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Interactive comment on "Decoupling silicate weathering from primary productivity – how ecosystems regulate nutrient uptake along a climate and vegetation gradient" *by* Ralf A. Oeser and Friedhelm von Blanckenburg

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REPLY We thank the reviewer for acknowledging the value of this study value in deciphering the complex interactions between plants and silicate weathering. We appreciate these constructive comments that point at possible improvements in the data presentation and the discussion that can be dealt with as suggested.
We reply point by point to the reviewer's comments.

COMMENT L11: "..these two drivers..". It is unclear what is meant – is it bio-

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genic vs. abiogenic?
REPLY Yes, we mean the relative impact of biogenic vs. abiogenic weathering. We will clarify this accordingly.

COMMENT L20: Ecohydrological controls of partitioning of water between drainage and evapo- transpiration may explain some of this discrepancy
REPLY We discuss this partitioning in Line 528ff. According to Ibarra et al. (2019) total runoff can decrease by up to 23% as vegetation cover raises from barely to highly vegetated sites. However, we find that this reduction is a minor effect when compared to the 100-fold increase in precipitation over the entire EarthShape gradient.

COMMENT L25: Taylor 2009 gives a good review of biotic impacts on weathering
REPLY Thank you for directing us at this useful review. We already discuss many of the mechanisms of biotic weathering in discussion section "is weathering modulated by biota?" (Line 497ff). However, Table 1 presented in this paper will serve as a useful resource that we will cite to summarize previous field studies done to explore these interactions.

COMMENT L27: "weatherability" should be defined, as it may mean different things in different contexts.
REPLY With "weatherability" we refer to the susceptibility of minerals to weathering, i.e. dissolution. We will clarify this in a future version of the text accordingly.

COMMENT L36: plants possibly affect a negative feedback that is also there without land plants. Otherwise the silicate weathering thermostat would not have worked prior to the colonization of land by plants
REPLY We will rewrite this statement to emphasize that plants have strengthened the negative feedback that already existed by abiogenic weathering.

COMMENT L125: Santa Gracia is affected by livestock grasing, which would add to nutrient export. This should be considered in the discussion later.
REPLY Grazing would indeed lead to increased nutrient export. We just wonder whether the more recent advent of grazing has already contributed to weathering given that the timescale over which the bio-available fraction resides is a few centuries –longer than grazing (see turnover times, Table 8). Nonetheless, we will add this point in the discussion.

COMMENT L129: Eco-systems are primarily N-limited. What does this imply for P-weathering and P nutrient supply? Should be added in the discussion.
REPLY It is a common observation that the study sites are primarily N-limited (Stock et al., 2019). With increasing NPP along the gradient, however, a N-P co-limitation on plant growth might develop, because Stock et al. (2019) found an increased activity of P-acquiring enzymes in the mediterranean and humid-temperate site compared to the (semi-) arid sites.

COMMENT L144: The sentence starting "They are thus towards the lower end of global cosmogenic nuclide-derived soil production rates. . ." should be clarified. Do you mean overall for all 4 locations?
REPLY We mean all four locations. Global cosmogenic nuclide-derived soil production rates are up to 20-fold higher than those reported for La Campana (see e.g. compilation by Dixon and von Blanckenburg, 2012)

COMMENT L273: "kinetically limited weathering regime" is an interpretation and should be included in the discussion. Have you considered that it may be "thermodynamically limited" (Winnick and Maher 2018)?
REPLY "Kinetically limited weathering regime", meaning that here are primary minerals left in regolith because erosion is sufficiently high such that weatherable minerals still

remain and the weathering rate is limited by mineral dissolution kinetics (e.g. Dixon et al., 2012), is in our opinion a factual observation, and hence we think it is not a discussion item. We consider the "Thermodynamic limited" weathering regime to be a subset of the kinetic limit, namely the balance between dissolving primary minerals and precipitating secondary phases at a metastable equilibrium. In the absence of concentration-discharge data we have no means to investigate the chemostatic behavior that would result from the thermodynamic limit. This is why we do not discuss this topic.

COMMENT L283: Probably not all nutrients are available to ecosystems, as some leave soils in dissolved form. The statement in line 610f should be included in some form in the main text.
REPLY We agree with this comment and will emphasize that the parameterized weathering fluxes are upper estimates of potential nutrient uptake.

COMMENT L298: This paragraph is hard to read. Stick to describing the trends and exclude the numbers from the text. P and K being the most important nutrients should not be called an exception to a trend. It would clarify the message overall to focus on the most important nutrients and leave the evaluation of the other elements to the appendix.
REPLY We can follow this suggestion, but we note that many readers wish to see at least some data in the results section.

COMMENT L325: In my opinion the Sr ratios mentioned here are not distinct.
REPLY We will rephrase the sentence accordingly.

COMMENT L392: Why does increasing P concentration along the gradient hint at P limitation? Where is P limiting? And what is the impact of livestock on the P budget in Santa Gracia?

REPLY Please see our comment to L129. As suggested by reviewers #1 and #2, we will remove the section "Are nutrient sources setting plant stoichiometry?" from the discussion. This section is not essential for our analysis and we can remove this without loss in any of the manuscript's conclusions.

COMMENT L446-460: In Oeser et al 2018 it was concluded that the weathering is not limited by mineral supply. This does not necessarily imply kinetically limited weathering. Equilibrium with regolith fluid characterizes a thermodynamic limit (Winnick and Maher 2018). That being said, the Nahuelbuta site could be in a kinetically limited weathering regime. It would improve the manuscript to clarify this and what role plants may play in different weathering regimes.
REPLY Is it not valid to assume that in these sites primary mineral dissolution is limited by the kinetics of mineral dissolution reactions (Dixon et al. 2012)? We consider the "Thermodynamic limited" weathering regime to be a subset of the kinetic limit, namely the balance between dissolving primary minerals and precipitating secondary phases at a metastable equilibrium. In the absence of concentration-discharge data we have no means to investigate the chemostatic behavior that would result from the thermodynamic limit. This is why we do not discuss this topic. However, to do justice to this comment we can reword "kinetically limited" into "supply limited".

COMMENT L581: I would revise the end statement. It is a leap to upscale from a local/regional spatial study to the global temporal cycle. Plants are not the driver of the global silicate- weathering-carbonate cycle, only a modifier in as much as they affect the atmospheric CO2 level at which the silicate weathering CO2 sink balances CO2 sources. Therefore, biotic enhancement of weathering at the global scale does not increase silicate weathering rates (in steady state).
REPLY It is good to point out that plants cannot produce a weathering flux that exceeds CO2 supply by volcanic emissions. However, because of the many reactions that plants and subsoil microbiota induce they have been suggested to make the delivery

of alkalinity into the oceans more efficient. Thus biogenic weathering would impact climate by setting lower atmospheric CO2 levels and are thus thought potentially drive global cooling (Berner et al., 2003; Pagani et al., 2009). We can clarify this point, but we wish to emphasize that the ability to recycle elements damps this response.

COMMENT L618: GrowthRate should be defined
REPLY Here we use NPP and Growth rate synonymously. This is indeed misleading, and we will use NPP instead.

COMMENT Figure 2: The left panel does not correspond to the text. Is litter layer and biota one box called plants?
REPLY We will rectify this in a future version of the figure and its caption.

COMMENT Table 2: Eq(4) Does this assume no recycling internally in the plant? Eq(6) is the notation tau$_x$ correct here?
REPLY Eq (4):Our parameterization of the nutrient uptake rate is independent on internal nutrient recycling. Eq (6) Indeed, it should be τ^X. We will correct this accordingly.

References

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