

## Reviewer 1:

Minor comments (line numbers refer to track changed manuscript):

We thank the reviewers for their insightful comments, all line references refer to the new track-changed document.

Figure 2a: any reason, why you leave iron sulfide precipitation out since you discuss it in detail in the secondary precipitation section?

We only included the most important secondary reactions in the figure for visual clarity. mERW aims to utilize sediments that are sufficiently aerated to prevent saturation effects (which prevents FeS formation and so FeS formation is not included in the overview of Figure 2).

Line 646: I would add 'pore waters' to the sediment, as this is where the dissolution products build up. '...contribute to the build-up of dissolution products in the sediment pore waters and promote...'.  
.

Changed accordingly.

Line 656-659: The last part of this sentence is not correct. All of the cited publications on olivine dissolution in sediments refer to secondary precipitates, especially carbonate precipitation (Bach (2024) and Fuhr et al. (2024) observed no secondary carbonate formation and discussed the reasons; Fuhr et al. (2023) suspected it at the end of the experiment).

The reviewer is correct that Bach (2024) and Fuhr et al. (2024) indeed suggested that secondary mineral formation may have occurred in their experiments, especially calcium carbonate formation, as the saturation index for calcium carbonate was elevated. However, our point was that calcium carbonate precipitation was not measured/demonstrated directly in these experiments (but indirectly inferred from ore water saturation state). We clarify on line 577-578.

*“... and studies conducted on olivine weathering in sediment have indicated that secondary mineral formation is possible, but not direct quantification of precipitates has been performed.”*

Line 783: I would restructure the sentence, as temperatures between 0 and 10 °C are actually quite common for coastal systems.

We agree with the reviewer. That said, the range reported here was to represent coastal systems around Earth. Indicating that even in warm coastal waters serpentinization would still be unlikely. We have adapted the text on line 633-634:

*However, serpentinization is unlikely during mERW as the process likely is negligible at the temperature range of coastal systems (0–35°C)*

Lines 789-790: I suggest to delete the 'for soils' at the end of the sentence to avoid duplication of the word soil.

Line 641: Agreed, we have removed the first mention of “soils”, now reading:

*“Even though the formation of saponite and iddingsite during olivine dissolution has been historically well-documented for soils”*

Line 945 and 950-952: see my third comment about secondary mineral formation.

Line 752-754 (former line 945): Changed sentence to:

*Most mERW experiments have been conducted in oxygenated seawater and are not fully representative of sedimentary conditions (e.g. in terms of O<sub>2</sub> and pH), and studies in sediments under hypoxic-anoxic bottom water have not directly measured the formation of secondary minerals (Fuhr et al., 2024).*

Line 758-759 (former 950-952): We have clarified that these authors have not quantified secondary mineral formation.

## **Reviewer 2:**

Note, the line references by the reviewer are for the accepted changes document, we reported changes in reference to the new track-change document.

Line 13: No “peer-reviewed” results are available

Line 13: Suggestion implemented.

L185: For references on ecological impact, I would recommend adding more recent papers that provide empirical results such as Guo et al., 2024, Jankowska et al., 2024, and Hutchins et al., 2023. If this is too many references, consider cutting Bach et al., 2019 as this paper was just conceptual (i.e. didn't provide actual data)

Line 224: we have changed the references accordingly.

L187: The Vesta project has concluded (not in the first stage of execution).

Line 187: Correct, we have changed the sentence to: “..., or are in the first stage of execution or results are being analyzed (e.g., Cornwall, 2023; USGS, 2023; Vesta, 2023)”

L222: Would reword this given that the Cr is not contained within olivine, but rather within chromite. So, dunite contains Cr.

Line 223: We have changed “olivine” to “olivine-rich rock (dunite)”.

L227: Again, recommend better references here. See above comment.

Line 227: changed accordingly.

L324-L325: This is overly simplistic. The likelihood of saturation is a balance between porewater exchange rate AND dissolution rate. Meaning, for the same porewater exchange rate, smaller particles, which dissolve faster, will drive saturation faster than larger particles.

Lines 327-328: The reviewer is correct that application parameters, such as grain size and the field specific dissolution rate will determine the rate at which dissolution products may build up. However, the aim of lines 327-328 was not to discuss when saturation effects occur, but rather to highlight that saturation are assumed not to occur in all three application scenarios. We later, explain that this assumption likely will not hold for cohesive sediments where the porewater exchange rate is lower than that of other sediment types. Nevertheless, we have clarified this further on line 328, now stating:

*“However, saturation effects are expected to occur in cohesive sediments with little advection or biological irrigation, or when dissolution rates are very high (e.g. when small grain sizes are used).”*

L361: You note incongruent mineral dissolution as one reason for spread in data. It could be good to include a sentence on the potential research avenue of improved olivine dissolution rate quantification using Si isotopes, rather than Si concentrations. The Si isotope method is a well established approach to measure silicate mineral dissolution rates while removing the confounding factor of secondary clay formation. To the best of my knowledge, this has not been done for olivine before.

Line 365: We would like to thank the reviewer for the suggestion. We have amended this to line 365-369, which now reads:

*“The problem of incongruent dissolution as well as the formation of secondary clay minerals could be potentially constrained through silicon isotope analysis (Chemtob et al., 2015; Gruber et al., 2013), if the source dunite rock has an isotope that is sufficiently distinct from the silicate sources in the application site sediment. However, such isotope analysis has not yet been performed in mERW studies and could be an avenue for future research.”*

L404-405: Looking at Figure 4, it seems there are plenty of data on olivine dissolution at porewater pHs.

Indeed, there are dissolution rates at pore water pH values, but not in saltwater conditions, which are relevant for mERW. We have clarified on line 412-413:

*“The lack of data on olivine dissolution rates at pore-water pH in marine conditions and the absence of suitable spatial maps...”*

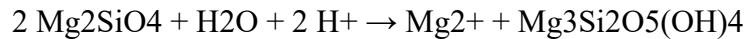
L525: Bearat et al. 2006 empirically tested particle abrasion of olivine as well and found it make a big difference.

Lines 531-534: The reviewer is correct that Béarat et al. 2006, also discusses the effect of grain abrasion, however, we opted not to include this reference here for two reasons. Firstly, this study focused on carbonation of minerals, which is a method where (in this case) olivine is exposed to high temperatures and pressure. Secondly, passivating mineral formation is much more pervasive in carbonation experiments than they are in mERW studies. We have amended lines 539-541, to include the experiments of Béarat et al., (2006), reading: *“Particle abrasion of olivine has also been found to strongly influence the rate of mineral carbonation at high temperature and*

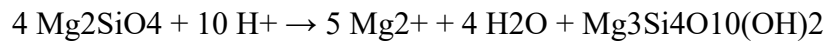
*pressure, where the formation of passivating layers is likely more pervasive compared to mERW (Béarat et al., 2006)."*

Table 3: My reference equations for these reactions give different values for YAT. Also relevant for section 3.2.1 (e.g. L590).

Serpentine + Chrysotile + Lizardite: YAT = 0.25



Talc: YAT = 0.3



*We derived the serpentinization equations from Griffioen, (2017). A critical difference to the equation provided by the reviewer and the one we used by Griffioen is that in the reaction of Griffioen (2017) brucite is formed. The original reaction reported by Griffioen (2017) reads:*



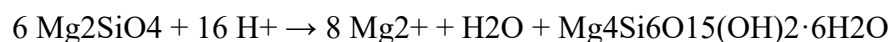
*Assuming brucite immediately dissolves or is not formed we get a formula akin to the one provided by reviewer 2 (written in function of H<sub>2</sub>O and OH):*



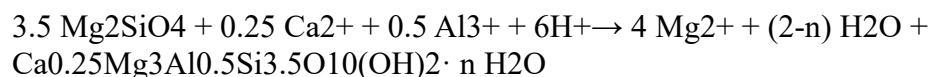
*In the latter case, alkalinity is produced as represented by the 2 moles OH<sup>-</sup> produced. this means  $\gamma_{AT}$  equals to 0.25, however when brucite is formed instead,  $\gamma_{AT}$  equals 0. We have clarified the potential role of brucite now more clearly in the text, line 755 now reads: "When the brucite formed during serpentinization dissolves further,  $\gamma_{AT}$  increases accordingly to 0.25".*

*In the case of talc formation, no reactions were reported as talc formation from olivine proceeds via or co-occurs with serpentine formation (as discussed on lines 788-791). Nevertheless, we have added talc formation as a reaction to Table 2. The value of  $\gamma_{AT}$  we derive is different from the reviewer as talc formation produces 10 mol alkalinity following 4 moles olivine. Ideally 4 mol olivine would produce 16 mol alkalinity so that talc formation reduced the alkalinity production by a factor  $\gamma_{AT}=0.625$  rather than 0.3.*

Sepiolite: YAT = 0.65



Saponite (x ≈ 0.3-0.6, using x = 0.5 illustratively): YAT = 0.43



*The equations used for sepiolite and saponite were taken from Isson and Planavsky, (2018). We are grateful to the reviewer in taking note that  $\gamma_{AT}$  in both reactions was incorrectly referenced; we had overlooked the need to subtract the alkalinity consumption caused by precipitation from*

*the alkalinity produced during the primary weathering reactions when the tables were merged. As such the correct  $\gamma_{AT}$  are 0.67 and 0.43 for sepiolite and saponite formation respectively, rather than 0.33 and 0.60. Following the comments of reviewer 2, we have carefully revised the table formulas and references again. To this end, we noted that a reference to Griffioen (2017) for serpentine formation was inadvertently removed from the first version when merging the tables. The reference is now restored again.*

L751: I would add a reference for the new Zhou et al., 2024 paper, and include its findings in your discussion.

*Line 769 and further: The reference to Zhou were included throughout the text. Since their results were very similar to the results of He and Tyka (2023), we have not discussed their results separately. However, we have added a critical note on their findings and how they translate to mERW specifically line 775-779 now reads:*

*“High values for  $\eta_{AT}$  were also associated with highly stratified systems, since the surface-released alkalinity remains in contact with the atmosphere for longer (He and Tyka, 2023; Zhou et al., 2024). However, these model results cannot be directly applied to mERW, since alkalinity is released from the sediment, and not directly within the surface water, and so high stratification would prevent sediment-borne alkalinity of reaching the surface.”*