

*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 by Geerts et al. provides an overview of different types of coastal applications using enhanced silicate weathering as carbon dioxide removal method. Different coastal areas (ranging from bedload movement to permeable and cohesive sediments) are discussed with their respective advantages and disadvantages. The impacts of relevant geochemical parameters, such as pH and temperature, are modelled for the distinct scenarios and the uncertainties as well as knowledge gaps emphasized. While the manuscript provides a nice overview of the current state of knowledge, I'm missing an outlook for the most promising areas of application despite the current knowledge gaps. I would appreciate an effort to model the deposition of olivine in the different coastal environments taking also into account the accessibility of the deployment site and feasibility in the sense of political will and economics into account. This modelling effort could also take into account the different temperatures in the vicinity of the equator versus the lower solubility in comparison to northern and southern latitudes.

*We agree that an in depth exploration of suitable deployment sites, a full techno-economic analysis as well as a discussion societal, legal and political considerations, would be highly interesting and worthwhile. However, this would require a very large endeavour, which goes way beyond the normal revision. Moreover, it would imply the collection and presentation of a large amount of information on top of what's already presented now. Given that the present manuscript is long, we consider this is outside the scope of the present review. Here, we'd like to focus on the geochemical feasibility of CDR via marine enhanced rock weathering (ERW) with olivine, which will e.g. provide crucial knowledge for a downstream techno-economic analysis.*

*Still, we have taken the remark as a crucial point of attention in the revision, and we now stress - where possible and relevant - the importance of geographical location throughout the manuscript (e.g., section 2.2 "Availability of olivine", section 4 "Conclusions and future outlook").*

Next to this more elaborated outlook, I am missing some literature or would replace literature investigating enhanced rock weathering in the terrestrial environment with publications explicitly investigating the minerals of interest in the marine environment (see comments below). As this manuscript is a review, I would carefully check all references and verify their applicability, especially in the context of marine coastal OAE.

Nevertheless, I enjoyed reading the manuscript and recommend publication after revising the two major points mentioned above and minor comments below.

*We have carefully checked the manuscript, and where needed, we have changed references to studies from marine environments. In some cases, references to terrestrial ERW have remained due to a lack of studies in marine systems. In such cases, we now explicitly mention that these studies dealt with terrestrial ERW.*

Line 23: Typo field conditions

*This has been corrected.*

Line 45: With respect to the cited literature and to my best knowledge, the estimated lower potential is 0.5Gt CO<sub>2</sub> yr<sup>-1</sup> instead of 0.1Gt CO<sub>2</sub> yr<sup>-1</sup>.

*The reviewer is right that there are many different CO<sub>2</sub> sequestration potential ranges reported in literature. The range used in the manuscript is derived from NASEM (2022) which reported a uptake potential of >0.1 to 1 Gt CO<sub>2</sub> yr<sup>-1</sup>. We have added a ">" that was previously omitted from the text.*

Line 48: I assume, Fig. 1a is not the correct reference here. These olivine beaches constitute more the exception on Earth's surface than the norm as you also show in your map in Fig. 3. Either refer to Fig. 1b or put a picture of a mountain drained by rivers.

*Figure 1a has been removed following comments from both reviewers.*

Line 50: What is meant by organic C sequestration? Blue carbon or enhanced primary production through fertilization? Please add a short explanation.

*Organic carbon sequestration encompasses both blue carbon and enhanced primary production, as well as land-based approaches such as afforestation. We omitted the mention of organic carbon sequestration to avoid confusion, as pH buffering is a benefit unique for OAE. We have clarified this:*

*"Moreover, OAE has the important benefit of counteracting ocean acidification, which is not the case for other CDR techniques that only target CO<sub>2</sub> sinking, such as reforestation on land, or blue carbon and ocean fertilization in the marine environment (Campbell et al., 2022; Caserini et al., 2022; Meysman and Montserrat, 2017)."*

Line 53-56: The publications by Eisaman et al. (2023) and/or Rau et al. (2018) should also be cited here?

*Thanks for the suggestion. We have added both references.*

Line 72: The publication by te Pas et al. (2023) investigates ERW in the terrestrial environment and conducted experiments solely in the context of soil applications? I highly doubt that mineral dissolution rates are comparable in soils and seawater matrix and I definitely wouldn't cite this publication here (and throughout the manuscript), in a study focusing explicitly on coastal marine applications. Also the other two cited papers don't appear to be very well fitting for OAE. I would rather cite publications explicitly investigating different mineral in the context of OAE, such as Hartman et al. (2013), Bach et al. (2019), and Renforth and Henderson (2017).

*We agree with the reviewer and have removed te Pas et al. (2023), as well as Huijgen et al. (2006), as their focus was on mineral carbonation. In their place we added references to Hartman et al. (2013), Bach et al. (2019), Renforth and Henderson (2017), and NASEM (2022).*

Line 74-75: why did you choose to concentrate on CESW via olivine addition in your review and not on the other methods discussed before? Motivation is not clear.

*We focus on olivine as it is the mineral that has so far received most interest in the context of marine ERW. This interest stems from its fast weathering rate, high CO<sub>2</sub> uptake rate and relative abundance. We have further clarified this and stress the motivation to focus on olivine earlier in the introduction, and discuss the use of olivine relative to other minerals already in section 1.2.*

Line 93: I wonder, if it is actually possible to compensate anthropogenic CO<sub>2</sub> just by ‘sitting it through’, if emissions continue as business as usual. I would add a comment, that this scenario is only possible when emissions are cut drastically.

*We agree with the reviewer. When emissions are halted, an excess of CO<sub>2</sub> will still be in the atmosphere. This excess of CO<sub>2</sub> will eventually be sequestered by natural silicate weathering, but this is only possible when emissions are halted. This has been clarified:*

*“However, the timescale of this response is too slow (> 10,000 years) for society. Even if emissions were completely halted, we would have to “sit through” an extended period of global warming before the excess of anthropogenic CO<sub>2</sub> is removed naturally (Archer et al., 2009).”*

Line 148: Also Fuhr et al. (2024) investigated OAE with sediments in their experiments.

*The study by Fuhr et al. (2024) has been added here and throughout the manuscript where relevant.*

Line 189: Error in the anorthite formula and also here, I would add a comment that dissolution rates of these minerals were not investigated in seawater matrix and might be lower.

*The formula for anorthite has been corrected, and we now specifically mention in the text that these rates are measured in freshwater laboratory experiments. The text has been moved to section 1.2.*

Line 238 ff: I doubt that this paragraph is very relevant, as the occurrence of olivine beaches is globally very low (as indicated in your Fig. 3). I don’t think, anyone is considering this option seriously for CDR, so I would delete this paragraph and also the two points on the map in Fig. 3.

*We have removed this paragraph and edited Fig. 3 accordingly.*

Line 334: I would state more explicitly, that Rimstidt et al. defined a pH of 5.6 and higher as basic, as a pH below 7 is normally not described as basic.

*We have clarified this in the text: “The rate equation was solved for pH 3.13 and 8.22, respectively, which were the average pH values of experiments which Rimstidt et al. (2012) classified as “acidic” (pH < 5.6) and “basic” (pH > 5.6).”*

Line 387 and 595: The experiments by Fuhr et al. (2023, 2024) were conducted at low T (~10°), alkaline pH, and in the 2024 publication also under anoxic conditions.

*The experiments by Fuhr et al. (2023, 2024) and Flipkens et al. (2023) were indeed conducted at low temperatures and seawater pH, but they did not report  $E_a$  values which is the focus of line 387. However, we have included references to Fuhr et al. (2023, 2024) and Flipkens et al. (2023). The text now reads as: “Only recently have studies been conducted at environmentally relevant conditions (pH ~ 8 and temperature 0–25°C, e.g., Flipkens et al., 2023b; Fuhr et al., 2023, 2024). However,  $E_a$  was not reported in these studies. The value of  $E_a$  is thus still uncertain for natural conditions, pinpointing an area for further research.”*

*We have revised line 595, now reading as: “Most mERW experiments have been conducted in oxygenated seawater and are not fully representative of sedimentary conditions (e.g. in terms of  $O_2$  and pH), and studies in sediments under hypoxic-anoxic bottom water have not investigated the formation of secondary minerals (Fuhr et al., 2024).”*

**Line 479:** Would be nice to include a comment about cable bacteria here: Fuhr et al. 2023 with reference to Meysman et al. (2019). In addition, the recent study by Li et al. (2024, <https://doi.org/10.1016/j.scitotenv.2023.168571>) investigates the impact on diatoms by olivine dissolution as well as the effect that diatoms have removing the passivating layer. This study should definitely be included here.

*We have added a brief mention of the effect of cable bacteria through a pH change in section 3.1.2 (Impact of pH on olivine dissolution), as the focus of section 3.1.6 is specifically on passivating layer formation.*

*The study by Li et al. (2024) has been added and is discussed briefly in section 3.1.6.*

**Line 580:** Why not turn it around then? Saponite (former bowlingite).

*We agree, this has been rewritten and clarified.*

**Line 607 to 625 + 657:** I don't understand the separation of Section 3.3 from the previous sections, as it also deals with secondary precipitates. Also, the formation of secondary clays can play a major role during CESW, as described for example in Griffioen (2017) and Fuhr et al. (2023), so I wouldn't exclude it here. The additional problem described by Bach (2024) is indeed important and should be highlighted. As you do this in line 657ff, I would recommend to delete this paragraph here to avoid repetition.

*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.*

*The additionality problem is now only described in section 3.2.4 (“Stimulated precipitation and inhibited dissolution of carbonates”).*

**Table 4:** In this sequence, it is not clear why you use fayalite instead of forsterite here, as fayalite makes up ‘only’ 6-20mol%, as you mention in line 684? I would bring this argument (your lines 684 to 688) earlier or make a comment in the caption of Table 4.

*We have clarified the table caption: “The  $Fe^{2+}$  stems from fayalite, as olivine typically comprises of 6–20 mol% Fe.*

**Line 781ff:** Personally, I’m not a big fan of referring to symbols instead of descriptions in flowing text. The repetitive look up for explanations disturbs the reading flow.

*We have added the full parameter names.*

## **Figures**

**Fig. 1:** (a) As nice as this picture is, I don’t think it adds much value to the discussion and I would remove the discussion about olivine beaches completely from this manuscript. (b) what about alkalinity-consuming process, e.g. carbonate formation?

*Following the suggestion from the reviewers, we have removed Fig. 1a. Fig. 1b was removed according to a suggestion from reviewer 2.*

**Fig. 2a:** I would include phyllosilicates here as well.

*Phyllosilicates formation has been added to the figure.*

**Figure 3:** Would be nice to include in this map, where currently dunite is mined and which of the deposits are realistic candidates for future exploration considering social, politic, and economic aspects.

*We agree with the reviewer that information about operational mines would be a relevant addition to the figure. However, while it is well known where certain commercial mines are operating (e.g., Norway, Spain, Italy, Turkey, Japan), information about the exact reserves that are exploited at the moment is not always available. To avoid confusion, we have opted not to include this feature in the figure. A discussion about social, political and economic considerations of dunite mining is beyond the scope of this review.*

**Fig. 4a:** is there a reason, why saline data points in the legend are much larger compared to the other? What are 75 CI and 95CI? Explanation missing in caption.

*Saline points are enlarged as these data are most relevant for marine ERW, emphasizing that dissolution rate data at seawater pH are lacking. The explanation of “CI” had indeed been omitted in the caption, we have now clarified that it stands for confidence interval.*