

We are grateful for the constructive and positive feedback on the manuscript provided by the reviewers. Their recommendations have significantly improved the structure and content of the text. We have carefully considered and addressed all comments; our responses are in italic.

The manuscript provides a review of coastal enhanced weathering with olivine minerals, and investigates the state of the art on many of the key factors which determine the rate of olivine dissolution, production of alkalinity, and conversion of alkalinity into actual CDR. I appreciate the effort but think the manuscript would be improved by a more complete overview of coastal enhanced weathering before focusing in on olivine. In addition, I think there are a number of important missing discussion points from the review. I also believe the manuscript would be improved with some restructuring. I elaborate on these points in detail below.

L8 - The authors use the term “coastal enhanced silicate weathering” (CESW) throughout the text. At this point, I think the field has essentially landed on “enhanced rock weathering” (ERW) as the name for this CDR strategy. If the authors wanted to distinguish coastal ERW specifically, I would therefore abbreviate it as CERW. I also would not reference “silicate” weathering specifically, given that CERW with non-silicate minerals is under consideration as well. Its unnecessarily limiting. And anyway “CESW with olivine” is redundant since olivine is a silicate. Altogether, my recommendation is “CERW with olivine” to be consistent with the field.

We have followed the suggestion of the Reviewer. We now use the term “marine ERW” rather than “CESW”. We have followed the suggestion of the Reviewer. We now use the term “marine ERW” rather than “CESW”.

L46 - Rather than citing papers that state storage timescales by referencing yet other papers, I recommend citing the actual papers that establish such estimates of timescale, e.g. David Archers work, Jack Middelburgs work, etc.

We have replaced Meysman and Montserrat (2017) with Archer et al. (2009).

Figure 1 - I would cut images a) and b) from this figure. With regards to a), the image of the beach in Hawai’i and natural olivine beaches aren’t discussed in the text and so no context is given. Plus you cant see any olivine in this picture anyway, so what is it adding? With regards to b) the natural weathering process has been covered at length elsewhere. For a review like this, I would simply assert that CERW is based on the natural process, provide references and move on. Its not a meaningful contribution to this article.

Following the suggestion from the reviewers, we have removed Figs. 1a and 1b. Additionally, we have restructured the introduction and decreased the focus on natural weathering.

L88-89 - “The chemical dissolution of silicate minerals in terrestrial and coastal environments releases alkalinity into the surface ocean, where it drives the uptake of CO₂ from the atmosphere” This is an incorrect summary of how ERW works on land. On land, the alkaline minerals react with, and sequester atmospheric CO₂ directly and the ocean is simply a holding tank for the resulting alkalinity. This is different from ERW in the ocean, where the alkaline minerals react with, and sequester seawater CO₂, which ultimately then drives CO₂ from the atmosphere through air-sea gas exchange.

We agree with the reviewer. After rewriting and shortening the text about natural silicate weathering, the line has been removed from the text.

L100 - Remove Schuiling and Krijgsman, 2006 reference. This paper did not suggest the application of silicates to coastal and shelf environments, only land.

We have removed the reference.

L100 - 119 - I take issue with the framing the authors present here that there are three distinct CERW scenarios. It does not appear to be based on an understanding of sediment transport and coastal geomorphology. To begin, for the “bedload scenario” there is essentially no real-world scenario where a coastal environment is made of only gravel. Coastlines exist with a gradient of particle sizes. There will always be some fine grained material, and any olivine sand that was added to a rocky coastline with almost immediately sort, based on the brazil nut effect, downwards to those finer native particles. Regarding the permeable and cohesive sediment scenarios, I would argue that the permeability (and porosity (Köhler et al., 2013)) of a sediment are simply factors which should be discussed as controls on advection, bioirrigation, as well as diffusion (the latter which was excluded altogether from this manuscript). Coastal sediment will have some component of all three of these processes occurring. Permeability and porosity influence how the relative importance, and magnitude, of each of these three processes differ across coastlines. And of course, other factors will influence advection, bioirrigation, as well as diffusion as well, such as local oceanographic conditions and organic matter content. Taken together, I think the presented framing is overly simplistic and somewhat inaccurate (e.g. diffusion), and the authors would be better served by instead including a discussion of the impact of permeability further on in the paper when they discuss other controls on dissolution rates.

The abstraction into three sediment environments provided a simplified description of the reality of coastal systems, but a useful one. The aim of this manuscript is to discuss factors that affect olivine dissolution in coastal environments. To this end, we provide a framework with three main types of sediment environments found in coastal systems. We do not contend that a single coastal system is solely made up of one of these types. For example, gravel beds do occur in the North Sea, along side more permeable sandy areas, and localized areas with deposition of cohesive muds (Wadden Sea, German Bight). The abstraction into three sediment types is valuable for illustrating the dominant controls on marine ERW exerted, as each sediment type has its own specific transport regime.

However, we agree with the reviewer that the rationale for doing this abstraction could be substantially improved. We now start the discussion by describing how hydrodynamic energy regulates the environmental conditions (grain size, permeability, organic matter, pH) and main transport process (advection, also bioirrigation, vs diffusion) in sedimentary systems, as these factors are strongly correlated with each other. For this reason, we believe that for the purposes of marine ERW, coastal systems globally can be categorized into three broad categories.

Additionally, this framing leaves out entirely the role for water column dissolution in CERW. Any CERW that uses sufficiently small particle sizes will have some fraction of the olivine dissolution occur in the water column through particle resuspension (i.e. suspended sediment). Furthermore, some scientists are working on understanding CERW project sites to intentionally

increase the amount of water column dissolution so as to avoid the messy complications of sediment dynamics. To date, much of the CERW modeling has actually assumed water column dissolution for small particle sizes (e.g. Feng et al. 2017). Water column dissolution - and what, for example, water column pH means for dissolution - should be added to this review.

Water column dissolution is captured by the bedload scenario, where dissolution occurs in ambient seawater (and not porewater with composition that is distinct from the overlying water column).

The reviewer is correct that modelling studies have focused on olivine weathering in the water column. Yet, we are not aware of any publications about experimental studies on olivine based ERW that aim to intentionally increase the amount of water column dissolution.

However, there are many problems and challenges with “intentional” water column dissolution (1) the energy required to produce sufficiently small grain sizes and keep olivine in suspension is very large, which reduces the CO₂ drawdown efficiency considerably (2) fine-grained particles will be also be transported and deposited into cohesive sediment sites.

We address this in section 1.2, as a motivation for why most research now target sediment-based ERW approaches.

Finally, this discussion also leaves out that many coastal environments under consideration for CERW are not beaches or similar sandy coastlines, but rather wetlands - marshes, mangroves, etc. There is a funded field trial of this in the US right now. This should be included in the discussion. <https://oceanacidification.noaa.gov/funded-projects/tidal-wetlands-as-a-low-ph-environment-for-accelerated-and-scalable-olivine-dissolution/>

In this framework, wetlands, saltmarshes and mangroves would be categorized as cohesive sediments.

Figure 2a - should include a depiction of clays as secondary minerals as well, beyond carbonates and metal oxides.

Phyllosilicates formation has been added to the figure.

L135 - 140 - I would reference here the extensive amount of work on ecology and ecotox that has been done, but simply state that it is outside the scope of the review.

We have added references to the ecotoxicology work that has been done. The sentence now ends as: “..., olivine dissolution rates (Hangx and Spiers, 2009; Heřmanská et al., 2022; Oelkers et al., 2018; Rimstidt et al., 2012), and ecotoxicology and ecology impacts (Bach et al., 2019; Flipkens et al., 2021, 2023a; Li et al., 2024).”

L142 - You reference here “Vesta, 2023” which is a monitoring report, but in the next sentence you say there are no reports on the outcomes of field trials. It seems like there is a report on outcomes, since you reference it. Additionally, Vesta (and their collaborators) have generated a

lot of conference abstracts, which I recommend you reference as well. The USGS also did a field trial of olivine in a coastal environment (see link below), the PI on this field trial is Kevin Kroeger. His team has also published a lot of abstracts on their findings at AGU and OSM. Both this field trial and key abstracts should be mentioned. I would also reference the field trial conducted by Planetary Technologies. They added the mineral brucite to the seafloor in Halifax Harbor, Canada. Its not olivine, but it is nonetheless a CERW field trial that warrants mention given how few are being conducted. Theyve published abstracts as well with Dalhousie University. In general, given that coastal enhanced weathering field trials are ongoing, I am surprised how little attention they receive in this manuscript, particularly given that “field trials are needed” is a primary conclusion.

<https://www.usgs.gov/media/images/usgs-and-partners-collect-a-soil-core-massachusetts>

The reviewer is correct that several field trials are ongoing, we have added this information in section 1.3. However, no peer-reviewed publications have emerged from these trials. We have deliberately chosen to only cite results from peer-reviewed studies and do not include conference abstracts.

L145 - You said these laboratory experiments are done under “idealized conditions”. I would argue that these are definitely not “ideal” given that they are closed systems and subject to batch effects, as discussed later in the manuscript.

We have removed the mention of “idealized” laboratory conditions throughout the text.

L178 - Also cobalt.

We have clarified that nickel and chromium are not the only trace metals in olivine, but the two main ones. The sentence now reads as: “Olivine also contains trace metals, notably, nickel (Ni) and chromium (Cr). Ni substitutes the divalent cations in olivine (Mg^{2+} , Fe^{2+}), and its content ranges from 0.2–1.2 mol% (Keefner et al., 2011; Montserrat et al., 2017; Santos et al., 2015).”. Although olivine contains some cobalt, the concentrations are very low (0.01 mol%; Herzberg et al., 2016), so we do not mention this metal specifically.

L190 - 195 - Since you have expanded the scope beyond just silicates here (e.g. carbonates) I would also mention brucite (particularly since Planetary already did a field trial with this mineral, as mentioned above), and anthropogenic minerals too. I think that would produce a better synthesis of the CERW field generally.

As part of the restructuring of the introduction we have moved the comparison of olivine to other rocks to section 1.2, where we also discuss brucite.

Figure 3 - On this figure you have a large olivine deposit labeled in Colorado. Is this supposed to be the Twin Sisters deposit? If so, the Twin Sisters deposit is in Washington State, not Colorado.

The reviewer is correct, we have edited the map.

L251 - 253 - Transport (shipping) of the olivine to the project site is also an important step between production of olivine (mining and grinding) and spreading the olivine. Please include.

We agree with the reviewer and have included the transport step. The section now reads as:

“The entire process of marine ERW can be quantitatively described as a six-step process (Fig. 2a): (1) production of olivine sand from olivine rich source rock (2) transport of olivine to the project site, (3) spreading and deposition onto the seabed (3) mineral dissolution, (4) alkalinity release to the overlying water, and (5) CO₂ sequestration at the air-sea interface. In the dissolution step, the olivine sand deposited during marine ERW will react on top of or within the seabed.”

L257 - The equation here presents R_{diss} as if there is a single dissolution rate attached to the feedstock applied to the seafloor, but further down (L268) the text acknowledges that any feedstock has accessory minerals that don't contribute alkalinity, though the text should include the possibility that the accessory minerals simply contribute alkalinity with a ratio g/g ratio and a different rate. In other words, the feedstock (not a singular mineral) applied to the seafloor will be a mixture of different minerals, with different CDR efficiencies and different dissolution rates but this is not made explicit.

R_{diss} is defined as the specific mineral dissolution rate, meaning that when a rock containing many different alkalinity-producing minerals (e.g. basalt) is utilized, a specific R_{diss} is needed for each mineral present within the rock. We clarify this in the text: “When feedstock is utilized which contains an assemblage of different alkalinity producing minerals (e.g. basalt), each of these minerals will be defined by their mineral specific R_{diss} . An overall R_{diss} for the entire feedstock can be calculated by averaging the mineral-specific R_{diss} based on mass ratio.”

L274 - Should also include a function that accounts for the potential of alkalinity loss in the water column to biotic carbonate formation, for example. This framework also leaves out the potential for incomplete air-sea gas exchange and a key factor that can reduce FCO₂.

We agree with the reviewer that incomplete air-sea gas exchange should be taken into account in equation 3 to 4, and have included a factor “ η_{AT} ” which stands for the alkalinity transfer efficiency. This is a time-dependent factor that denotes the proportion of bottom-water alkalinity available for air-sea gas exchange, and is further discussed in section 3.3.

Loss of alkalinity from marine ERW due to biotic carbonate formation in the water column is possible, but unlikely, and we now discuss this more in detail in section 3.2.3. Past marine OAE studies have not shown increase in biotic carbonate production (Ferderer et al., 2022), at the same time olivine addition promotes the growth of all phytoplankton and silicifying algae in particular, which outcompete carbonate-forming phytoplankton (Bach et al., 2019; Li et al., 2024), therefore, we do not include this as a factor in the equation.

L284 - In general, this section and the overall manuscript should include a discussion of porewater CO₂ as a limiting factor for olivine dissolution. If porewater exchange is low, or respiration is low (low organic matter), etc CO₂ consumption may be the rate limiting factor for olivine dissolution.

Past olivine dissolution experiments have demonstrated that the CO₂ concentration does not affect olivine dissolution rates directly, but only indirectly through changing the pH (section 3.1.1). The effect of pH on olivine dissolution, and drivers behind the pH (including the amount of oxic mineralization of organic matter), are discussed specifically in section 3.1.2 and throughout the manuscript.

L298 - Section 3.1.1 - Generally speaking, I think this section would be improved by a discussion of the reasons why there is so much spread in the dissolution rate data (and therefore, how we can improve on it). For example, mineral impurities in the tested materials or the method of rate quantification (e.g. the use of Si concentrations which gives net dissolution rate) <https://www.sciencedirect.com/science/article/abs/pii/S0016703719307811>)

We agree with the reviewer that this is an important point, and have modified the end of section 3.1.1: “The 95% confidence interval (CI) around the value of k_a is large, spanning an order of magnitude log units at $pH < 5.6$ and several orders of magnitude at $pH > 5.6$ (Fig. 4a). This large spread in the data has previously been discussed by Oelkers et al. (2018) and Rimstidt et al. (2012), who attributed it to mineral purity (pure olivine dissolves faster e.g. Golubev et al., 2005), initial incongruent dissolution of olivine, lack of common data format and inconsistent reporting, differences in sample grinding and preparation, and most importantly, to inaccuracies in reactive surface area measurements section 3.1.5).”

Additionally, in section 3.1.5, we clarify that the inconsistency between BET surface area and mineral dissolution rates is one of the main reason for uncertainties: “Interestingly, this large increase in BET was not accompanied by a concurrent increase in dissolution rate (Grandstaff, 1978), highlighting that there can be an increase in the total surface area while the reactive surface area stays the same. Rimstidt et al., (2012) identified this inconsistency as the major reason for the large data spread found between dissolution experiments (Fig. 4a).”

L506 - In general, its not clear to me why carbonate precipitation isnt included in this section on secondary mineral precipitation. Why distinguish between clays + metal oxides and carbonates?

L513 - “Clay formation has been proposed as a third potential mechanism...” but sepiolite is a clay. Youve just mentioned clay formation in the prior sentences. **L579** - This section is titled “formation of other clay minerals” but then talks about metal oxides. Though metal oxides are also discussed in section 3.3.2. Combine. **L607** - This “secondary reactions” section (3.3) is a combination of mineral formation (though excluding clays, which is section 3.2) plus a discussion of carbonate dissolution and additionality. My recommendation is to combine section 3.2 and the mineral formation part of section 3.3 into one section on secondary mineral formation, and then make section 3.3 strictly about mineral dissolution and additionality. I think this would make for a better logic flow.

The separation of sections 3.2 and 3.3 was made to distinguish between reduction of the net alkalinity release due to incomplete dissolution of olivine (less alkalinity is being produced) and precipitation of dissolution products (alkalinity produced through dissolution is consumed). However, following recommendations from both reviewers, we have merged sections 3.2 and 3.3 as for some secondary minerals, it is not clear which pathway has led to their formation. In merging the sections, we have also restructured and clarified the discussion about clay formation.

L658 - I think this discussion of carbonate dissolution would benefit from a brief review of where carbonate dissolution and the additionality question are likely to be significant (or not), as discussed in Bach, 2024. I think in the present manuscript, important context is missing that this additionality consideration is not likely to be important everywhere.

We have expanded section 3.2.3 to include a description of the environmental conditions that may lead to additionality issues in marine ERW (sediments where oxic mineralization of organic matter drives a lowering of the pH) and discuss the impact on choice of location for olivine application.

L672 - 682 - This paragraph, and discussion of water column processes, comes a bit out of nowhere. I would recommend that consideration of the water column should represent a unique step that must be assessed for CERW efficiency. This paragraph ends with the assertion that “The slow alkalinity release from the seabed during CESW implies a substantial dilution in the overlying water, thus avoiding local alkalinity excursions, and hence the chances of immediate CaCO₃ precipitation in the overlying water are minimal.” but this assertion isn't based on anything quantitative. This would be substantially improved with even a back-of-the-envelope calculation. I'll also add that referencing biotic calcification (which is not said specifically here) would benefit the manuscript.

We now discuss biogenic CaCO₃ formation in the water column and runaway precipitation within the same paragraph, and have added a back-of-the-envelope calculation of the alkalinity release and resulting changes to CaCO₃ saturation state.

L704 - This section should include a discussion of how water mass sinking and incomplete air-sea gas exchange also play a major role in determining CO₂ drawdown - references such as He and Tyka, 2022 or Ho et al., 2023.

We agree. The effect of water mass sinking and incomplete air-sea gas exchange on the CO₂ sequestration efficiency are now discussed in section 3.3.