Replies to the comments on "**Effects of precipitation seasonality, irrigation, vegetation cycle and soil type on enhanced weathering – Modelling of cropland case studies across four sites**".

G. Cipolla, S. Calabrese, A. Porporato, L.V. Noto

Review of revised manuscript No: egusphere-2022-196

General comments:

Referee: *Most of my comments have been addressed, but there are still a few points that need some attention:*

- *The other reviewer mentioned that some explanation of how to read heatmaps would be useful and I certainly found this to be the case when I looked at them again; it took me a moment to remember that the Y axis represented simulation year. Day of year is on the X axis which seems obvious to me, but it is still worth mentioning. All these figures have colour bars so an explanation of what the colours mean was less important, at least for me. In any case please say what the axes tickmark values represent, either in the text or (preferably) in the caption of the first heatmap figure, e.g.: Figure 6. Time-series heatmaps (X axes: day of year, Y axes: simulation year) ...*

Response: Good suggestion! We specified what the axes tickmark values represent in the caption of Figure 6.

- *Please provide a reference for the laboratory rates being considered in the following revised text. One good possibility would be Bandstra et al. 2008 (in Brantley, Kubicki and White Kinetics of Mineral Dissolution): "The order of magnitude of weathering rates provided by our model is more typical of the field environment with respect to those achieved in laboratory conditions (e.g., Bandstra et al. 2008)."*

Response: Thank you for the insight. Done.

- *The immediately-following revised text stating that the modelled rates are "similar" to those of Amann et al. (2020) is misleading because the modelled weathering rates are an order of magnitude faster than those of Amann et al. Your rates are a lot more similar to those of Renforth et al. (2015, you already cite them), although their study used soil without plants. It is best to state that both the modelling and mesocosm studies achieved rates that are slower than those observed in the laboratory, and then give all the values for readers to see for themselves, e.g. "This was also the case for the mesocosm studies of Renforth et al. (2015) and Amann et al. (2020). Our rates (10^-11.67 Iowa, 10^-11.79 California, 10^-12.50 Padan and 10^-12.32 Sicily, mol Olivine/m2/s) are comparable to theirs: 10^-12.7 to 10^-11.8 mol Olivine/m2/s (Renforth et al 2015), 10^- 13.12 and 10^-13.75 mol Olivine/m2/s (Amann et al. 2020)."*

Note that the spread of dissolution rates observed at similar pH can approach or exceed an order of magnitude even at the same experimental scale, as can be seen in the laboratory data shown

for olivine by Bandstra et al. (figure on page 809 in the Appendix of Brantley, Kubicki and White 2008).

Response: This is a very good point. We rephrased the sentence as suggested.

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General comments:

Referee: *The authors carefully considered each and every one of my (many) review comments, thereby accepting nearly all suggested changes, answering my scientific questions whilst also clarifying these topics in the new manuscript where appropriate, and providing sound reasons for those suggested changes they did not implement.*
Whereas the first manuscript was already an interesting and important publication, the revised manuscript is better structured, more easily understandable and more complete, with extra information and better readable figures.
As I have no background in computational modelling, I greatly appreciate the extra information the authors provided me in their responses and which allowed me to better understand the model, especially the mineralogy part (both in soil and EW material) which is my field of expertise.
In particular I had not quite grasped that the background weathering flux for the 4 locations was not just based on the main mineral of their bedrock (quartz or calcite) but that actual values of soil CEC were also considered as well as a calibration with soil pH. Therefore, my previous worry that the background weathering fluxes might be a lot less realistic than the other model input data of climate and vegetation, turned out to be misplaced.
I am excited to read that the authors appreciate my suggestion for further development of their model to implement multi-mineral EW materials, which are more realistic. Although I understand that modelling EW with a rock dust consisting of 100% Mg-endmember olivine allows for other parameters to vary – and their influence on EW efficiency to be assessed – in the real world there is no 100% olivine rock consisting exclusively of Mg2SiO4 to be found. Some thoughts:

- *Only in case of rather pristine mantle rocks (dunite, peridotite) there might be up to 90-95% olivine with minor pyroxene, Fe-Ti-oxides, … as other mineral phases. For these rock types one could indeed just take into account the olivine fraction of the rock for calcuations.*
- *In case of altered mantle material, that is more commonly found, olivine percentages can be 30-70% and in this case it becomes interesting to also take into account the other main mineral phases that dissolve within relevant time frames.*
- *Finally, a rock such as basalt contains up to ca. 10% of olivine, so the other main mineral phases now really need to be taken into account. It furthermore also consists of volcanic glass, another quickly weathering material (no mineral as no crystal structure) which would be interesting to include in the calculations.*

Response: We thank the Reviewer for appreciating our work and for defining it *better structured, more easily understandable and more complete* after our responses to the very worthwhile comments. We also appreciate the thoughts about the use of multi-mineral EW materials; we will surely take them into account in a future application of the model considering this more realistic condition.

Specific comments:

Referee: *I did not have the time to read through the revised manuscript document in as much detail as I did with the authors' responses document, but when I read through it I observed the following potential typos/missing words (bold is suggested change):*

- *line 20: …such as **Ca-Mg** silicates…*
- *line 27: …can be **found in** igneous rocks… and mostly in… (extracting olivine from rocks is not done except for large, high quality olivine crystals then used as gemstone 'peridote')*
- *line 44: … conditions **whilst still being** a closed and controlled system… (this paragraph I wrote in my initial review was a comment to explain these different EW experiments, not a grammarly complety correct text to include as is in the manuscript)*
- *line 61: … in laboratory **experiments**, such…*
- *line 92: … when required **based** on crop…*
- *line 128: … to be mixed **throughout** the…*
- *line 162: … phenology **and despite** the modest MAP differences… (I think this is what is meant here?)*
- *Figure 3: In the main text and in the figure caption the 'average rainfall depth (α) and frequency (λ)' are mentioned, but the Y-axis in the figure itselfs mentions 'average storm depth (α) and average storm frequency (λ)'*
- *360lines 252-253: … the periods related to… and late **season**, respectively)…*
- *line 257: … **derived** from plant metabolic…*
- *lines 260-270: two different ways of writing the dates, with and without a comma (for July, 6th for corn - for July 6th for corn) are used intermittently within this paragraph*
- *line 312: … of multiple irrigation **events** are…*
- *lines 360-362: … "Apart from some spikes, occurring on some specific days, averaging the grain scale weathering rate ratio, we achieved that the clay loam soil results in a weathering rate about twice as high as what obtained with the silty clay loam soil, at all four locations"…. This sentence is very strange to me, perhaps the word 'achieved' is not correct here, in any case I do not understand the structure of the first part of the sentence. Please rephrase to make more clear.*
- *line 368: … acidifying effect resulting from the displacement of…*
- *line 414: … two Italian sites, Sicily and Padan plain, which…*
- *line 444: … that has the slowest background weathering flux…*
- *lines 451-452: …"more than double the olivine we added in our study (i.e., 22 kgm−2 of dunite corresponding to about 18 kg m−2 of olivine)" Although should it not be 'almost double' the olivine, since this study added 10kg/m² and you calculate that given its olivine contents the dunite represented about 18kg/m²?*
- *line 455: … the leached concentrations of the…*
- *lines 461-462: … A great amount of rainfall, contextually occurring to low transpiration losses, leads… Not sure what is meant here? Contextually occurring together with low transpiration losses? Contextually leading to low transpiration losses? Please rephrase to clarify.*
- *lines 485-488: "Taking into account the case of Iowa, which resulted in the highest carbon sequestration rate and is characterized by a cropland area covered by the corn of about 56,000 km2, the annual average sequestered CO2 could reach the value of about 0.023 Mt y−1, if the whole cropland area were amended with olivine. Sicily, instead, may sequester on average a mass of 0.0002 Mt y−1, if amending the total cropland area cultivated with wheat of about 265,000 ha.'' Maybe it is good to repeat here that these annual sequestration rates are calculated based on a once in 10 year 100ton/ha application of 100% pure Mg2SiO4 – for those readers that did not go through the entire paper but from abstract to final discussion and conclusions. Since there are so many 0s in the sequestered CO2 when expressing it as*

Mton/year, it might be easier on the eye (and to understand the amount) when expressed in kilo tons? Or even as 23,000 tons and 200 tons?

- *line 498: … In effect, **even in** ~~this~~ **our** previous work we obtained about…*
- *line 512: … products (e.g., **NO3 –**), … (superscript for '-')*
- *lines 524-525: Haque et al. (2020) carried out a wollastonite EW experiment on three farms with different plants, located in three **separate sites** in Canada.*
- *line 527: Kelland et al. (2020) (i.e., 2-4 $tCO2ha-1$) … please also mention in what time frame, I presume per year? Then it can become 'greater **annual** carbon sequestration rates (…) **compared** to…'*
- *line 537: Another relevant aspect to consider when planning an EW intervention is **the** economic feasibility **on** itself*
- *line 544: … biochar, leading to **consideration of** EW as a reasonable…*
- *line 547: … sequestration potential **and minimal** related costs…*

Regarding these lasts comments of the need to combine EW potential with minimizing costs for each location specifically, this can be addressed in model calculations by using multi-mineral compositions reflecting real rocks potentially suitable for EW that are found near those locations. For Europe there is a paper of Kremer et al, 2019 (https://www.mdpi.com/2075-163X/9/8/485) identifying natural rocks suitable for EW.

Response: Once again, we appreciate the very in-depth analysis conducted by the Reviewer on our work. We corrected the above-highlighted typos/missing words and rephrased some sentences that were unclear. We also corrected the axis of Figure 3 as suggested.

In particular, the sentence related to lines 360-362 was rewritten as:

Averaging the grain scale weathering rate ratio (i.e., the curves shown in the last row of the figure), the clay loam soil results in a weathering rate about twice as high as what is obtained with the silty clay loam soil, at all four locations.

In the sentence in lines 485-488 we better specified that carbon sequestration rates were *calculated as the average over 10 years of 100% pure Mg_2SiO_4 amendment with a rate of 100 t ha^{-2}*. We also expressed carbon sequestration rates in kilo tons.