understand the reviewer comment. We will edit accordingly by first introducing the setup (type bottles, shaker, temperature and RPM selection). We then will proceed with the preparation of the experiments (bottles cleaning, seawater preparation and equilibration). The section will finish by highlighting forecasted issues that may have arisen during the experiments, such as evaporation, and how to control it.

Line 58: Define “room temperature.” I see it is mentioned again in line 132. I suggest combining this information so that all relevant details are presented together.

Thank you for pointing this out, this will be moved up and mentioned in the first lines of the section.

Line 59: Why was circular motion used? Does this represent seawater movement under natural hydrodynamic conditions? Please clarify.

Thank you for pointing this out. We have introduced the concept of Reynolds number which gives information on turbulence, and suggest: “Similarly, the tests revealed that a circular motion was more appropriate to resuspend the dye without breaking water tension, while the dye resuspension using linear motion was insufficient and the water tension was significantly disturbed compared to the static bottles. Such conditions yielded a Reynolds number ~$1.6 \cdot 10^4$, above the threshold considered for turbulent systems (Thorpe, 2007). Such setup was used for a total of 6 months to assess the long-term response of alkalinity addition to seawater.”

Line 68: A small schematic of the experimental setup would be useful for clarity.

We understand. The schematic below will be added to the appendix.

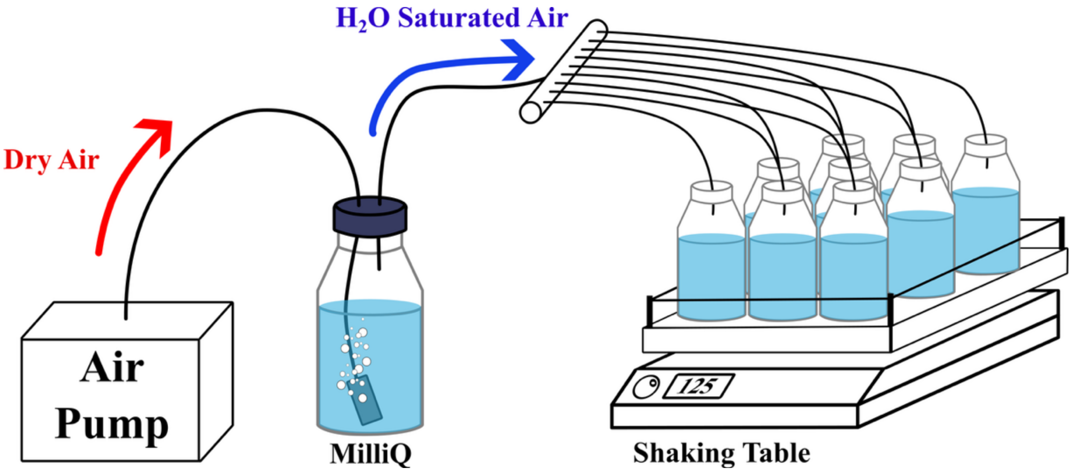

Figure A 1: Schematic of the experimental setup. An air pump is taking up dry air and pushes it into a closed Schott bottle where MilliQ is bubbled. The new H_2O saturated air is then carried to an air diffuser from which individual tubing are connected to each experimental bottle, constantly and equally supplying each open bottle with fresh atmospheric CO_2.

Line 72: There should be a space between “90” and “L.” Please ensure consistent spacing throughout the manuscript.

Thank you for pointing this out. This has been noted and will be edited throughout the text.

Lines 85–86: This sentence is unclear. Was MilliQ only added if the difference in weight was between 5 and 10% (and not if higher or lower)? Please clarify.

We understand the confusion, we suggest reformulating as: “If a difference in weight was observed to be higher than 5% after these 2-3 days, a precise amount of fresh MilliQ was added by weighing to compensate for the slight evaporation.”

Line 88: What were the light conditions? Was the experiment conducted in the dark or under normal laboratory light? Please specify.

This is a good point, we suggest adding: “Experiments were conducted in the dark except during sampling where laboratory lights were tuned on”.

Line 90: Change “organisms’” to “microorganism.”

This will be done.

Line 100: “Powders were sieved to allow for direct comparison.” Even after sieving, the powders can still differ significantly in grain size distribution and reactive surface area. Please elaborate on this limitation.

Thank you for the comment. To tackle this limitation, we have sent the samples for grain size distribution analysis which we will add to the manuscript as soon as the results are available and if those are of relevance to the interpretation.

Line 108: Write dissolved inorganic carbon (DIC) in full before using the abbreviation.

This will be edited as suggested.

Line 109: Does “3.3” refer to the type of TOC vials? The manufacturer name is typically mentioned in brackets. Please apply this consistently throughout the manuscript.

The 3.3 refers to the type of borosilicate, which is named “borosilicate 3.3”.

Line 138: This is already a result and should not be included in the Materials and Methods section.

We understand and suggest reformulating as: “Secondary mineral formation was expected and suspected to occur on the surface of added material, i.e., $Ca(OH)_2$ and $Mg(OH)_2$.”

Line 144: How does Ca and Mg analysis of the water help determine whether undissolved $Mg(OH)_2$ (solid phase) could have acted as a substrate for $CaCO_3$ precipitation? Please clarify.

We understand and suggest adding: “In the case where $CaCO_3$ precipitated on yet to be dissolved $Mg(OH)_2$ particles, a clear signal of Mg^{2+} would be observed. Otherwise, if $CaCO_3$ was to precipitate from the water column only, the Mg^{2+} signal will be absent.”

Line 145: Why was the remaining water volume weighed and dried in a 50-mL Falcon tube? I assume this was to estimate the precipitated $CaCO_3$ amount, but please explicitly state this. Were filters rinsed with MilliQ before drying? Please elaborate.

This is a good point, we suggest adding more details such as: “The weighing of the remaining liquid allowed to determine the exact amount of seawater filtered for the analysis, which is required to report on the concentration after analysis.”.

Line 151: Quality control for what? To compare measured and calculated carbonate system parameters? Please clarify.

We understand the question and suggest adding: “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.”

Results

Figure 1: Instead of showing only average values, also display variation (standard deviations). Was one day expected to be sufficient to dissolve $Ca(OH)_2$ and $Mg(OH)_2$ under stagnant or rotation conditions? Perhaps the percentage is higher after several days?

The figure will be adjusted accordingly. Here is our suggestion:

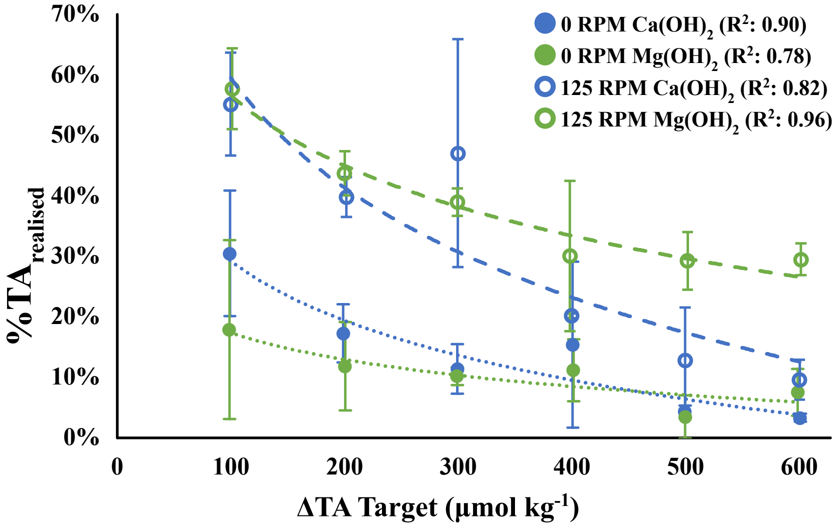

Figure 1: %TArealised (ΔTA measured ± standard deviation) after 1 day, i.e., maximum ΔTA measured) as a function of the ΔTA target (in µmol kg^{-1}). Each point represents the average of each triplicate, with blue markers representing the $Ca(OH)_2$ treatments

and the green markers representing the $Mg(OH)_2$ treatments. Dotted lines represent logarithmic fits of the 0 RPM treatments while the dashed lines represent the logarithmic fits of the 125 RPM treatments. Corresponding R^2 are provided in the legend.

Line 160: Space missing between "125" and "RPM." Please check spacing throughout.

This is noted and will be checked throughout the manuscript.

Line 168: After "%TArealised," "which was" is missing.

This will be added.

Line 173: The statement comparing $Mg(OH)_2$ and $Ca(OH)_2$ TA generation without advection needs reformulation. $Mg(OH)_2$ values (3–18%) appear mostly lower than $Ca(OH)_2$ (3–30%) relative to expected TA increase. $Mg(OH)_2$ seems only significantly higher than $CaCO_3$ in at most half of the treatments (100, 200, 400 µmol kg^{-1}). Please revise accordingly.

We do not fully agree here. In fact, figure 1 clearly shows that the plain green data points ($Mg(OH)_2$, no advection) are lower than the plain blue data points ($Ca(OH)_2$, no advection) in nearly all experiments, i.e., ΔTA targets from +100 to +500. The only target with a higher %TArealised with $Mg(OH)_2$ and no advection occurred with a target of +600.

Line 187: "Overtime" should be "over time."

This is noted and will be edited as suggested.

Line 188: Add standard deviations to the 100 and 180 µmol kg^{-1} averages and to other averages throughout the text.

This will be edited as suggested.

Line 197: Provide actual numbers instead of "slightly negative ΔTA."

We understand and will edit accordingly.

Line 199: Add "values" after "positive."

We understand and will add it.

Line 202: The "l" is missing in "slight."

Thank you for pointing this out.

Line 203: Provide exact values instead of “about 100 µmol kg^{-1}.” Also, error bars are large after one month in the 600 µmol kg^{-1} treatment. Is there actually a statistically significant decrease compared to day 6? Statistical analysis would strengthen this section.

This is noted and will be edited accordingly. Regarding the error bars after one months in the 600 µmol kg^{-1} treatment, we are not sure what the reviewer means but assumed it was for the bottles exposed to no advection. There is indeed a decrease compared to day 6, both with $Ca(OH)_2$ and $Mg(OH)_2$. While we agree with the statistics comment below, we are not convinced a statistical analysis here will change the interpretation. In our case, we are mostly interested in the pattern and the clear difference following varying alkalinity addition rather than a statistically significant change. Nevertheless, we have conducted a statistical test and will include it in the text (see below).

Line 215: “In most cases…” — this sentence needs improvement; it does not read fluently and contains errors.

We understand the point and suggest: “In most cases, the decrease in pCO_2 was correlated to the targeted ΔTA, i.e., higher targeted ΔTA led to lower pCO_2. This was the case in most treatments except for the 0RPM $Ca(OH)_2$ experiment, where the lower pCO_2 was observed in the +400 µmol kg^{-1} experiment.”

Line 220: The trend in pCO_2 values for 0 RPM $Mg(OH)_2$ treatments appears very similar to 0 RPM $Ca(OH)_2$ treatments. I do not agree with the current interpretation. Statistical analysis would help reduce subjectivity.

We agree with the comment and have run a simple t test, to identify from which time point the treatments pCO_2 were not statistically significantly lower than the control pCO_2. We created a p-value table (see below) that will be added to the appendix of the manuscript.

*Table 1: Compiled p-values from a two samples t-test to determine whether a given treatment pCO_2 is statistically significantly lower than the control pCO_2 for a specific day. The treatments statistically significantly lower with a confidence level of 95% (p-value ≤ 0.05) are bolded, and those with a confidence level of 90% (p-value ≤ 0.1) are indicated by a *.*

0RPM	**$Ca(OH)_2$**						**$Mg(OH)_2$**						**NaOH**
ΔTA	*100*	*200*	*300*	*400*	*500*	*600*	*100*	*200*	*300*	*400*	*500*	*600*	*600*
Day 1	**0.02**	**0.01**	**0.00**	**0.00**	**0.00**	**0.00**	**0.01**	**0.01**	**0.01**	**0.01**	**0.01**	**0.00**	**0.00**
Day 3	**0.02**	**0.01**	**0.01**	**0.00**	**0.00**	**0.00**	0.07*	**0.02**	**0.01**	**0.00**	**0.00**	**0.00**	**0.00**
Day 6	0.27	0.06*	0.33	0.60	**0.00**	**0.05**	0.33	**0.10**	**0.01**	**0.01**	**0.04**	**0.00**	**0.00**
Day 35	0.07*	**0.04**	**0.04**	**0.02**	**0.02**	**0.04**	**0.02**	**0.01**	**0.01**	**0.01**	**0.01**	**0.01**	**0.00**
Day 69	0.06*	0.32	0.17	0.24	0.30	0.56	0.29	0.84	0.39	0.80	0.45	0.79	**0.03**
Day 108	0.57	0.52	0.88	1.00	1.00	0.83	0.53	0.78	0.71	0.88	0.87	0.17	0.52
Day 174	0.32	0.95	0.96	0.99	1.00	0.99	0.97	0.99	0.95	1.00	1.00	0.99	0.16

125RPM	**$Ca(OH)_2$**						**$Mg(OH)_2$**						**NaOH**
ΔTA	*100*	*200*	*300*	*400*	*500*	*600*	*100*	*200*	*300*	*400*	*500*	*600*	*600*
Day 1	**0.01**	**0.00**	**0.00**	**0.00**	**0.00**	**0.00**	**0.00**	**0.01**	**0.00**	**0.00**	**0.00**	**0.00**	**0.00**
Day 3	0.68	0.60	0.97	0.76	0.21	0.17	0.79	0.29	**0.00**	**0.00**	**0.00**	**0.01**	**0.00**

Day 6	0.75	0.73	0.75	0.55	0.64	0.81	0.75	0.78	0.74	0.66	0.77	0.68	0.60
Day 35	0.31	0.34	0.55	0.61	0.19	0.32	0.15	0.40	0.27	0.21	0.20	0.16	0.07*
Day 69	0.55	0.53	0.80	0.71	0.51	0.36	0.60	0.72	0.63	0.63	0.67	0.44	0.19
Day 108	0.13	0.06	**0.05**	**0.04**	0.13	0.26	0.16	0.16	**0.01**	**0.03**	0.34	0.10	0.87
Day 174	0.28	0.47	0.87	0.64	0.66	0.12	0.38	0.45	0.49	0.22	0.59	0.93	0.34

We therefore suggest editing as follow: “With both $Ca(OH)_2$ and $Mg(OH)_2$, pCO_2 slowly increased over 1 month and remained statistically significantly lower than the control pCO_2 values. After 2 months, the difference was not statistically significant anymore (Table A1). Over the following 4 months, pCO_2 slightly varied, but followed the control batch fluctuations.”

Line 223: There is no day 2 data point; 24 h should likely be 48 h (day 3).

This is correct. We suggest editing as: “In the case of $Ca(OH)_2$, the pCO_2 values were back to concentrations similar to the control incubations after only 48h.”

Line 224: Please rephrase. For low alkalinity treatments, $Mg(OH)_2$ pCO_2 values were already similar to the control at day 3.

Thank you for pointing this out, we suggest being clearer as: “The $Mg(OH)_2$ incubations' pCO_2 also quickly bounced back but reached control values only after 3 days for additions of 100 and 200 µmol kg^{-1}, and after 6 days for additions of 300-600 µmol kg^{-1}”

Lines 236 and 337: The first two sentences of this paragraph appear contradictory. Please rephrase.

We agree and suggest: “The NaOH experiments revealed differing patterns to those from the $Ca(OH)_2$ and $Mg(OH)_2$ incubations. While both setups are meant to be identical, a clear difference emerges regarding the ΔTA.”

Section: Effects of advection on pCO_2 changes: Statistical analysis would be useful to identify when pCO_2 values are not significantly different from the control.

We agree and have provided some statistical tests results above. We will incorporate them throughout the section as suggested.

Figure 2 caption: Replace “range” with “error bars.”

This is noted and will be changed to: “Each point represents the average of triplicates, with error bars representing the uncertainty based on the standard deviation of these three measurements.”

Lines 256–258: Move this calculation to Materials and Methods and explain how it was calculated.

We agree and will move it up in the text.

Line 259: Add “more” after “about 194 µmol kg^{-1}.”

This is a good point and will be edited accordingly.

Table 1: Consider whether this could be presented as a figure for improved clarity.

We have discussed further regarding such issue, and the table appears to be more suitable than a figure here.

Table 1 caption: Specify that it refers to the 600 µmol kg^{-1} treatment.

This is a valuable point, we will edit accordingly.

Line 261: Add the formula for estimated Ca^{2+} in Materials and Methods.

We agree and will move it up in the text.

Lines 264–266: Improve fluency.

We agree and will consider keeping it much simpler, such as: “Finally, another major difference emerges from the measurements of Mg^{2+}, where the 0RPM has nearly twice as much Mg^{2+} as the 125RPM. Overall, the particles collected from the 0RPM experiments were composed of 3.8% ±0.4% of magnesium, while the particles collected from the 125RPM experiments, were about 3.0% ±0.3% magnesium.”

Figure 5 caption: Mention exact treatment (e.g., 600 µmol kg^{-1}), not “higher addition.”

We agree and will incorporate it.

Discussion

Line 293: Specify $Ca(OH)_2$ and $Mg(OH)_2$ instead of “such materials.”

We agree and suggest: “A very general observation regarding alkalinity generation is that the dissolution of $Ca(OH)_2$ and $Mg(OH)_2$ here was lower than reported in the literature”

Line 295: Explain why this incubation is more realistic and which coastal conditions are being mimicked. The impact of advection on CO_2 ingassing rates is not sufficiently discussed despite being in the section title.

This is a good point and we suggest: "Such an outcome was to be expected as the milder, yet likely more realistic approach of the shaking table, relying on water diffusion and mild vertical mixing compared to the vortex created by stir bars, decreases the dissolution effect and disturbance created with a magnetic stir bar"

Line 298: Flipkens et al. (2023, GCA, 359, 84–99) reached similar conclusions in an olivine incubation under different advective regimes and could therefore be cited.

Thank you for providing the reference, this will be incorporated.

Line 313: Provide percentage of total expected TA increase rather than absolute concentration.

We agree and will edit accordingly.

Line 338: Change "loss of alkalinity to $CaCO_3$" to "loss of alkalinity through $CaCO_3$ precipitation."

This is a good point and will be edited.

Line 349: Improve sentence fluency.

We agree and suggest: "Hence, allowing for fast CO_2 ingassing limits and decreases the risk for runaway $CaCO_3$ precipitation:

Line 353: Change "figure 2, Figure3" to "Figure 2 and 3."

This is a good point and it will be addressed.

Line 358: Specify which figure is being referenced.

Thank you for pointing this out, we meant Figure 5 and will add it there.

Line 405: Should this be "upper limit of effects" rather than "lower limit"?

It is indeed upper limit, thank you.

Line 406: The statement that real-world OAE applications may only have small effects, if any, on living organisms is too bold. Results from a 1-L bottle cannot necessarily be extrapolated to a 50-m water column. CAP duration and impacts could differ. Also specify which OAE applications are meant. This conclusion cannot yet be drawn for all OAE approaches, especially those involving trace-metal- releasing materials such as olivine or steel slags. Please nuance this statement.

We understand and suggest a short edit such as: “In open ocean application, the quick dilution and subsequent CO_2 ingassing will most likely decrease the CAP duration significantly, decreasing OAE effects. However, such hypothesis is yet to be tested as open ocean OAE trials are very scarce.”

Line 413: Add “drastically” after “decreased.” CCS/CCU of calcination associated CO2 emissions will be essential for ocean liming (see Foteinis et al., 2022).

This is noted and will be edited.

Line 429: Export to deeper ocean is not the only possibility; settling onto coastal sediments is also possible.

We agree and suggest: “Hence, the crystals will ultimately sink and be transported onto coastal sediments or into the deeper ocean”

Line 431: “Buried in sediments without further dissolution” is incorrect. The cited studies show $CaCO_3$ dissolution in surface sediments under undersaturated conditions. Dissolution would only stop if buried below the reactive layer. Please correct.

Thank you for the details, we suggest: Once transported in the deeper waters, $CaCO_3$ particles may redissolve in areas of undersaturated seawater (like below the carbonate compensation depth) or be buried in sediments where further dissolution may occur until transported below the reactive layer”

Line 443: It is unclear why slower $Mg(OH)_2$ dissolution would allow a higher CAP in the diffusive boundary layer. Wouldn’t slower dissolution instead prolong but reduce oversaturation intensity?

This is a fair point. We agree that with the faster dissolving $Ca(OH)_2$ particles, the oversaturation intensity is likely to be higher. However, it is most likely “washed away” faster when diffusion and mixing occurs. In the case of $Mg(OH)_2$, the slower dissolution would likely prolong the CAP as the reviewer points out. The level of oversaturation as well as the time at which such level is maintained are both key factors to trigger $CaCO_3$ precipitation.

It is safe to assume that in the diffusive boundary layer, the Ω_A is reaching very high levels, much higher than levels required to trigger homogeneous precipitation. We can therefore assume that the oversaturation intensity is, in both cases, critically high, leading to instant nucleation. As discussed above, the second important factor for $CaCO_3$ precipitation kinetics is the length of time during which such levels are maintained. Therefore, when $Mg(OH)_2$ dissolves, instant $CaCO_3$ formation is triggered but also critical conditions maintained for longer than with $Ca(OH)_2$, which ultimately results in further $CaCO_3$ precipitation.

Line 461: Add “seawater” before “the bottle.”

This is a good point and will be edited.

Line 470: Add typical open-ocean gas transfer velocities in brackets for comparison.

We agree and suggest adding: : These gas velocities are significantly lower than those observed naturally in the open ocean (averaging between 14 and 20 cm h^{-1}, but ranging from nearly 0 to roughly 50 cm h^{-1}) but may be correlated to wind speed (Zhou et al., 2023; Yu et al., 2013; Frew et al., 2007)."

Line 483: This is not purely hypothetical. Your data and other studies (England & Bach, 2025; Flipkens et al., 2023) support this claim. Please cite accordingly.

We understand and agree, we suggest: "While the effect of wave height on the dissolution of alkaline materials is fully constrained, results presented here and published research (England & Bach, 2025; Flipkens et al., 2023) support that the stronger the water movement and disturbance, the quicker the mineral dissolution."

Line 492: Other long-term OAE experiments exist (e.g., Fuhr et al., 2025), but not with fast-dissolving minerals like $Ca(OH)_2$ or $Mg(OH)_2$. Please rephrase.

We agree and suggest: "The experiments presented here report on the first long-term (6 months) laboratory incubations for OAE with fast dissolving feedstocks."

Lines 497–499: Rephrase this sentence; it is currently too complex.

We agree and suggest separating in two sentences as: "$CaCO_3$ formation was directly correlated to the amount of $Mg(OH)_2$ added at 125RPM. However, above targets of 300 µmol kg^{-1} with $Ca(OH)_2$, the TA generation decreased while $CaCO_3$ precipitation rate increased at 125RPM."