General comments:

This manuscript investigates olivine dissolution and associated trace metal release in natural filtered seawater during a 21-day flow-through reactor experiment. Both stagnant and turbulent conditions were examined, and the influence of natural beach sand on olivine dissolution was evaluated. The main novelty of the study lies in the relatively high flushing rates applied in the reactors. The results suggest that sand-induced abrasion can transiently enhance olivine dissolution during the first days of the experiment, after which dissolution rates converge between the pure olivine and olivine/sand treatment. Overall, the study provides a useful, although relatively incremental, contribution to our understanding of olivine dissolution under conditions relevant to coastal environments.

Several conclusions throughout the manuscript would benefit from being presented more cautiously. In particular, the abrasion effect appears to be short-lived. Furthermore, the severeness of the "additionality problem" will likely also depend strongly on environmental conditions such as sediment type, porewater flushing rates, and hydrodynamic regime. This should be more clearly acknowledged, and conclusions should be better aligned with the presented data and the natural setting it tries to mimic.

The manuscript would also benefit from improvements in writing style. Several sentences are grammatically awkward, reducing readability. In addition, many results are presented only as averages. Including measures of variability (e.g. standard deviations) throughout the text would considerably improve the interpretation of the results. Overall, I believe major revisions are required before this manuscript can be considered for publication in Biogeosciences.

Dear reviewer,

Thank you for your very careful and detailed review of our manuscript. We agree with most of your comments which are responded to in detail below. Especially the description of the additionality problem was revised and the interpretation formulated more carefully. We agree that the problem might not be as important for the surf zone as initially stated. We highlight now that under stagnant conditions, the problem might be quite relevant, as we observe an alkalinity sink, likely caused by secondary precipitates and reduced alkaline-mineral dissolution in the sand. To account for this, we will focus the discussion in section 4.3 on mineral dissolution and potential precipitation reactions in the sand in case we are invited to submit a revised manuscript (please see revised section below). We hope, you agree with the improvement of the sections included below. Individual responses to your comments are shown in blue.

'4.3 *Mineral reactions in beach sand*

The sand from the west coast of Brittany (Argenton) that was used in our experiments consisted of 2 materials that can release alkalinity upon dissolution, i.e. carbonates and feldspars. The sand contained up to 14 wt.% carbonate (calcite, high Mg-calcite and aragonite; supplementary Table S4), and when in contact with corrosive seawater or pore water, carbonates can dissolve causing a natural increase in TA (Middelburg et al., 2020; Torres et al., 2020). In our study, an indication for carbonate dissolution is only seen in changes in the CaO content of the sand, which decreased by ~1 wt.% in the turbulent sand treatments (supplementary Fig. S6b; Table S4) and eventually by an increase in Sr, though this increase cannot unequivocally be attributed to carbonate dissolution (see following

paragraph). However, in all our experimental treatments and in the seawater reference, carbonate minerals were oversaturated (Ω_{Ar} > 1; Fig. 4c), and potential dissolution limited or concentrated on localized grain surfaces (see section 4.1). The addition and subsequent dissolution of olivine even increased the carbonate saturation state to higher levels, further inhibiting the dissolution of sand-occurring carbonates.

Feldspars are highly abundant in the used sand, making up to 30.4 wt.% (plagioclase and microcline; supplementary Table S4) and, as plagioclase contains Ca, could also be responsible for the Ca loss in the solids upon dissolution (supplementary Fig. S6b,c; Table S4). Although being highly reactive in seawater, dissolution rates of feldspars (plagioclase) are about 20% lower compared to olivine (forsterite) (Jeandel and Oelkers, 2015). Nevertheless, feldspars can dissolve quickly when initially in contact with seawater, having the potential to impact marine element cycling (Geilert et al., 2023). While high seawater background concentrations of Na, K and Ca inhibit a direct correlation with mineral dissolution, a strong increase in Sr and Li concentrations is visible only in the sand-bearing treatments compared to sand-free experimental treatments (Fig. 5c, d). While Sr continuously increased over the experiment, Li showed a decrease after ca. T+100 h. This may indicate the precipitation of secondary minerals, however, this is not directly visible in the TA concentrations of the sand-bearing treatments (see section 4.2 and 4.4). Also, Sr is lost from the solids of S and the OS treatments, which was most pronounced in the turbulent OS treatment (supplementary Figure S6e,f). Unfortunately, we cannot unequivocally identify the mineral source and later sink of these elements, as Sr and Li are both enriched in carbonates and feldspars (Bentley, 2006; Penniston-Dorland et al., 2017). In addition, the stoichiometric balances discern that the dissolution of carbonates and feldspars produce the same amount of TA (2 HCO_3^-); therefore, we cannot further constrain the TA source in the sand treatments based on our data set. Nevertheless, we can conclude, that TA generation in the turbulent sand treatments are lower (T≤100hr), or equal (T≥100hr) throughout the experiment compared to the turbulent OS treatments. Thus, the natural TA release by sand was not hampered by the addition of olivine in the turbulent treatments. In contrast, in the stagnant sand-corrected OS treatments a shift to negative ΔTA values is observed (Fig. 2e, f), indicating the precipitation of secondary phases, likely carbonates (Fig. 8, section 4.2). The stagnant OS treatment scenario resembles in its low-energy setup the deeper beach and shallow shelf environment. Our results could therefore indicate a reduction in natural alkalinity release and maybe even a reversal to alkalinity consumption. This phenomenon was also investigated by Bach (2024) and defined as additionality problem, i.e. the modification of natural alkalinity release in marine sediments upon the deployment of anthropogenic alkalinity sources, for example by olivine addition in OAE scenarios. In conclusion, our results point to an effective alkalinity release in high energy environments when grain collision and high dilution with seawater enhance mineral dissolution. In contrast, in low-energy environments, simulated in our stagnant treatments, which requires further investigations with respect to OAE applications in deeper, low-energy marine environments.'

Specific comments and technical corrections:

Line 20: Please mention the duration of the experiment when introducing the flow-through reactor experiment (i.e., "after 21-day flow-through reactor experiment").

The experimental time was added as suggested.

Line 25: It is important to clarify that the high alkalinity release (11.5 mmol mol^{-1} olivine d^{-1}) was only observed transiently during the initial phase of the experiment, after which fluxes converged between treatments. Consider adding "transient" before "highest alkalinity release".

We added the word 'transient' as suggested.

Line 27: Please add "rapidly" before "diminishes". A decrease over approximately two days represents a very short timescale compared with the decades to centuries expected for complete dissolution of olivine grains of this size.

The word 'rapidly' was added as suggested.

Line 36: This sentence is unclear and potentially misleading. Olivine dissolution increases porewater alkalinity, which may reduce the extent of natural $CaCO_3$ dissolution, one of the major alkalinity-generating processes in coastal sediments. However, the extent to which $CaCO_3$ dissolution is affected will likely depend on porewater flushing rates and dilution effects, as suggested by this study. Therefore, describing the interaction as "cooperative" appears too strong. The effect may be limited, and olivine addition does not necessarily enhance the already occurring natural alkalinity production. Please rephrase this statement accordingly.

We completely agree and also based on your major comment we rephrased the last part of the abstract as: '*…In addition, our results show that the natural alkalinity release from sand was not diminished by olivine addition in turbulent treatments mimicking the high-energy surf zone, but that the risk of secondary mineral precipitation and by this CO_2 release increases under stagnant conditions, representative of low-energy coastal environments….*'.

Line 38: Please briefly elaborate on how these ephemeral peaks of critical element concentrations could be avoided or mitigated in practical applications.

A short explanation was added: '*Our study shows that ephemeral peaks of high critical element (e.g. Ni) concentrations due to high mineral reaction rates in the beginning of the experiment can be counteracted by avoiding thick olivine accumulations through the addition of sand*.'

Lines 43–44: Please replace "include" with "with" and "by" with "include" to improve the accuracy of this sentence.

The sentence was revised.

Lines 49–51: For olivine dissolution, the direct effect is an increase in alkalinity rather than a change in DIC. Changes in DIC result from subsequent atmospheric CO_2 uptake. Please revise these sentences to include this distinction.

The sentence was revised to highlight the difference between TA increase via mineral dissolution and changes in DIC as: *'The increase in total alkalinity (TA) via mineral dissolution increases the buffer capacity of the ocean and enhances the reservoir of dissolved inorganic carbon (DIC) by atmospheric CO_2 uptake in the form of bicarbonate and carbonate ions on timescales greater than 10000 years (Middelburg et al., 2020).'*

Line 53: Please provide a reference supporting the statements regarding forsterite dissolution.

The reference '*Jeandel C. and Oelkers E. H. (2015) The influence of terrigenous particulate material dissolution on ocean chemistry and global element cycles. Chem. Geol.* ***395****, 50–66*' was added.

Line 58: Moras et al. (2022) did not investigate olivine in their study. Therefore, this citation does not appear appropriate in this context and should be removed or replaced.

Moras et al. was added as a reference because of their work with respect to secondary carbonate precipitation. But we agree, that this reference is misleading here, therefore we agree with removing it.

Line 78: Please replace "sand" with "sediment". The additionality problem is not restricted to sandy environments and may occur in other sediment types as well.

We followed your suggestion and replaced 'sand' with 'sediment' to highlight the general marine seafloor alkalinity release.

Line 87: The sentence "if trace metal concentrations higher than background values were reached" reads awkwardly. Please rephrase for improved readability.

The sentence was revised: '*This setup allowed us to assess the alkalinity build-up due to grain collisions and examine whether trace metal concentrations reach potentially toxic levels for the marine ecosystem*.'

Line 89: Please specify "olivine OAE" rather than simply "OAE", as ocean alkalinity enhancement includes a broad range of techniques and mineral feedstocks.

The sentence was revised.

Line 95: Please replace "have been" with "were" for grammatical accuracy.

The sentence was revised (see next comment).

Line 97: Please provide the temperature and salinity of the seawater here (e.g., in parentheses). Although it is mentioned that these parameters were measured during subsequent analyses, the corresponding results are not presented in the main text.

The information was added as: '*The experiments were performed at the Ifremer (Institut Français de Recherche pour l'Exploitation de la Mer) experimental laboratory in Argenton, Brittany, France (48° 31.2768' N, -4° 46.1016' W). Natural coastal seawater was used from an on-site outdoor pool, which was replenished with spring tide. Before entering the lab, the seawater was in-line UV-treated (GERMI CP 75 PEHD), passed through a 5 µm and 1 µm filter and then subsequently in-line UV treated again (JBL AquaCristal UV-C serie 2 18W) (Di Poi et al., 2022). Over the course of the experiment, average temperature and salinity were 16.3±0.7°C and 35.2±0.2 PSU, respectively. For the experimental setup, ArtemioR Set incubation cones (JBL) were used as flow-through reactors, which were filled with 600 mL of the UV-treated and filtered seawater.*''.

Line 97: Please provide more details on the UV treatment procedure. For example, what type of UV treatment was applied and for how long?

We added more information on the UV treatment in the revised manuscript (see revised text to your comment to line 97).

Line 100: The filtration procedure is unclear, as the manuscript states both 1 µm and 5 µm filtration. Please clarify the exact filtration protocol and clearly describe how the UV treatment was integrated into the procedure.

We apologize for the lack of clarity. The filtration of incoming seawater was consistent and never changed. The system had two sequential filters. First passing through a 5um filter then a 1um filter before being delivered to the header tank which feed all treatment cones. The UV treatment was applied after filtration where seawater first entered the room. Please see the revised text in your comment to line 97.

Lines 103–104: Please clarify whether the grain size distribution and mineral composition data were obtained from Sibelco or from your own analyses.

The information was added as: *'Solid material consisted of dunite, with 88% of the grains between 0.355 and 0.710 mm in size (the remaining 12% were between 0.250 and <0.063mm) provided from the company Sibelco™, mined in Åheim, Norway (grain size data obtained from the supplier). This dunite was mainly composed of Mg-olivine (endmember forsterite; mineralogical composition described in section 3.2.1); from here on referred to as olivine.'*

Line 105: The location should be "Aheim" rather than "Arheim".

The location name was revised.

Line 105: A forsterite content of 75% appears relatively low for Aheim dunite. Please briefly discuss this observation.

Thanks a lot for identifying this discrepancy. The 75% was referring to a published value in Fuhr et al (2022), while our own measurements gave a higher value, as you also mentioned further below. We revised the text accordingly (see our answer to your comment above).

Line 107: Please clarify whether the material was "sieved to 0.5 mm" or "sieved to pass through a 0.5 mm sieve", as these represent substantially different grain size fractions.

As stated in the text, the sand was sieved to 0.5mm in order to fit the average olivine grain size. Therefore, we don't think further clarification is required here.

Line 108: Please specify the duration of autoclaving of the beach sand. Additionally, is autoclaving an effective method for sterilizing sediment material in this context? If this method has been previously validated, please provide an appropriate reference. Please also briefly explain the rationale for autoclaving.

The experimental lab we worked in also rears larval bivalves and autoclaving for sterilization is standard practice when working with natural materials. Thus, we applied the same sterilizing method to ensure that no foreign bacterial communities were present on the sand which could interfere with dissolution processes in the secretion of biofilms and metabolic activities that could change the water chemistry. The sand was autoclaved at 121◦C for 1 ½ hours. This information was added to the text.

Line 109: Direct comparisons between treatments are challenging because the amount of olivine differs substantially between the pure olivine treatment (11 g olivine/600 mL) and the olivine–sand treatment (2.2 g olivine/600 mL). Please explain the rationale behind using equal total solid mass rather than equal amounts of olivine and discuss how this affects interpretation of treatment differences.

This decision was based on the assumption, that in real-world applications, an olivine to sand ration of 20 to 80 would be realistic. In addition, we did not want to increase total solid content in the reactors due to size constraints and the degree of mixing through bubbling, which was hampered at higher solid amounts. Therefore, we decided to conduct the experiments with a lower amount of olivine and a fixed total amount of solids in the reactor of 11g. The alkalinity correction with respect to the amount of olivine used is compensating this discrepancy and allows interpretation and comparison of the results.

Line 109: To improve readability, I suggest defining the treatments as: (1) olivine (O),

(2) sand (S), and (3) olivine–sand mixture (OS).

These abbreviations can then be used consistently throughout the manuscript and will also simplify the notation in equations.

We appreciate this suggestion and revised throughout the text.

Line 112: Please describe the bubbling treatment in more detail. What was the gas flow rate? Were all sediment and olivine grains continuously maintained in suspension, or did some material remain deposited at the bottom of the reactors? This information is important for interpreting the differences between turbulent and stagnant conditions.

The gas flow rate was adjusted to keep all material in suspension. This differed slightly between reactors due to small variabilities in tube connections to the reactors. Therefore, we can not report one coherent flow rate for all reactors. We added this information in the revise manuscript as: '*Turbulent conditions in 9 reactors— simulating grain collision from wave action— were created by bubbling with high-flow air from the bottom to keep all material constantly in suspension (Fig. 1).*'

Figure 1 caption (Line 124): Please add the flushing rate to the figure. In addition, the coloured lines currently give the impression that the particles were artificially maintained at a fixed position within the water column. Consider modifying the schematic so that stagnant conditions show a deposited sediment layer, whereas turbulent conditions show partial particle resuspension, which would better represent the experimental setup.

We appreciate the comment and modified the figure accordingly.

Line 136: This should refer to the "pH electrode" rather than the "titrator electrode".

The text was revised accordingly.

Line 137: Please report the accuracy of the measurements. For example, how much deviation was observed from certified reference values?

All pH measurements were corrected to a theoretical Nernst response using Tris at a room temperature of 15°C based on the small temperature range in the cones. All measured pH values were then corrected and converted to the total scale. The precision of alkalinity titrations were brought to be ≤ 4 µmol kg^{-1} by running sequential blank measurements prior to sample measurement. Occasional CRM measurements were made throughout the experiment with absolute values no more than 8 µmol kg^{-1} of the true value. These information were added as: *'The precision of alkalinity titrations was ≤4 µmol kg^{-1} and CRM absolute values deviated no more than 8 µmol kg^{-1} of the true value.'*

Line 139: Rather than reporting the amount of nitric acid added, please provide the resulting acid concentration in the sample, as this is more informative and independent of the sample volume.

We added the volume of sampled experimental fluid, so that this information can be derived.

Lines 202: The commas before and after "average" should be removed. "Average TA" is not a parenthetical expression and does not require commas.

The commas were removed.

Lines 210–211: Regarding the sand correction: the amount of sand differs between the sand treatment (11 g) and the olivine–sand treatment (8.8 g). Therefore, subtracting the TA contribution from the sand treatment assumes that both treatments have identical sand-derived alkalinity release despite different sand/water ratios. Please justify this assumption.

Thank you for highlighting this important point. We completely agree and corrected the sand contribution for the OS treatment accounting for the lower amount of sand present (11g versus 8g).

Line 204: The correct abbreviation for equation is "Eq.", not "Equ.". Please correct throughout the text.

The abbreviation was corrected.

Line 204: The notation for the different accumulation values could be simplified. I suggest replacing ΔTA_sw,cor with simply ΔTA, as it is already clear from the context that the values are seawater-corrected.

We agree with this simplification and changed the notation throughout the text.

Line 212: The equation notation could be simplified for clarity. For example:

ΔTAO = ΔTAOS - ΔTAS

Please consider adopting this simplified notation throughout the manuscript.

We appreciate this suggestion and revised throughout the text.

Line 225: The use of the division line in the equation is confusing. Consider simplifying the notation to:

FO = FOS - FS

which is clearer and easier to interpret.

Same as in the comment before, we appreciate this suggestion and revised throughout the text.

Line 242: Please clarify where TA varies above and below the incoming seawater reference. Does this occur in all treatments or only in specific treatments? The current wording is too general.

This sentence is an introduction sentence and the specific variations for the individual treatments are listed in detail lower in this section.

Line 244: The average TA increase for the olivine treatment appears to be <100 µmol kg^{-1} in Figure 2. Please verify whether the reported value of 102 µmol kg^{-1} is correct and adjust if necessary. In addition, please include variability in the text (e.g., 102 ± 10 µmol kg^{-1}) to illustrate the spread among replicates.

We revised figure 2 with the correct data as reported in the table (>100 µmol kg-1), the revised sand correction data (with respect to your comment to line 210-211) data and the new axis labelling. In addition, we added the standard deviations throughout the text.

Line 246: Please remove "remaining", as the previous sentence already refers to the turbulent treatments.

The word 'remaining' was removed.

Line 247: Please clarify during which period the treatments remained within the uncertainty range of the seawater reference. The current wording is confusing because the following sentence states that turbulent sand treatments showed a clear increase in TA (18 ± 8 µmol kg^{-1}) relative to the incoming seawater.

This section was revised and the sand contribution clarified as follows:

'Alkalinity: seawater corrected

Over the experimental period, TA varies above and below the incoming seawater reference TA (Fig. 2, supplementary Table S1). Especially within the first 48 hours, turbulent treatments from the O and OS experimental treatments yield highest ΔTA increases with 102±34 µmol kg^{-1} and 78±27 µmol kg^{-1}, respectively (Eq. 2; Fig. 2, supplementary Table S1). After 48 hours, ΔTA decreases in both treatments and stabilizes to an apparent steady state with values of 23±10 µmol kg^{-1} and 25±9 µmol kg^{-1} (average of the last six samples, excluding the T+480h sample), respectively. The turbulent S treatments do not show a ΔTA peak in the beginning of the experiment and yield apparent steady-state values of 19±8 µmol kg^{-1}.All stagnant treatments stay within error close to the seawater reference with apparent steady-state values of -2±8 µmol kg^{-1} for the O treatment, 6±11 µmol kg^{-1} for the S treatment and -0.4±10 µmol kg^{-1} for the OS treatment (Fig. 2, supplementary Table S1).'

Line 250: This statement appears to repeat the information provided in Line 247. Please rephrase to avoid repetition.

See above.

Line 257: Please add "(black circles, dashed lines)" after "seawater reference" in the Figure 2 caption.

This information was added.

Lines 257–259: The term "seawater-corrected data" is somewhat abstract. Consider rephrasing this sentence, for example:

Absolute changes in (C) alkalinity and (D) pH are shown together with changes normalized to the amount of olivine added in each treatment and corrected for alkalinity release from the sand.

This text was revised as proposed.

Line 264: Should this refer to Section 3.1 rather than Section 4.1? Please verify. The same comment applies to other figure captions containing this statement.

Thanks for this observation. The reference was revised here and in all other captions.

Lines 264–265: The final sentence should be removed from the figure caption, as it extends beyond describing the figure and begins interpreting the results.

The sentence was removed.

Line 273: Please also provide the mean and uncertainty values for the two stagnant treatments.

The average steady state values including uncertainty were added. In addition, we realized that the uncertainty for the turbulent OS treatment is actually lower than previously reported. We corrected the value to 0.5±0.2 mmol mol^{-1} olivine (former ±0.4 mmol mol^{-1} olivine).

Alkalinity fluxes section: The data from Fuhr et al. (2023) are included in the figure but are not discussed in the text. Please briefly introduce these values and explain the rationale for including them in the comparison.

The data from Fuhr et al. (2023) is discussed further down in the text, in section 4.2. We added a comment in the

Lines 285–286: Please clarify whether the calculated average fluxes exclude the first two sampling points. If so, explain the rationale for this choice.

Average fluxes are based on the last two sampling points excluding the T+480h samples as we wanted to compare steady-state fluxes without the high TA fluxes in the beginning of the experiment. The rational is, that these are likely not representative for the long-term behavior of olivine-deployed OAE. We added 'steady state' and the sampling times in the revised text to highlight this conservative choice.

Line 288: Please replace "Alkalinity (TA) fluxes" with "Alkalinity fluxes (FTA)" for consistency with the notation.

This abbreviation was changed as suggested.

Line 301 The statement: "Error is the standard deviation (1SD) of the last 2 sampling intervals excluding the last one at T+480h (see section 3.1)." is unclear, as it suggests that the standard deviation may be based on only one sampling point. Please rephrase this sentence to clearly indicate how

variability was calculated. Additionally, I suggest moving Table 1 to the Supplementary Information, as it mainly supports the interpretation of the main text.

The sampling points for the calculation of the standard deviation are now specified as: '*Error is the standard deviation (1SD) of the last 2 sampling intervals (T+192 & 288hr) excluding the last one at T+480h (see section 3.1).*'.

We agree with moving the table and combined the average fluxes with the individual measurements in supplementary Table S2.

Table 1: Consider combining average values and standard deviations into a single column (e.g., mean ± SD). This would make it immediately clear that the SD refers to the average value calculated from the final two sampling intervals rather than the maximum TA flux. Furthermore, the highest TA flux values should also include standard deviations calculated from the three replicates.

We combined the average values with the respective standard deviations as suggested, now shown in supplementary Table S2. The highest TA flux is based on the average TA of the three replicates, therefore we can not derive an uncertainty for these fluxes.

Line 317: Please place the range in parentheses to make the sentence more concise.

The text was revised accordingly.

Line 317: Replace "the seawater concentration trend" with "the trajectory of the reference seawater" to avoid repetition of the word "trend" within the same sentence.

The text was revised.

Line 318: Replace: "were higher in DIC" with: "showed higher DIC values" for improved scientific phrasing.

The text was revised.

Lines 324–325: Please mention that the increase in pCO_2 was more pronounced in the stagnant treatments and reference seawater compared with the turbulent treatments.

The information was added.

Lines 343–344: Please provide the magnitude of Mg increases and consider including these data in the main text. Showing changes in concentration rather than absolute concentrations would better highlight treatment effects.

The range of Mg concentrations was added.

Figure 5 caption (Line 359): The caption appears incorrect. Li is not mentioned in the caption and Mg is not shown in the figure. Please revise accordingly.

Thanks for noticing, we revised the caption.

Lines 360–362: This sentence contains interpretation of the results and should be removed from the figure caption.

We consider the highlighting of certain element trends not interpretation and decided to leave it in to facilitate the recognition of different trends in the different experimental treatments.

Line 371: Please clarify which EU guideline values are shown in the figure and provide the relevant references. The European Environmental Quality Standard (AA-EQS) for dissolved Ni in marine waters is 8.6 µg L^{-1} (Directive 2013/39/EU), corresponding to approximately 146.5 nmol L^{-1}, which differs from the value shown in the figure. Please verify and correct these values.

Thanks for noticing. We revised the reference and value (see the revised text in our answer to your comment to line 679).

General comment for Results section: Throughout the manuscript, please include standard deviations whenever average values are reported. This is particularly important when discussing treatment differences and comparing values between experiments.

Standard deviations were added for all reported averages in the revised manuscript.

Lines 390–391 The statement: "In contrast, Sr and Li maximum fluxes were recorded in the turbulent and stagnant olivine/sand treatments." is unclear. Fluxes are calculated for each treatment, so please reformulate. For example: "Elevated Sr and Li fluxes were observed in the turbulent and stagnant olivine–sand treatments."

The text was revised as: '*In contrast, elevated Sr and Li fluxes were observed in the turbulent and stagnant OS treatments.*'.

Line 394: Please clarify the basis for excluding this outlier. If there is no clear evidence that the value results from an analytical or experimental error, it should not be excluded.

Thanks for this point, we revised the text to: '*Whereas the Li fluxes initially increased in the beginning of the experiment at T+4h from 87 to 168 µmol mol^{-1} olivine day^{-1} (with one drop to 8 µmol mol^{-1} olivine day^{-1} at T+74 h).*'

Lines 402–403: Please replace "concentrations" with "fluxes", as this sentence refers to flux calculations.

Thanks for noticing, we revised the text.

Line 410: The statement that the material contained 93% olivine appears inconsistent with Line 105, where the dunite is described as containing 75% Mg-olivine (forsterite). Please clarify this discrepancy.

Thanks for highlighting this discrepancy, the 75% referred to an earlier measured value in Fuhr et al. (2022), while the 93% is our own measurement, which agrees also better with the reported values for Aheim dunite, as you also stated earlier. We corrected the data in the revised manuscript.

Line 423: "Geochemistry" is a very broad section title. Consider replacing it with a more specific title, such as: "Changes in seawater elemental composition"

The section header was revised to '*Geochemistry of the solid material*'.

Lines 424–425: If these elements occur at <1 wt%, they are not considered major elements. Please remove "major" from this sentence. The same applies to Line 427.

The text was revised accordingly.

Lines 438–444: Please clarify which of these observed changes exceed analytical uncertainty. Results should be interpreted more cautiously where differences are within measurement uncertainty.

The text was revised accordingly as: '*Correction of the trace metals to the initial composition showed a depletion of -570 ppm for Cr in the turbulent O treatment (supplementary Figure S6). Whereas, for the S treatment, the sample showed a depletion for Sr of -74 ppm for the stagnant experimental run. The turbulent OS treatment showed enrichment in Ni of +196 ppm and depletions in Cr (-140 ppm), Sr (-115 ppm) and Ba (-54 ppm). The stagnant OS treatment showed depletions of Cr (-62 ppm), Sr (-51 ppm) and Ba (-58 ppm) (supplementary Figure S6). All other elements stayed within error to the initial composition.*'

Line 439: Please change Figure S5 to Figure S6.

Thanks for noticing, we revised the reference.

Line 457: To support the hypothesis of enhanced abrasion in the olivine–sand treatment, representative SEM images of olivine grains from both the pure olivine and olivine–sand treatments should be provided. This would allow direct evaluation of potential differences in grain surface alteration or abrasion.

We do agree that a direct comparison of the olivine and olivine/sand treatment would be desirable but because of budget constraints, we could not conduct these additional measurements. Nevertheless, we decided to include the observations we had from the olivine treatment as already here, the abrasion due to grain collision is nicely visible.

Lines 469–470: This statement is unclear. The preceding sentences already discuss amorphous silica; please clarify what additional point is being made.

Thanks for noticing, this sentence was removed.

Line 493 Replace: "but like the" with: "resembling"

The text was revised accordingly.

Line 484: Please discuss why the incoming seawater pCO_2 was relatively low. A possible explanation (reduced DIC due to photosynthetic activity) is mentioned in the following paragraph; please also include pCO_2 there to establish the connection between low DIC and low pCO_2.

For pCO_2, we added a reference to the following paragraph in which the low DIC concentrations are discussed and included there a reference to pCO_2.

Lines 496–498: The influence of incoming seawater is already correctly accounted for in the flux calculations (Equation 4). Therefore, while inflowing seawater may influence concentration dynamics, it should not directly influence calculated TA fluxes. Please verify and revise this statement accordingly.

We agree with the reviewer and see the redundancy of this statement. We have revised '*That is, mixing of incoming seawater with the reactor fluid was a more primary driver of TA flux in the stagnant, passive, system*.' to: "*This demonstrates that the flushing rate of each cone and, thus, mixing with incoming water, was the main driver of the corrected TA flux compared to any diffusive TA signal in the stagnant cones.*".

Lines 504–506: There was no injected air stream in the stagnant treatments. Therefore, the observed increase in pCO_2 and DIC in these treatments requires further explanation. Similarly, please discuss why pCO_2 and DIC increased in the inflowing seawater over a relatively short period of days.

Thanks for noticing this very important point. We revised the text accordingly: *'Over the course of the experiment, we saw an increase in pCO_2 that translated to a decrease in pH (Fig. 2b, d and 4a, b). For the turbulent treatments, this was likely caused by CO_2 invasion from the injected air stream (Fig. 4a,b; 8). For the stagnant treatments and the seawater reference, the changes in geochemistry must originate from the source pond outside of the laboratory and were likely caused by varying mixing degrees of pond waters with incoming tides.*

Lines 508–510: This interpretation appears too strong. The fact that two outlying points align with the carbonate dissolution trend does not necessarily demonstrate that carbonate dissolution was a significantly contributing process across all treatments. The largest cluster of turbulent treatment data appears to follow the olivine dissolution trajectory rather than the carbonate dissolution trajectory. Please reconsider and moderate this conclusion.

We completely agree and that is what is also stated in the text (lines 527-534 in the original manuscript). We state that despite some samples fall on the carbonate dissolution line, we exclude carbonate dissolution as major TA contributor as there are no carbonates present in the olivine pure treatment that could dissolve in addition to the observation that carbonates are all the time oversaturated (Ω>1, Fig. 4c). Therefore we conclude, despite the fact that the samples fall on the 2:1 line of carbonate dissolution, that olivine dissolution and CO_2 invasion are the major drivers of TA and DIC changes. What might have caused the confusion, is the term 'indicating carbonate dissolution' (line 508 in the original manuscript). To highlight that this is only an option that we consider unlikely, we rephrased this sentence to 'shifting to higher TA concentrations'.

Line 515: The olivine–sand treatment should also be influenced by dissolution of other mineral phases, not only the beach sand treatment. Please acknowledge this or explain why contributions from other mineral phases can be excluded.

Olivine dissolves about 6 times faster compared to feldspars (suspected to be the TA contributor in sand upon dissolution) (Jeandel & Oelkers, 2015) and should therefore be the dominant dissolving phase and TA source in the OS treatment. Nevertheless, we cannot completely exclude feldspar dissolution in the OS treatment and changed the wording from 'impacted' to 'dominating'.

Line 517: The arrows indicating a 2:1 decrease in TA relative to DIC should represent $CaCO_3$ precipitation, rather than $CaCO_3$ dissolution. Please correct the figure accordingly.

Thanks for noticing, this was a typo and was revised accordingly.

Line 519: The statement "with minor carbonate precipitation" appears difficult to conclude from this figure alone, as the data points do not consistently show TA values lower than the seawater reference. Please either provide additional evidence or soften this interpretation.

We revised this sentence by deleting the part about carbonate precipitation.

Lines 519–520: Several aspects require clarification:

- Why is $CaCO_3$ dissolution considered unlikely in the sand treatments, given that Sr concentrations suggest potential carbonate dissolution?
- Why are brackets used around "($CaCO_3$ dissolution)"? Please remove them for consistency.
- Why is photosynthesis excluded while respiration is considered? Since the seawater was UV-treated, both biological processes may be expected to be strongly reduced.

Thanks for highlighting theses aspects, we revised the figure accordingly.

Line 520: The statement: "carbonate dissolution is unlikely to occur due to $\Omega > 1$ for all treatments" requires further consideration. The reported saturation state represents the bulk solution and does not exclude $CaCO_3$ dissolution occurring in microscale environments around reactive mineral surfaces. The observed Sr increase may indicate some degree of carbonate dissolution. Please discuss this uncertainty and avoid making definitive statements unless supported by additional evidence.

We deleted this sentence in the caption and formulated the interpretation more carefully in the main text as:

'4.2 ***Effectiveness of grain collision on mineral dissolution and alkalinity generation***

The effect of grain collision, providing a natural grinding mechanism, is clearly visible in the results of our experiments. All olivine-containing turbulent experiments show a higher average steady-state TA flux (1.0±0.1 mmol mol^{-1} olivine d^{-1}; last two sampling points at T+192&288h) compared to the stagnant treatments (0.4±0.3 mmol mol^{-1} olivine d^{-1}) (Fig. 2, 3; Table 1). This indicates that turbulent conditions are enhancing alkalinity generation, and can be regarded as an efficient application for OAE beach deployments.

Amongst all turbulent treatments, the OS treatment reached the highest TA flux with 11 mmol mol^{-1} olivine d^{-1}, while the O treatment yielded 3.6 mmol mol^{-1} olivine d^{-1} (T+24h; Table 1). Such an initial TA peak has also been seen in experiments by Flipkens et al. (2023), who used the same type of olivine, though with a smaller average grain size (between 84 and 250 µm). This high TA increase in all olivine-bearing treatments could be caused by a larger fraction of small olivine particles (<250 µm making up 11.4 wt.% of the total used olivine (section 2.1)), following the common assumption, that with a higher surface area the dissolution rates increase. Alternatively, the TA increase could be due to fast mineral reaction rates in the beginning of the experiment when minerals are far-from-equilibrium when first brought in contact with seawater (Jeandel and Oelkers, 2015).'

Lines 528–529: Please quantify the difference in TA fluxes between turbulent and stagnant treatments. Additionally, indicate whether these differences are statistically significant.

The text was revised accordingly (see comment before).

Lines 530–534: This section mainly repeats previously presented results. Consider removing or shortening this paragraph to maintain focus on interpretation.

The text was revised accordingly (see comment before).

Line 536: Please change: "Flipkens et al., 2023)" to: "Flipkens et al. (2023)"

The reference was corrected.

Lines 537–540: The fine fraction of olivine is unlikely to dissolve completely within hours based on dissolution rates reported by Rimstidt et al. (2012). Could these fine particles instead have been rapidly lost from the reactor through the 50 µm outlet mesh? Please discuss this possibility.

We don't expect the fine particles to dissolve completely within the first hours of the experiment, but a fraction is likely to dissolve and cause the high TA increase in the beginning of the experiment. With increasing Si concentrations and saturation states, the dissolution should slow down in the course of the experiment and with that also the impact of fine particle dissolution. As only about 0.1% of the material is smaller than 50µm (Sibelco data sheet), we exclude the possibility that the flushing out is an effect visible in the TA concentrations.

Lines 563–565: Please provide further discussion of whether all material was continuously suspended during the turbulent treatments. The higher olivine concentration in the pure olivine treatment compared with the olivine–sand treatment may also contribute to differences in TA fluxes.

If some material remained deposited at the bottom of the reactors, localized accumulation of reaction products around olivine grains could potentially slow dissolution rates. Please discuss whether this process could influence the interpretation of the abrasion hypothesis and how important this mechanism may be relative to grain abrasion.

The material in the turbulent reactors was constantly kept in suspension. However, in some rare cases, the bubbling decreased slightly over night, causing some material deposition on the ground of the reactor, though still moving. As this was solved in the morning, the deposition was not for long and likely not impacting the geochemistry of the 24hr-sampling interval. Even if the dissolution behavior was slightly altered, the effect will show in a larger standard deviation between the three replicates. Therefore, we exclude this process to have had a major impact on our dissolution data.

Line 567: Please remove "in" and clarify whether the experiments by Fuhr et al. (2023) were truly flow-through experiments. This is not clearly described in their manuscript. Flow-through cores are typically applied to coarse-grained sandy sediments, whereas Fuhr et al. (2023) investigated fine-grained sediments. Unless these were indeed flow-through experiments, consider changing this description to "sediment core incubation experiment."

Indeed, this is a good point and we thank the reviewer for raising this topic. The experiments by Fuhr et al. (2023) are indeed rather core incubation experiments as no fluid was circulated through the core from bottom to top. 'Only' the overlying bottom water was circulated in a flow-through manner and we agree that this phrasing might raise confusion. Nevertheless, the minerals were added to and partly incorporated in the surface sediments and by this directly affected by the water flow. We clarified this

description as: '*When comparing our TA fluxes to values observed by Fuhr et al. (2023) in core incubation experiments with overflowing bottom water simulating the low-energy benthic environment, we see a ~3-times higher TA generation at the start of our experiment (11 mmol mol^{-1} olivine d^{-1}). In Fuhr et al. (2023) olivine dissolution was promoted by microbial activity in their low-energy simulation (average of Dun1 & Dun2: 3.6 mmol mol^{-1} olivine d^{-1})….*'

Line 568: The statement regarding higher TA generation compared with Fuhr et al. (2023) appears to only apply to the initial time point. The steady-state TA fluxes appear comparable between the studies. Please reformulate this statement accordingly.

Even if the TA flux is only higher in the beginning of the experiment, this observation is still valid, as also in Fuhr et al. (2023) a high initial flux was observed, which subsequently decreased. In lines 571 to 574 in the original manuscript, we already emphasize that steady-state fluxes approach similar values, so we do not see a need here to revise this sentence. However, we added a comment that highest values only occurred in the beginning of the experiment (see answer to comment to line 567).

Line 571: Consistent with the previous comment, the "magnified dissolution effect" appears restricted to a short initial period rather than representing a general enhancement. Please revise this statement.

Thanks for this consideration. We still think, an interpretation of high TA fluxes as magnified dissolution effect is a valid assumption, as the small grain size used in the experiments by Fuhr et al. should yield similar TA fluxes, if only grain-size dependent dissolution is responsible for the TA generation. So, even being only restricted to a short time, the released TA is contributing to OAE and by this, needs to be considered in assessment studies.

Lines 580–583: The discussion should consider that larger particles may experience stronger collision forces. As discussed by England and Bach (2025), larger particles have greater mass and therefore greater collision momentum, potentially enhancing grain fragmentation or removal of passivating layers. However, the grinding effect may also be offset by the lower reactive surface area of larger particles, making it difficult to separate abrasion effects from grain-size effects.

Furthermore, unless flushing rates are sufficiently high, TA fluxes could potentially become lower or even negative if enhanced dissolution promotes $CaCO_3$ precipitation. Therefore, the proposed benefit of finer particles or higher abrasion should be presented more cautiously and acknowledged as being dependent on environmental conditions.

Thanks for highlighting this very interesting point. Indeed, an (anti)correlation of grain size and grain abrasion is a very interesting next step for future experimental investigations. We included a statement at the end of the paragraph: '*However, this effect might be offset from a lower collision momentum due to the smaller mass of small grain particles (England & Bach, 2025). This correlation and eventual counter acting reactions of grain abrasion and grain size dissolution effect requires further experimental investigations to identify optimal OAE conditions*.'

Lines 589–590: The comparison with previous studies requires a more detailed discussion. Please elaborate on why the observed dissolution rates differ between studies and explain how the plotted values were derived (potentially in the Supplementary Information).

Please also compare key environmental parameters between studies, including:

- temperature,
- seawater pH,
- olivine grain size,
- dissolution rate constants.

Temperature, in addition to pH and grain size, is an important driver of olivine dissolution kinetics and should be considered when interpreting differences between studies.

Thanks for highlighting this point. We added more information on the ambient conditions of the different studies (temperature, salinity and pH) and added a comment about the material. We cannot include dissolution rate constants in the discussion, as they are not available for the study be England & Bach (2025) and ours. The description of the calculation will be added to the Supplement. We revised the main text as follows:

'*In addition to an effective grain abrasion process, we see an enhanced dissolution behaviour, due to the constant exchange with seawater caused by the flow-through experimental design compared to batch experimental setups (Flipkens et al., 2023; England and Bach, 2025). The batch experiments were conducted at similar temperatures (~15°C), salinities (~33) and pH (7.8-8.1) compared to our flow-through experiments (T: 16.3±0.7°C; salinity: 35.2±0.2; pH: 8.28±0.09). In the batch experiments, in which the effect of grain collision on either pure olivine (Flipkens et al., 2023) or pure olivine and a mixture of olivine and sand (England and Bach, 2025), an increase in alkalinity was observed. In all of these set ups, however, the TA accumulation was lower compared to our flow-through experimental results (Fig. 9). Next to the nature of experimental batch setups, the used olivine might have impacted the TA release. While Flipkens et al. (2023) used the same olivine as in our study, England and Bach (2025) used a different olivine (peridotite from Mortlake, Victoria, Australia), the latter with an unknown mineralogical composition and therefore an unknown TA generation potential. In conclusion, our experiments confirm the effectiveness of grain collision on TA generation, and in addition to the previous batch studies, show the enhanced mineral dissolution by constant dilution with seawater, at least when comparing the same starting material.*'

Lines 591–592: Could this enhancement represent only a transient short-term effect? Complete olivine dissolution occurs over decades or longer, whereas this experiment represents approximately 20 days of dissolution. Please discuss how confident you are that the observed differences would persist over longer timescales compared with studies such as Flipkens et al. (2023) and dissolution rates summarized by Rimstidt et al. (2012).

We agree that we can only discuss the TA accumulation results with respect to the experimental time. In our case, we can already observe an approach to steady state values, but everything beyond would be speculation. Especially because the question remains, how long the minerals will remain in the high-energy surf zone and what will happen when the material is transferred to the low energy shelf environment, where dissolution kinetics will change. Therefore we decided not to include this discussion in order to avoid speculation.

Lines 606–608: $CaCO_3$ dissolution is also suggested by the Sr increase. Please check whether this Sr increase correlates with the observed decrease in CaO content (~1%).

We added a statement about the potential correlation with the observed Sr increase as: '*In our study, an indication for carbonate dissolution is only seen in changes in the CaO content of the sand, which decreased by ~1 wt.% in the turbulent sand treatments (supplementary Fig. S6b; Table S4) and eventually by an increase in Sr, though this cannot unequivocally be attributed to carbonate dissolution (see following paragraph).*'

Line 637: The statement: "the additionality problem might occur less than originally proposed" is too strongly formulated. This may only apply under conditions with sufficiently high flushing rates. In environments with very high porewater exchange, $CaCO_3$ dissolution may already be limited because porewater cannot reach undersaturation. Please reformulate this conclusion in a more conditional manner.

We revised the text accordingly (see our answer to your major comment above).

Lines 647–648: This statement is not fully correct. A positive ΔTA does not exclude simultaneous secondary mineral precipitation. Olivine dissolution can contribute alkalinity while some precipitation processes occur simultaneously. You won't always have runaway precipitation. See for example the new pre-print by Moras et al. (2026): https://doi.org/10.5194/egusphere-2025-6144. Please revise this statement accordingly.

We revised the text accordingly (see our answer to your major comment above).

Line 651: The statement regarding $Fe(OH)_3$ precipitation should be reconsidered. Under oxic conditions, $Fe(OH)_3$ formation is expected to occur rapidly (e.g., Griffioen, 2017). Bulk solution saturation states alone may therefore not accurately predict the likelihood of precipitation. Please revise this interpretation.

We revised the text accordingly: '*However, despite the undersaturated state of phyllosilicates and $Fe(OH)_3$ throughout the experiment (supplementary Fig. S7), we cannot rule out their formation as localised precipitation can occur on mineral surfaces (e.g. Griffion, 2017).*'

Line 670: Would SEM analyses be able to detect the presence of passivating layers? Please discuss whether the performed SEM analyses provide evidence supporting or excluding this mechanism.

As discussed above, we could unfortunately not perform SEM analyses on the mixed OS grains and could therefore not investigate potential surface layer formation, which could be detectable via element mapping. Therefore we restrain from including this discussion here.

Line 679: The derivation of the water quality guideline values is unclear. As mentioned previously, the European AA-EQS for dissolved Ni in marine waters is 8.6 µg L^{-1} (approximately 0.147 µM). The value of 64 nmol L^{-1} shown in the manuscript appears to correspond to another threshold (possibly the upper PEC range) rather than the EQS itself. Please clarify the source of these values and correct them if necessary. Furthermore, there is no marine Cr EQS under the European Water Framework Directive. If Cr guidelines from other jurisdictions (e.g., USEPA) are used, this should be clearly indicated.

Thanks for highlighting this very important point, the text and references were revised to: '*These high Ni concentrations exceed the recommended EU guidelines for marine waters, which are 146.5 nmol L^{-}*

[1] (European Union, 2013). For Cr, maximum discharge limits vary per EU member states and in general are found to correspond to 96.2 µmol L^{-1} (European Union, 2005).'

Line 694: Please remove "in real-world application", as this repeats the meaning already expressed earlier in the sentence.

The text part was removed.

Lines 695–705: This sentence is grammatically unclear and could be substantially improved. Additionally, Flipkens et al. (2024) also investigated responses of marine amphipods. Suggested wording: This would also be favourable, as marine amphipods and gastropods have been shown to avoid pure olivine but not sediments containing ≤30 wt.% olivine.

The text was revised accordingly: '*This would also be favorable, as marine amphipods and gastropods have been shown to avoid pure olivine but not sediments containing ≤30 wt.% olivine (Flipkens et al., 2024).'*

Lines 704–705: Please discuss whether these experimental conditions are directly comparable with natural beach environments. In particular, what seawater residence time is represented by the reactor setup?

The residence times and references were added following: '*The high flow rate in our system provided a constant dilution with seawater (water residence time of 6 to 7.5h), which is comparable to natural beach settings (residence time of 1 to 7h; McLachlan et al. (1985); de Sieyes et al. (2011)).'*

Line 706: Please consider discussing the role of flushing rates in controlling carbonate saturation states. High flow rates may maintain carbonate supersaturation, whereas lower flow rates may lead to undersaturation (in the absence of olivine dissolution) or oversaturation due to accumulation of dissolution products from olivine or aragonite dissolution. See, for example, Goossens et al. (2026) *Global Biogeochemical Cycles*, *40*(3), e2025GB008936 for discussion of porewater residence time effects on $CaCO_3$ saturation states.

Thanks for this comment. This sounds like a very interesting study, but would extend the discussion quite a bit in this section. Therefore, we decided not to include the role of flushing rates here, as the discussion is already quite long and we want to avoid a focus shift.

Line 708: Geerts et al. (2025) is a review paper on olivine dissolution rather than a laboratory experiment. Therefore, this citation should be corrected.

The reference was removed.

Lines 716–718: Please discuss the possibility of non-stoichiometric Ni release, as observed by Flipkens et al. (2023) and Montserrat et al. (2017). The applicability of stable Ni isotopes as tracers for olivine dissolution therefore requires further investigation.

Thanks for this comment, we added this information in the text as: *'However, in order to act as a reliable dissolution tracer, Ni release should be stoichiometric which is not always the case as seen in earlier experiments (Montserrat et al., 2017; Flipkens et al., 2023).'*.

Line 744: Please reconsider the statement: "as critical mineral saturation levels can be avoided" An alternative explanation may be that $CaCO_3$ dissolution is already limited under these conditions. Please elaborate.

We fully agree and revised the text accordingly: *'…Thus, our experiments show an argument for an additional mineral dissolution effect under olivine-undersaturated conditions due to the constant flushing with seawater, which resembles natural field conditions more closely. However, our results from the stagnant experiments clearly show secondary mineral precipitation and thereby alkalinity loss, which might be a risk with respect to OAE, when olivine particles are transported out of the high-energy environment to deeper shelf settings. ….'*