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.

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").

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".

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.

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.

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

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

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.

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

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.

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.

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

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

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

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.

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

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.

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

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

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

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.

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.

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.

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.

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.

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.

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

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

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.

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

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.

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

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

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

$\Delta TA_O = \Delta TA_{OS} - \Delta TA_S$

Please consider adopting this simplified notation throughout the manuscript.

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

$F_O = F_{OS} - F_S$

which is clearer and easier to interpret.

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.

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.

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

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.

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

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

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.

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.

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.

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

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.

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

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

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.

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.

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

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.

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

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.

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.

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.

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

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.

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.

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."

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.

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

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.

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

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.

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.

Line 439: Please change Figure S5 to Figure S6.

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.

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

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

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.

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.

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.

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.

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.

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.

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.

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.

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.

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

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

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

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.

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.

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."

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.

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.

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.

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.

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).

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%).

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.

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.

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.

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.

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.

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

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.

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?

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.

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

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.

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.