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Interactive comment on "Impacts of Enhanced Weathering on biomass production for negative emission technologies and soil hydrology" *by* Wagner de Oliveira Garcia et al.

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Dear reviewer, we have carefully reviewed the comments and revised the manuscript. We thank you for constructive remarks and took care to include all of them in the manuscript. We added a detailed reply to each point as addressed below.

Reviewers comment

Our reply

RC1: Comments 62: How does this older Gaillardet number compare to those more recently produced by co-author Hartmann's papers and Moon et al. (2014)?

I would consider citing all of these sources and giving a range of the CO2 consumption rate by silicate weathering is

In line 67 and 68 we gave the range and considered the suggested authors.

RC1: 71-76: Work by Porder and Hilley, as well as Waldbauer and Chamberlain (2005) should be cited in this paragraph or previously

The work of Porder and Hilley (2011) is cited in line 80, and Waldbauer and Chamberlain (2005) is cited in line 64.

RC1: 97: Have others done this type of analysis or suggested this since von Liebig and Playfair (1843)?

We rephrased the sentence. According to Liebig's Law of the minimum(von Liebig and Playfair, 1843), biomass productivity will be limited by the scarcest nutrient in the soil. This law is widely applied to biological populations and in many ecosystem models. The new sentence is in line 103 to 106.

RC1: 108: What about changes in precipitation and runoff?

The weathering rates of minerals are controlled by the following mechanisms: (i) soil pH (Lasaga et al., 1994;Grathwohl, 2014;Arshad et al., 1972); (ii) redox conditions for Fe and Mn minerals (Hering and Stum, 1990;Gilkes et al., 1973;Cánovas et al., 2019); (iii) soil solution composition (Lasaga et al., 1994;Grathwohl, 2014;Hayes et al., 2020) and soil moisture (Sverdrup et al., 1995;Appelo and Postma, 2005); (iv) temperature (Lasaga et al., 1994;Velbel et al., 1990;Hayes et al., 2020); and (v) grain size/density of exposed structural defects on mineral surfaces (Lasaga et al., 1994;Arshad et al., 1972;Holdren Jr and Speyer, 1985;Cánovas et al., 2019). In this study we did not account for impact of changes in hydrology on weathering because model projections of hydrology in the future are not reliable. i.e. for most parts of the global model predictions it differs from each other despite similar boundary conditions (e.g. IPCC AR5). Model predictions of temperature change, on the other hand, are more reliable and all models agree on the sign (i.e. warming). Goll et al. (2014) used a simple weathering model that accounts for temperature, runoff and soil thickness controls on weathering, and illustrated that global warming was the dominating driver behind changes in weathering using climate change reconstructions from four earth system models. The reconstructions showed large differences with respect to hydrology and changes in global weathering rates to changes in temperature were rather minor. The temperature sensitivity of global weathering rates was significant and rather homogenous among four different climate models. The temperature sensitivity implicitly accounts for changes in hydrology, however it is not able to capture regional differences. Thus we only account for the T changes. This was done earlier in Sun et al. (2017) Earth's Future. Besides the large uncertainty with respect to climate change projections, current data availability is insufficient to calibrate a complex global model, which are currently only applied on site to catchment scale (Pierret et al., 2018;Ackerer et al., 2018;Beaulieu et al., 2016). We argue that a simple model of weathering is appropriate. The used model is very simplistic but the used weathering rates were calibrated on 381 catchments in Japan (Hartmann et al., 2009) and describes the chemical weathering as a function of runoff and lithology, temperature and soil thickness (Hartmann et al., 2014). The model is increasingly used for global scale studies (Sun et al., 2017;Goll et al., 2014;Wang et al., 2018). We included part of this discussion in line 216 to line 222.

RC1: Relatedly, what are the "Parameters" in yellow in Figure 1? It might help to list them on the figure

We rearranged Figure 1 and they are explicitly specified there.

RC1: 169: In addition to Figure 1, a schematic of what is actually being done in the methods own so please specify in parentheticals and provide the median or mean estimate

We rearranged Figure 1, so the used steps are clearer and they refer to the respective chapter sections and used equations.

RC1: 191: Again, what about changes in runoff to weathering rate?

See above.

RC1: What is this 9%/degC equivalent to in an Arrhenius style calculation? While temperature increases make sense (kinetics) much of the world weathers chemostatically (equilibirium, see Winnick and Maher (2018, EPSL) and papers by Godsey for example) so changes in soil infiltration rate (P-ET = runoff) and thus soil moisture should also influence these changes

The Arrhenius corresponding average increase is 6.5%/degC for basalt that had an average activation energy of 50 kJ/mol, 3%/degC for carbonate activation energy of 24 kJ/mol, and 10%/degC for a felsic rock type activation energy of 74 kJ/mol (Fig. 1). Our 9%/degC value is also based on the Arrhenius calculation but represents the average value considering different lithotypes (they had mean apparent activation energies ranging from 50 to 80 kJ/mol and a deviation of $\pm$ 20 kJ/mol, based on field data and laboratory experiments (Goll et al., 2014)). The apparent temperature sensitivity of weathering from Goll et al. (2014) implicitly includes changes in hydrology as it is derived from global model simulations using a model which describes the chemical weathering as a function of runoff and lithology, temperature and soil thickness (Hartmann et al., 2014). As lay down above we omit a more detailed representation of water effects due to high uncertainty with respect to projections of hydrology (IPCC AR5) and as temperature was found to be the dominant driver of changes (Goll et al., 2014).

RC1: 220-233: What about the grain size of the rock powder be applied? Does that matter at all? I assume it would be based on reactive transport simulations such as those by Maher (2010, 2011) for example. And relatedly how fine the rock is powdered will change the texture described in the next section right?

Yes, the grain size matters since it will influence on the exposed reactive surface area (generally fine textured basalt powder would have a high reactive surface area than a coarser textured basalt powder) and consequently the weathering rates. However, in our approach we considered that the grain sizes would range in between 0.6 – 90 μm (for the fine basalt powder) which is enough to completely dissolve the deployed rock powder after one year (Strefler et al., 2018). For the coarse basalt powder 70% of its granulometry fall into the 0.6 – 90 μm range and from the other 30%, at least 20% would be dissolved in one year (Strefler et al., 2018). The finest grain size we can consider is the clay size which comprehends grains >1 μm and <3.9 μm. If a basalt powder would have only clay size granulometry, the effects on soil hydraulic conductivity would decrease in average by 37% for deployment amount of 30 kg basalt m-2 (for the same deployment amount and for the fine rock powder used in our work, the hydraulic conductivity would decrease by 2%). However, the finer the grain gets the higher the energy input for grinding is, which can drastically affect the costs of EW (it can reach up to 500USD/tCO2 sequestered; Strefler et al., 2018). Part of this discussion is included in line 254 to 255. Additionally, we make it clear that the used the grain sizes not higher than 90 μm in line 279 to 283 and discussed about the assumptions of full dissolution in chapter 4.4 in line 562.

RC1: 240-243: Thus, this scenario will be the maximum effect? I think that should be stated explicitly here and elsewhere

Thanks, now it is explicitly stated in lines 283, 285, 393, and 394.

RC1: 248 (equation 10) and text surrounding: Is this the 'pedotransfer function' mentioned in the above paragraph? Is the only empirical values the lambda? Further, where does the 1930 in the equation come from? A lot of this appears to be buried in the supplement, maybe it'd be more helpful if it is more explicitly stated that this is from Saxton and Rawls (2006) upfront in this section

This is one of the used pedotransfer functions. The 1930 is a constant (i.e., a regression coefficient) which is dependent on the database used to derive the pedotransfer equation as described in the work from (Campbell, 1974). Now the pedotransfer equations are present in the main text in subchapter 2.6. No, lambda is estimated according

to equation 18. The empirical values are the organic matter content and the granulometry of soil and rock powder (equations 15-17).

RC1: 332: What about other soil types? Is this information in the literature? What is the most common agricultural soil type?

Considering the extension, Oxisols are the most common agricultural soil type followed by Ultisols. We added the value for Ultisol in line 450 to 453.

RC1: 379: But if they are empirical wouldn't those datasets inherently have clay minerals forming in those systems? In general, I think the caveats brought up in this paragraph should be acknowledged earlier in the paper

The used databases probably contained clay minerals in the clay-sized fraction of the soils. But the Saxton and Rawls (2006) equations accounted only for soil texture and soil organic matter content influences on water retention and hydraulic conductivity. The sentence was misleading and we changed it. You can find the improved sentence in line 523 to 531. What was meant by the old sentence was that once the rock powder will be added into the soil, the primary minerals will be weathered and genesis of clay minerals can occur. The new formed clay minerals will positively contribute to water retention in soil, since clay minerals with high cation exchange capacity were able to significantly influence soil water retention at a matric potential of -33 kPa (Gaiser et al., 2000). We have also included the potential effect of expected increase in soil organic carbon/matter on plant available water. We placed the caveats in the methodology section (lines 328 to 333) and rephrased the old sentence from line 379, which is now in line 523 to 531.

RC1: Specific Line by Line Comments 19,21 and 22: Are these ranges 95pct/2 sigma ranges? Lines 147 and 172 suggests that this might be driven by the minimum and maximum harvest rates. What is the central estimate? Abstract should be able to stand on it's own so please specify in parentheticals and provide the median or mean estimate

Now we explicitly correlate the amount sequestered for each geogenic P supply scenario and AR P demand (high, corresponding to 95th quartile of wood chemistry and low corresponding to 5th quartile of wood chemistry). You can find it in line 20 to 25.

RC1: 33 and 34: Please put "i.e." and "e.g." phrases in parentheticals

We did the changes.

RC1: 59: Suggest authors but "i.e., P, Mg, Ca and K" in the parenthetical before the citation. I also think some more recent work should be cited here and in the next line (60) for controls on atmospheric CO2 over geologic timescales

We did the changes and cited the study from Singh and Schulze (2015) for weathering as the natural source nutrients (line 63) and the study from Yasunari (2020) for the weathering as control on atmospheric CO_2 (line 65)

RC1: 185: Are the databases sources at most 2 by 2 degrees?

No, By common convention, fine spatial resolution data are generally upscaled to fit the coarse spatial resolution framework to minimize distortions of location (Pontius, 2000). Besides that downscaling a coarse data is not suggested since information on finer resolution is missing. We added this information in line 202 to line 203.

RC1: 196: Suggest putting "cf. Wang et al. (2017)..." as a parenthetical

We accepted the suggestion, you can find it in line 211.

RC1: 223: Please add citation and average P content in basalt here

You can find the accepted suggestions in line 258 and 259. Additionally we improved the description on data selection for calculating the descriptive statistics from line 243 to 252.

RC1: 262: "here estimated" is confusing

We rephrased the sentence. Now it is explicitly said which equations were used to

obtain the estimated numbers. You can check it in line 341 to 350.

RC1: 282-283: Is this a citation in the parenthetical?

We corrected the citation. You can find it in line 443.

RC1: 307: "under study"?

We changed the sentence, you can find the changes in line 458 to line 460.

RC1: 387: please fix grammar of this sentence

We rephrased it. You can find the new sentence in chapter 4.4 line 537 to line 539.

RC1: 442: I would suggest citing Uhlig's papers here rather than what I believe from the reference list list is the PhD thesis. The papers are Uhlig et al. (2017, Biogeosciences) and Uhlig and von Blanckenburg (2019, Frontiers in Earth Science)

We added the suggested citations. You can find them in line 610.

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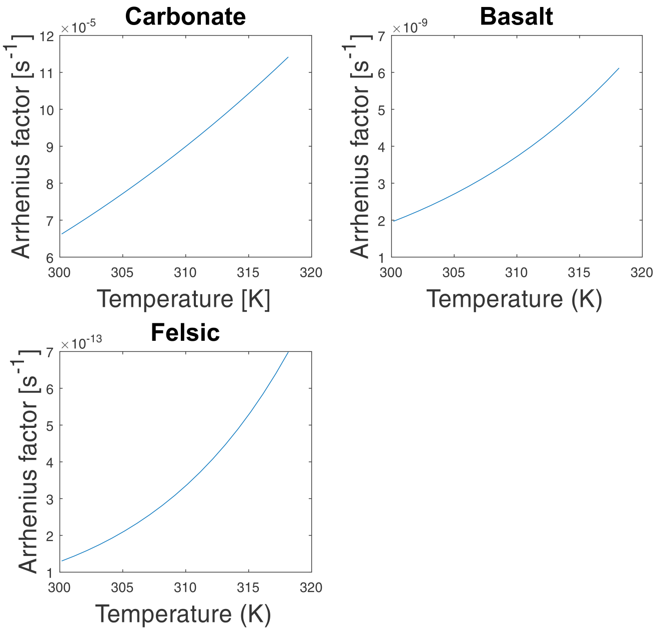

Figure 1: Increases in weathering rates for different temperatures (T), in Kelvin, estimated according to Arrhenius equation ($Arrhenius\ factor = e^{-Ae/(R*T)}$) for a gas constant R of 0.00831 kJ $K^{-1}mol^{-1}$. Average of 6.5%/°C increase for basalt that had an average activation energy (Ae) of 50 kJ/mol, average increase of ~3%/°C for carbonate activation energy of 24 kJ/mol, and average increase of ~10%/°C for a felsic rock type activation energy of 74 kJ/mol.

Fig. 1.