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Biogeosciences Discussions

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 taking the time to provide us with his detailed critique of our manuscript. The reviewer has raised important points that lead us to think even harder on how to tackle the formidable challenge of unraveling the links between geochemical and biological processes in the Critical Zone, with the confounding factors that no doubt make the resolution of trends caused by an individual process so difficult. This objective lies at the heart of the German-Chilean "EarthShape" project (Earth Surface shaping by biota), and the aim of this project is exploring these interactions over

the most extreme climate and biological gradient on Earth. If we are not able to resolve these interactions, there we might never be able to do so – anywhere. Towards this aim we supply here an extensive dataset of elemental and isotopic weathering zone, soil, and plant compositions at four Critical Zones along this gradient. And we employ several (in part novel) metrics to quantify the geological and biological fluxes. Our aim is to use these quantitative metrics for hypothesis testing: namely to resolve the interactions between ecosystem productivity and silicate weathering. We reply point by point to the reviewer's comments.

COMMENT This paper is ambitious in scope and attempts to tease apart the effect of plants on weathering across a well-studied climate/productivity gradient in the Andes. I applaud the attempt. . .
REPLY We are grateful for this acknowledgement. Indeed, we have introduced and suggested a whole set of in part novel geochemical and ecological methods and metrics that we deem necessary to tackle this problem.

COMMENT . . .but am not convinced by the conclusion that plants have little effect on weathering. In the end, despite the ambition, there are so many confounding variables between these four sites that I think site to site variation makes larger conclusions impossible. Four sites, with so much variation both within and among them, are not likely to be sufficient to see the signal through the noise.
REPLY: We appreciate that the reviewer raises the point of the confounding variables. We are fully aware of this issue, in particular with respect to the role of denudation rate. For this very reason for addressing the core question, namely whether plants accelerate weathering, we have limited our comparison to the two sites where all things (except precipitation and biomass growth rate) are similar: Semi-arid Santa Gracia and humid-temperate Nahuelbuta. It is the comparison between these two sites that leads us to conclude that plants do not accelerate weathering. In a revised version we can emphasize that we pursue this strategy to minimize the effect of confounding variables.

COMMENT For example: 1) An alternative to the idea that plants retard weathering is that some of these soils are in the same “process domain” as defined by Vitousek and Chadwick (2013).

REPLY: The “Process Domain” and “Pedogenic Threshold” are indeed interesting concepts that were developed for the chronosequence in Hawaii – sites of hugely differing age but featuring stability of their surfaces. In contrast, here we are dealing with sites that have no discrete age as they are permanently eroding. It is not obvious whether and how the threshold concept applies to these entirely different boundary conditions, and even if so, whether the application of these concepts adds to the quantification of linked geochemical and biogenic cycling, which is the objective of this paper. We get back to this point below.

COMMENT: Given relatively short residence times, . . .

REPLY: We do not understand what the reviewer means with “short”. In contrast to My old soils in Hawaii our soil tunrover times can be seen as short. Assuming the cosmic ray attenuation depth of 600 mm and the measured soil denudation rates of 4 to 27 mm/ky we get cosmogenic nuclide integration times (that can be interpreted as soil residence times) of 20 – 140 ky. Compared to many soil denudation rates from eroding landscapes most of our profiles are in fact at the “long” end of residence times.

COMMENT . . . and relatively dry conditions (even at the wetter site), this is not surprising.

REPLY: Again, we do not understand why the wetter site is considered to be “dry”. With 1100 mm/yr Nahuelbuta is way above terrestrial mean global annual precipitation of 700 mm/yr. It is a “humid” site.

COMMENT For example, sites at these rainfalls do not differ after 10000 years of soil development on a Hawaiian lava flow, and barely differ after 150,000 years.

REPLY: The detailed soil and weathering zone characterization of the EarthShape study sites show that these sites indeed differ in many, if not most properties (see below), and possibly transect a range of thresholds. Yet even though the idea sounds intriguing, we do not know how a strategy to apply the Hawaii concept is to the granitoid

rocks of the Chilean coastal range would look like, and how this would aid to answer the biogenic weathering question.

COMMENT That does not necessarily mean that plants have no effect on weathering.

REPLY: As stated above: we never say this.

COMMENT: Between the two wetter sites, plant cover as a percent doesn’t differ, but NPP differs by a factor of 2 s, catchment denudation rates by a factor of 10, and soil denudation by a factor of 3.

REPLY: The soil denudation rates are the relevant ones because this is where we study the ecosystems and weathering (in particular in La Campana catchment-wide rates that the reviewer cites exceed soil rates by landsliding due to the steep slopes (van Dongen et al., 2019)). Mean soil production rates only differ by a factor of 2 between the two wetter sites.

COMMENT Yet the CDF is much higher at the drier of these two sites. One way to interpret this is that there is no effect of plants on weathering. Another is that there are so many differences between these sites that it would be hard to see the effects of plants, especially as these are both relatively dry sites, and weathering under dry conditions takes a long time.

REPLY: While the reviewer is right to say that when comparing these two sites it is hard to see the effect of plants in light of these differences, we disagree that these sites are relatively dry. There is a large body of literature stating that for most solutes weathering rate scales with fluid flow, and thus it is difficult to understand why the CDF should be higher at the dryer site. However, because of the confounding effect of the high denudation rates at the second wettest site (La Campana) we based our interpretation on the comparison between to the two sites where all things (except precipitation and biomass growth rate) are similar: Semi-arid Santa Gracia and humid-temperate Nahuelbuta.

L45 – Porder et al 2007 evaluated mass loss and dust inputs on a climate x time matrix.

I think it's relevant to cite here.
REPLY: We have made extensive use of the papers by Porder, Chadwick, and colleagues that have shaped our thinking. We believe that the suggested reference does not add anything substantially different in addition to the three Porder et al papers we cite already.

L59 – I'm not sure I follow the logic here. Nutrient recycling makes plants less dependent on inputs of nutrients via weathering, but it doesn't necessarily mean that plants don't drive weathering anyway. For example, as organic matter accumulates in soil over time this can help drive down pH. Plants may increasingly rely on the organic matter for nutrients, but the lower pH may drive increased weathering none the less. A classic example of biology driving weathering quasi independently of nutrient uptake is the role of nitrification (which provides nutrients to plants) in driving soil acidification via nitrate leaching.
REPLY: We do never state that plants do not drive weathering. In fact, in the manuscript's first paragraph and lines 503-510 we explain in detail the mechanisms through which plants do drive weathering. In the line in question we state that the dependence of plant growth on weathering would be non-linear as some fraction of the nutrients needed to fulfill the plants' physiological needs will stem from recycling rather than "fresh" nutrients derived from weathering of primary minerals. This concept is essential to the entire manuscript, and when revising we will make sure that this point is not missed.

L92 – river sand or soil profile cosmogenic 10Be?
REPLY: Schaller et al. (2018) determined weathering rates using soil profile cosmogenic ^{10}Be. We will clarify this in a future version of the text accordingly.

L105 – It is worth thinking about these results in the context of the "pedogenic threshold" model of Vitousek and Chadwick. It strikes me that all of these sites may be in a pretty similar "process domain" and that given the mean residence time of the soils one might not expect big differences in the amount of observed weathering if the soils

are relatively well buffered.
REPLY: As stated above we are concerned that the "pedogenic threshold" model may not be transferable in a straightforward manner from the Hawaiian chronosequences into the eroding Chilean Coastal Range. However, in a detailed pedogenic description we see that many (and more) of the properties used by Vitousek and Chadwick to define their pedogenic thresholds do indeed vary in Chile, and this work was cited (Bernhard et al. (2018). Moreover, it is found that, unlike in Hawaii, these relationships in the soils of the EarthShape study sites vary in a non-linear relationship. The most prominent thresholds were found between the arid Pan de Azúcar and the semi-arid Santa Gracia and was attributed to MAP exceeding potential evapotranspiration (see also Slessarev et al., 2016). However, a threshold for base saturation was found to exist between the mediterranean site (La Campana) and the humid-temperate site (Nahuelbuta). Thus, different thresholds exist for different soil properties which do not feature at identical positions along the climate gradient. It is very obvious that our sites are not within the same process domain.

L137 – It seems odd to state that erosion rates are similar between these sites when they vary by more than an order of magnitude. That seems a potential cofounding factor. It doesn't vary directly with precipitation, which is nice, but it will set up differences in soil residence time that could confound the results here (since climate by time interactions are common, see Porder et al 2007 as an example).
REPLY: Catchment-wide denudation rates indeed vary by almost an order of magnitude and we do not conceal that information. On the soil pit scale (which count for this study), mean denudation rates vary from 10 to 40 t km^{-2} yr^{-1}, and individual rates from 8 to 70 t km^{-2} yr^{-1}. In the detailed parts of the discussion, we focus on the comparison between Santa Gracia and Nahuelbuta. In these two sites, soil residence times are similar (24 $\pm$ 1 and 28 $\pm$ 2 kyr in Santa Gracia vs. 22 $\pm$ 1 kyr in Nahuelbuta; Schaller et al., 2018). Yet our general conclusions on the impacts of plants on weathering also agree with the findings at the arid (Pan de Azúcar) and mediterranean study site (La Campana) even though these are subject to either atmospheric deposition of e.g. Ca

and Sr or increased denudation rates because of very steep hill slopes, respectively.

L165 – The "gently sloping hills" at Nahuelbuta would lead to longer soil residence times and thus more weathered soils. Again, I am skeptical of the "control" over erosion rates and residence times in this set up. Especially because the depth of the weathering zone is not known.
REPLY: We agree that gently sloping hills would lead to longer soil residence times and thus more weathered soils. However, the soil residence times in Nahuelbuta are shorter than in Santa Gracia despite lower slope angles (these are the two sites we base our principle comparison on).

L180 – Not sampling roots will lead to an underestimation of both the plant pool and of NPP. In addition, some grasses and desert woody plants have an extremely high fraction of biomass below ground, so not sampling belowground will lead to bias (not just underestimates). Since there is very little detail on vegetation sampling, it is hard to evaluate how much a problem this is, but it could be substantial. In addition, the stoichiometry of NPP is not just NPP x chemistry, since woody plants and perennials in general may have a bulk chemistry that is very different form the chemistry of leaves that are forming and falling more frequently. Much more description of the vegetation and the assumptions about pools and fluxes is needed in order to evaluate this part of the paper.
REPLY: Indeed, we acknowledge that the description of vegetation sampling should have been more detailed, and we will amend this deficit. We are also aware of the fact that the estimation of plants' representative chemical composition is an estimate, and that this estimate includes assumptions. This is due to the fact that a) preciously little information exists on belowground biomass, on its mass per se but even more so on its stoichiometry; b) it is very hard and mostly close to impossible to sample whole plants including roots, in particular when it comes to large trees. Nevertheless, to begin somewhere we have attempted to do whole-plant budgets, and this is how we have done it: Vegetation samples have been taken in the austral summer to autumn

2016 and was restricted to mature higher plants in the study sites (e.g. grasses have been excluded from sampling). From each sampled plant (n=20), multiple samples of leaves, twigs and stem have been taken and were pooled together and homogenized prior analysis. Our estimation of the plants' chemical composition invokes several assumptions: (1) Roots' biomass growth attribute only little to total plant growth, namely 9% in angiosperms and 17% in gymnosperms (Niklas and Enquist, 2002). We thus treat roots and stem/twig as one plant compartment and allocate 68% and 52% in angiosperms and gymnosperms, respectively, of relative growth to these compartments. (2) Differences do only occur between angiosperms and gymnosperms. (3) The pattern of relative growth and standing biomass allocation holds true across a minimum of eight orders of magnitude of species size (Niklas and Enquist, 2002). Thus, we assume that the growth rates of plant organs do not vary considerably between different plant species. We are aware that these assumptions result in only a rough estimation on plants' chemical composition. NPP was derived from a dynamic vegetation model simulating the vegetation cover and composition during the Holocene (Werner et al., 2018). It is thus independent on our sampling strategy.

L199 – Drying vegetation at 120C will lead to a substantial loss of carbon and nitrogen. Loss of P and cations will be smaller. Were plant standards dried at this temperature to ensure that this high temperature did not influence the results? It's hard to tell when the NIST standards were included in the process.
REPLY: We did not analyze C and N. We did dry plant samples at this high temperature to ensure that any H_2O will disappear. Unfortunately, the SRM 1515 has not been dried at 120°.

L235 – Depending on the age of the parent material and the mineralogy, using Sr isotopes from granitic rocks as a tracer through plants can be problematic. This occurs particularly if there are high amounts of Kspar (which likely varies from site to site here and will be particularly sensitive to the occurrence of "metamorphic basement"(L170) at the Nahuelbuta site. See early work by Tom Bullen for a more complete description

of the problem.
REPLY: Bullen et al. (1997) did investigate the behavior of Sr along a chronosequence and found that ^{87}Sr/^{86}Sr extracted using ammonium-acetate decreases from a value representing K-feldspar to those of plagioclase and hornblende with increasing soil age, suggesting that the exchangeable pool is dominated by Sr which is leached from K-feldspar after deposition. In contrast, the abundance of K-feldspar in the EarthShape bedrocks is similar (Oeser et al., 2018) and based on the calculation of τ-values (i.e. K, Si, Na), no preferential or early dissolution of K-feldspar is recorded. We would further like to stress differences in the setup between our study and the study by Bullen et al. (1997). Their study is based on the concept of a chronosequence whereas our study follows the climosequence approach. Bullen et al. (1997) derived their conclusions based on soils of varying age whereas in the EarthShape sites, the soil residence times i.e. the average time a mineral grain remains in the mobile layer are broadly similar and range from 11 $\pm$ 1 to 30 $\pm$ 2 kyr (Schaller et al., 2018).

L275 – I find the lack of replication within site really troubling, especially given how sensitive CDF can be to variations in Zr (as the authors note). I appreciate that the authors used Monte Carlo to get at uncertainty, but there seem to be so few samples that I worry this will underestimate the uncertainty nonetheless. Another concern is that (in Appendix A) it seems many samples were excluded from the parent material if they had different chemistry (e.g. pegmatite, mafics). However those samples must contribute to the soil. Including them would make for much bigger error bars on CDF (I think) and thus make consideration about differences (or lack of differences) between sites all that much harder to justify. As for the potassium issue discussed here, couldn't the concentration of K be increased by a combination of plant uplift (e.g. Jobbagy and Jackson, 2004) and soil collapse (which is why you correct by Zr to get tau)? Overall, these uncertainties are very understandable, given heterogenous bedrock etc. But that speaks to the need for way more sampling in order to constrain that heterogeneity.
REPLY: The reviewer is very wrong with the statement that there is a "lack of within-site replication". At each site two regolith profiles situated on opposing slopes have

been studied to account for variations in substrate and/or effects of insolation and microclimate on weathering and nutrient uptake by plants. These two regolith profiles at each site are replicates. In fact, in the site description paper that this study is based on (Oeser et al. 2018, cited) we have measured four profiles at each site. Here, due to the extremely time-consuming extraction and isotope work, we have limited these replicates to two per site. Only for synthesis we present the elemental fluxes in terms of study site averages. Maybe the reviewer read these to infer lack of replications. We will clarify that have in fact made extensive replication. The reviewer rightly scrutinizes the exclusion of certain samples. However, the exclusion of regolith samples only involves samples from one of eight profiles: The S-facing regolith profile in Nahuelbuta. This exclusion does not influence the estimate of $W^{X}_{regolith}$. The excluded samples are exclusively situated within the saprolite and we parameterize $W^{X}_{regolith}$ using the most negative tau-value of the lowermost mineral-soil sample. $W^{X}_{regolith}$ thus integrates over the entire mass loss occurring in regolith. Note that these samples were not excluded in the determination of the inventories in saprolite and soil as their solutes released from weathering indeed contribute to the bio-available fraction. In terms of CDF, the exclusion of these samples from the Nahuelbuta S-facing profile remove samples with highly negative CDF only. Negative CDF cannot be explained with element loss through weathering in regolith with a single parent material. Instead, regolith chemical composition might reflect mixing of multiple parent materials with distinct rates of soil production each. However, we are not able to disentangle these rates. In the previous study (Oeser et al., 2018), four regolith profiles at each site were used to determine study-site representative values for CDF and no regolith samples have been excluded. The results of both studies show consistent results. Oeser et al. (2018) determined the volume loss or gain in the four sites using the volumetric strain ϵ (Brimhall and Dietrich, 1987). Dilatation (or soil collapse?) was only found in the A and B horizon in Santa Gracia. In the other sites, saprolite and soil was characterized by volume expansion.

L275 – The idea of "kinetically limited weathering" seems more an interpretation than

a result. Thus it seems more appropriate for the discussion.
REPLY: There are primary minerals left in the soil, thus erosion is sufficiently high such that weatherable minerals still exist in soil and the weathering rate is limited by mineral dissolution kinetics, based on the concepts of numerous publications (e.g. Dixon et al., 2012). In our opinion this is a factual observation, hence it is no discussion item.

L284 – If weathering is deep below the rooting zone weathering from rock does not necessarily mean availability from plants.
REPLY: Weathering occurs throughout the entire regolith and different weathering fronts do exist. They depend on the minerals' dissolution kinetics. We did refer to "most plant-essential rock-derived mineral nutrients" to emphasize the geologic origin. We will address this concern and rephrase the sentence accordingly.

L290 – Equation three is a good example of why I think there needs to be a much more rigorous treatment of uncertainty. D, X parent and tau all have uncertainties associated with them, but that does not seem to be considered when thinking about the differences between sites. The data are presented without any estimate of uncertainty, and thus it is impossible to tell whether there are any statistically significant differences between sites.
REPLY: The calculation of $W^{X}_{regolith}$ did involve a rigorous error propagation and the uncertainties on the weathering fluxes were estimated by Monte Carlo simulations. For this calculation we did use 1SD of the respective profiles Denudation rate, the 1SD of the bedrocks' element concentration of interest, and 3% relative uncertainty on the element concentration in regolith samples (see Table 3). We will address this concern and include the standard deviation in the text such that the reader can more easily decide whether a statistical difference exist or not.

L320 – It is true in all ecosystems that uptake of nutrients is fed mostly by recycling and very little by the weathering flux. That is true even if 100% of the nutrients were originally supplied by weathering.
REPLY: But over geological time scales the losses occurring through erosion and as

solutes need to be balanced by supply from weathering (Uhlig and von Blanckenburg, 2019), otherwise ecosystems would run into depletion.

L325 – First, how is a range of 0.723-0.737 "distinct" from 0.726 which falls in that range? Second, given incongruent weathering, why would one expect the bulk bedrock value to match the regolith value?
REPLY: Indeed, the wording of this sentence is misleading, and we will rephrase the sentence accordingly. In Pan de Azúcar, the degree of weathering is very low, mainly attributed to physical disintegration of rock. Given the low losses through weathering, we would assume that bedrock and regolith are identical in their $^{87}Sr/^{86}Sr$.

L338 – Here, incongruent weathering is postulated. What not anywhere else?
REPLY: The regolith profiles in La Campana were the only profiles where we were able to correlate changes in $^{87}Sr/^{86}Sr$ to losses of certain elements by using τ^{Sr}, τ^{Ca}, and τ^{K}. The other regolith profiles did not permit to resolve weathering-related trends in $^{87}Sr/^{86}Sr$ or incongruent weathering.

L349 – Perhaps due to very few samples, and soils integrating lots of different minerals plus atmospheric inputs?
REPLY: We do not understand why the reviewer places so much emphasis on the "low" number of samples. In total we did sample 13 different plant species comprising 20 different specimens. Each leaf sample for example integrates over several leaves which have been homogenized prior dissolution. In terms of interpretation, it seems very unlikely that we did sample just by coincidence the plants with the same $^{87}Sr/^{86}Sr$ ratio than the bio-available fraction they grow on. It is rather the proof that plants take up nutrients from the bio-available fraction.

L364 – Al is often toxic to plants so I'm not sure I would call it "plant beneficial".
REPLY: Plant-beneficial elements are those which compensate for the toxic effects of other elements or substitute for elements and cover some of their less-specific functions (e.g. maintaining osmotic pressure; Al, Na, Si). However, whether an element is

essential or beneficial to plants is species dependent (Marschner, 1993). According to Liang et al. (2007), the effects of Al-toxicity can be mediated by Si.

L365 – What does it mean to be "mostly N limited"? And why do you consider other elements to be "co-limiting"? These seem two key points for the following text, and should be explained more clearly so the reader can follow the argument.
REPLY: It is an observation by soil ecologists that the EarthShape sites are first and primarily N-limited (Stock et al., 2019). However, the role of additional nutrients on NPP is increasingly recognized- subsumed under the term "co-limitation". We will add explanatory text.

L385 – I don't understand why you say the system is N limited (by which I presume you mean NPP is N limited), and then compare other elements to P?
REPLY: For N-limitation, see previous comment. However, this paper is about the mineral nutrients of which P is the most likely element to limit NPP unless continuously supplied by weathering (over the timescale of the "geogenic pathway"). This is why we normalize to P, in a "Redfield Ratio" sense.

L389 – I'm not sure I agree with this interpretation, since the available nutrients are coming out of recycled organic material.
REPLY: We fully agree with the reviewer's statement. In fact, we make exactly this point in this paragraphs' point (4) and in section 5.3. In the first instance, we find that a first-order stoichiometry is set by the geogenic source (our point 1.). We then indeed find evidence that with increasing recycling efficiency (increases from Pan de Azúcar towards Nahuelbuta), the nutrient pools in the soil bio-available fraction are increasingly dominated by the pool of recycled (our point 4). These two findings lead to key concepts of this paper.

L395 – You might have a look at Ben Turner's recent (2018) Nature paper, where they show relatively constant production across a very strong soil P gradient. Production is maintained by species turnover. Not all plants need the same amount of P (or other

nutrients) to maintain the same NPP.
REPLY: Turner et al. (2018) did focus on a multitude of different plant species, their P concentration in leaf and in soil. This, however, is well beyond the scope of our study. The reviewer might be right however in as much as possible nutrient limitations on NPP are buffered by the entire ecosystem community by a shift in species community composition (i.e. species turnover).

L475 – I'm surprised by this interpretation. There are probably more (in amount) atmospheric inputs at the wetter sites, but the relative balance between rock and atmospheric fluxes is a different thing (could be 50/50 at all sites, but still have much higher fluxes at one site than another). This point comes back to the uncertainty in the weathering fluxes, which themselves depend on three highly variable numbers: D, [Bedrock], and tau.
REPLY: We do not understand this comment. We do not present absolute values on atmospheric fluxes. We rather estimate the relative (!) contribution based on radiogenic Sr isotope ratios. In Pan de Azúcar, the weathering release fluxes are very low but a constant supply of moisture (and aerosols) through the Atacama Desert fog (Camanchaca) is prevailing. Thus, our estimate of roughly 90% seems reasonable. Regarding the other sites their relative contribution to ecosystem nutrition is minor and negligible.

L525 – If you do not know the depth of the weathering front how you can tell the total amount of weathering, or assert that the total does not differ between sites? **REPLY:** Because the metric $W^X_{regolith}$ integrates over entire loss occurring in regolith, as the lowest τ^X is (by definition) the depth-integrated weathering loss. Measuring weathering flux with cosmogenic nuclides and chemical depletion does not require knowing the depth of the weathering front.

L572 – I completely agree that NPP is maintained by recycling across your sites and indeed across all ecosystems. That does not mean that over long timescales the weathering flux is unimportant.
REPLY: This is EXACTLY what we say. The "geogenic pathway" does the job over the

long-term.

L574 – If the “geologic pathway” stays constant, one possible reason for that is that the soil residence time is very short for all these sites. The tau and CDF values you present are all pretty low relative to highly weathered soils. This doesn’t mean that plants are accelerating weathering, it simply means that the crank is turning over more quickly. This comes back to the total denudation rates at the specific sites where the soil pits were dug.
REPLY: We failed to understand what the reviewer wishes to say. However, we reiterate that soil residence times are not what we consider to be low. Also, we do not find direct evidence that plants accelerate weathering along the gradient, and do not say they do so. However, plants likely do weathering work. Our point is that weathering isn’t proportional to NPP.

L578 – I do not think it is appropriate to speculated on what nutrient might be in line to be “next” for limitation. This is not really how ecosystems work, and the high level of species turnover among these sites make stoichiometric interpretations such as these even more speculative.
REPLY: Agree this may be a simple geochemists’ view on how processes in ecosystems work. We can tune this statement down, or eliminate it. But we would be curious to hear how one can explain our evidence for deep K uptake in La Campana and the higher K concentrations in plants relative to P when compared to the source (Figure 6).

L580 – I really don’t see this conclusion as supported by the data.
REPLY: This point is indeed speculative. It results from the discussion (line 547ff on soil CO_2 and Si solubility) and explains the lower weathering rate in the wettest and highest NPP site.

L653 – Clarify if this is increasing towards the top or bottom. Also, tau values are negative, so increasing tau usually means less weathered. Some clarification in the text would help avoid confusion.

REPLY: The τ-values do increase towards the profiles’ top, thus indicating elemental gain.

Figure 1 – It would be great to see error bars on these plots.
REPLY: We decided not to plot the error bars in this figure to maintain a clear layout. However, this can be done.

Figure 5 – It would be helpful to have the atmospheric input Sr value on these graphs as well. That way we could see what fraction of the Sr flux is coming from rock vs atmosphere.
REPLY: We will add the atmospheric input Sr value on these graphs.

Figure A2 – I am not aware of a method that uses NH4OAc to extract “bioavailable” P.
REPLY: P-accessibility in the bio-available fraction has been determined by Brucker and Spohn (2019) using a modified Hedley sequential P fractionation method. We will correct the figure’s caption accordingly.

Table 3 – Is D the catchment wide rate or the average of the two soil profiles at the site? Seems like the latter but it would be helpful to clarify.
REPLY: D is the average of the two soil profiles at the site. We will correct the table’s caption accordingly.

Table 4 – If you don’t include the whole weathering zone how can you know how much weathering is occurring?
REPLY: In this table we report on the size of inventories, not about the degree of weathering or the weathering rate. We did decide to scale the extent of the saprolite and the regolith inventory to 1.0m for purposes of comparisons. Once the actual extent of the weathering zone is known, one can extrapolate the size of the inventories to that depth.

Table 5 – Why would grasses and trees have the same leaf:stem biomass (5:95)?
REPLY: Please see our response to line your comment on L180: According to Niklas

and Enquist (2002), the pattern of relative growth and standing biomass allocation holds true across a minimum of eight orders of magnitude of species size.

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