Geilert et al., investigate weathering of olivine-bearing rock powder in different turbulene/sand regimes. The study is very interesting and provides an important dataset for the assessment of coastal enhanced weathering. I do have some major comments, but I think these can be dealt with in the review process.

I hope my comments help to further improve the manuscript.

Lennart Bach

Major comments:

I think the discussion on the "additionality problem" is not quite correct due to 2 issues.

- Additionality is defined as CDR in a perturbed scenario (here olivine added) minus CDR in a baseline scenario (here sand_only), i.e. Additionality = CDR_olivine-CDR_sand OR CDR_olivine/sand - CDR_sand_only. Geilert et al, investigate additionality on the basis of TA (not DIC) which is totally fine because doing this with carbon is often difficult (or impossible) as it requires exact knowledge of the source of alkalinity. In this case: Additionality = TA_olivine-TA_sand OR TA_olivine/sand – TA_sand_only. The analysis by Geilert seems to focus on the mixed olivine/sand treatment to interpret additionality. However, the mixed olivine/sand treatment is an anthropogenic scenario so that the sand_only treatment must be subtracted from it to estimate additionality (see equation above). When looking at Figure 2C, it appears that there could be a substantial additionality problem. In the turbulent treatments: delta_TA is quite similar in the sand_only and the other treatments after 100 hours, hovering around 20 umol/kg. As such, when you subtract the sand_only from the two olivine-amended treatments then there is not much additionality occurring after 100 hours (but quite a bit of additionality from 0-100 hours). The additionality problem is potentially even more pronounced in the stagnant treatment. Here, delta_TA is around zero, but frequently higher in the sand_only treatment, suggesting that the addition of olivine reduced TA release from sediments. Importantly, for the additionality problem (on the TA level!!) it does not really matter what the source of alkalinity is in the sand_only treatment. It only really matters how high TA emissions are relative to the anthropogenic scenario.

- It is argued that TA accumulation is lower in flow-through scenarios and therefore "that the additionality problem might occur less than originally proposed". When looking at Fig. 5d in the Bach (2024) paper then it becomes clear that additionality costs on TA release were not described as a problem for these scenarios in the first place. It has been argued that the problem occurs in more acidic environments that provide fuel for TA production from sediments. Furthermore, it has been argued that dilution effectively mitigates the issue. As such, I think it is incorrect to state that it could be less of an issue than previously proposed, because it hasn't been proposed as a problem for the conditions studied by Geilert et al.

We thank Lennart Bach for these very important and valid comments on the 'additionality problem'. We do agree that the scenario simulated in the experiments do not consider the main areas where the additionality problem might occur, that are corrosive bottom waters and pore fluids. We revised the text accordingly (please see below).

However, there might have been a misunderstanding with respect to the assessment of additionality as we already conducted the calculations as you suggested in the original manuscript following equation 3 (line 212 in the original manuscript) and shown in figure 2e&f. This misunderstanding might have originated from the abbreviations that we used for the sand and seawater correction calculations. Also based on reviewer 1 comments we revised the abbreviations in the correction calculations and throughout the text, for example in equation 3 to $\Delta\mathrm{TAO} = \Delta\mathrm{TAOS} - \Delta\mathrm{TAS}$, with O standing for olivine, S for sand and OS for the olivine-sand treatment.

We completely agree with your interpretation that additionality occurs at T<100hr and subsequently decreases to about cero at T>100hr (figure 2e&f). This observation is described in lines 530-540 in the original manuscript, though not explicitly naming it 'additionality'. The sand corrected TA variations in the stagnant treatments were described in lines 547-556 in the original manuscript.

In order to account for the very valid points raised by you, we revised the section header of section 4.3 and the text parts accordingly and focus the discussion now on the potential mineral sources of TA in the used sand:

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 ($\Omega_{Ar} > 1$; 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, the reversal can occur, which requires further investigations with respect to OAE applications in deeper, low-energy marine environments.

Additional comments

In general, I think there are some minor grammar issues and some of the sentences were only understandable upon second read. On the other hand, I also appreciate that the text hasn't been homogenised by some AI, so in fact I appreciate the human touch (and authenticity). But it could be useful to check for grammar issues.

Line 23: found that…?

The text was revised accordingly.

Line 30: I don't understand the phrase starting with "within…". Can this be rephrased?

The text was revised as follows: '*This effect rapidly diminishes over experimental time, when an apparent steady state is reached after ca. 74 hours. Towards the end of the experiment, a constant alkalinity release from the turbulent OS treatment is attained with 1.1±0.6 mmol mol^{-1} olivine d^{-1}; within error to turbulent treatments of pure olivine (1.0±0.2 mmol mol^{-1} olivine d^{-1}).*'

Line 34: see major comments.

In accordance to the major comments, we revised the text to: '*…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 108: How effective was the removal of organics by autoclaving the sand? If all organics were lost, there is little potential to dissolve natural sand.

In our experiments, we wanted to investigate the abiotic mineral dissolution not impacted by bacterial-induced reactions. Therefore we decided to autoclave the sand following standard procedures. 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 water chemistry.

Line 132: Unclear if TA was also measured on incoming seawater. Please clarify.

Thanks for noticing, yes, TA was also measured on the incoming seawater. The text was revised to: *'Additionally, three reference samples of the incoming seawater were taken on each sampling day and immediately analyzed for alkalinity, pH, temperature and salinity (WTW Multi 3630 IDS).'*

Line 203: Was TA measured on each sampling day before and after reactor passage?

Yes, the seawater was analyzed also in triplicates before entering the reactor cones and after leaving the reactors. The data are shown in the tables and figures as 'seawater reference'.

Line 245: When you say TA decreases, do you mean the generated amount of TA?

The decrease in alkalinity is relative and refers to the initial peak at T+48hr. Therefore, the decrease in alkalinity refers to the amount of generated TA.

Line 247: Are within error? What is within error?

The text was revised from '*All remaining stagnant 246 treatments as well as the turbulent sand treatments are within error equal to the seawater reference.*' to '*The turbulent S treatments do not show a TA peak in the beginning of the experiment and yield apparent steady-state values of 18±8 µmol kg^{-1}. All stagnant treatments stay within error close to the seawater reference with apparent steady-state values of -2±7 µmol kg^{-1} for the O treatment, 5±5 µmol kg^{-1} for the S treatment and -0.4±7 µmol kg^{-1} for the OS treatment (Fig. 2, supplementary Table S1).'*

Line 410: Is it possible to distinguish between forsterite and fayalite in olivine?

It is possible to distinguish the endmembers, however not based on the XRD but on the XRF measurements (section 3.2.2). The analyses show that the used olivine is composed of 49.38wt.% MgO and 7.75 wt.& Fe_2O_3 (Table S5). This indicates that the olivine tends to the forsterite endmember (MgO), though forming a solid solution series with the fayalite (Fe_2O_3) endmember.

This characterization is mentioned in section 2.1, in lines 105-106 in the original manuscript: *'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.'*

Lines 541ff: This explanation in this paragraph is unclear to me. Both treatments are olivine-bearing, so what is compared here? Sand/olivine vs. just olivine?

Please note that we revised the abbreviations of the different treatments to facilitate reading: olivine (O) treatment, sand (S) treatment and olivine/sand (OS) treatment. The text was revised to:

'*Amongst all turbulent treatments, the OS treatment reached the highest TA flux at 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 is either 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, or by 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).*'

Line 547: Would be more intuitive to talk about delta_TA here.

We agree and refer to ΔTA in the revised manuscript.

Line 551: It is also worth noting that Omega thresholds depend a lot on the availability of seed particles, hence this value is not necessarily transferrable to all conditions.

We agree and revised the text to: '*However, the precipitation of carbonates not only depends on the Ω_{Ar} threshold, but also on the availability of seeding material. Under the stagnant conditions in our experiment, the OS mixture remained undisturbed at the bottom of the reactor, providing nucleation sites and localised carbonate mineral saturation in the pore space could exceed the Ω_{Ar} of on average 3.7±0.5 in the outflowing solution, potentially triggering carbonate mineral precipitation.*'

Line 591: I think the effect of dilution could be overestimated relative to the England and Bach study as they also used another olivine-bearing rock, most likely one with inherently lower forsterite content and therefore TA release potential.

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 agree that in comparison to England & Bach (2025), we can not rule out an effect of the different starting material. Nevertheless, we think that the effectiveness of mineral dissolution by constant dilution with seawater is still a valid conclusion, especially in comparison to Flipkens et al. (2023). We revised the 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.'

Line 648: At least no net loss, but gross loss of olivine-derived TA is still possible if only part was precipitated as CaCO3.

We agree and revised the text to: *'In the turbulent treatments, TA was always higher compared to the incoming seawater reference, indicating the dominance of olivine dissolution over potential secondary mineral formation (Fig. 2 and 3).'*

Line 650: This conclusion is at odds with the section headline. The stagnant treatment, which is much more widespread in an oceanographic context than the extremely turbulent one is arguably more relevant.

We revised the section header to '*Probability of secondary mineral precipitation'*.

Line 658: The second clause in the sentence ("…,though small increases…") is unclear to me. How does SiO2 and MgO increases link with phyllosilicate formation?

The increase in SiO2 and MgO is observed in the solid phase, not in the fluid phase, therefore arguing for phyllosilicate precipitation. To highlight this observation, we revised the text from '*Therefore, phyllosilicate formation likely played a minor role in our experiments with respect to TA, though small increases in the solid concentrations of SiO_2 and MgO are present in the turbulent OS treatments (supplementary Fig. S6c).'* to '*Therefore, phyllosilicate formation likely played a minor role in our experiments with respect to TA, though small increases in concentrations of SiO_2 and MgO in the solid phase are present in the turbulent OS treatments (supplementary Fig. S6c).*'.

Line 684: is the decline due to dilution? Perhaps worth mentioning the reason.

We cannot unequivocally rule out the effect of dilution, but if dilution plays a role, this would only be a secondary effect as the main control are likely decreasing olivine dissolution rates. A similar concentration decrease is also observed for Si and Fe, both originating from the dissolving olivine, the same as Ni. The information was added to the text: '*The fast decline of Ni after T+24h, which is likely due to decreasing olivine dissolution rates, to concentrations below the EU guideline values for the OS treatment limits the risk of potentially toxic exposure for the sensitive organisms in the water column.*'

Line 701-706: See major comments.

We revised the text accordingly to: *'With respect to beach OAE applications, next to natural TA release, the transport, deposition and burial of the olivine grains out of the high-energy environment in the surf zone impose additional challenges. The risk of secondary mineral formation and by this alkalinity consumption in low-energy environments was shown in our stagnant experimental treatments (Fig. 2, section 4.2 & 4.3). If this secondary mineral precipitation will also occur in natural low-energy environments such as the deeper shelf areas still needs to be investigated. As much as this is considered a risk in coastal applications, this process could be counteracted by macrofauna bioturbation, digestion and microbial processes impacting ambient bottom water and pore fluid conditions (Meysman and Montserrat, 2017; Eisaman et al., 2023). Recent studies show, that microbial processes in the benthic*

environment are quite efficient in enhancing mineral dissolution, though in the case of the Baltic Sea, carbonate dissolution was identified to dominate over olivine dissolution (Fuhr et al., 2023, 2024, 2025; Dale et al., 2024). The dissolution efficiency of olivine along the shelf still needs to be investigated but could provide an additional alkalinity source next to the alkalinity generated in the surf zone.'

Line 720: This transport and burial is the argument why I think the stagnant treatment is arguably the more important one (see the comment that refers to line 650). I think it is worth expanding on that because when olivine is added to beaches much of it will likely be buried or moved away from the swash zone quickly.

We agree and revised the text of this paragraph (see comment to lines 701-706).

Line 728: I am not an expert on this but it seems implausible to assume that adding olivine to a beach (with surf) is easier than releasing it with a barge slightly offshore. Politically almost certainly, but also logistically.

We are also no experts on this, but Foteinis et al. (2023) clearly states: '*To avoid the complexity of using marine vessels and specialized equipment for mineral spreading in seawater, the minerals were assumed to be spread on the dry beach width using standard lime-spreading equipment. Alternatively, olivine could be directly unloaded in piles along the coastline and allow waves and tides to distribute the material in the coastal zone, similar to beach nourishment*'.

Line 743: see major comments.

The text was revised to: '*…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. ….*'.