Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-69-RC1, 2020

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

Anonymous Referee #1

Received and published: 4 April 2020

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, 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. 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). Given relatively short residence times, and relatively dry conditions (even at the wetter site), this is not surprising. 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. That does not necessarily mean that plants have no effect on weathering. 2) 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. 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.

I also provide some line by line comments below:

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.

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.

L92 – river sand or soil profile cosmogenic 10Be?

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.

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).

L145 – I agree these don't vary as much as erosion rates around the world, but I'm not sure that's the bar by which to judge whether these are "similar" sites.

L155 – "south" not "soul"

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.

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.

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.

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.

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.

L275 – The idea of “kinetically limited weathering” seems more an interpretation than a result. Thus it seems more appropriate for the discussion.

L284 – If weathering is deep below the rooting zone weathering from rock does not necessarily mean availability from plants.

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.

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.

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?

L338 – Here, incongruent weathering is postulated. What not anywhere else?

L349 – Perhaps due to very few samples, and soils integrating lots of different minerals plus atmospheric inputs?

L364 – Al is often toxic to plants so I'm not sure I would call it "plant beneficial".

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.

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?

L389 – I'm not sure I agree with this interpretation, since the available nutrients are coming out of recycled organic material.

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.

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.

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?

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.

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.

L578 – I do no 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.

L580 – I really don't see this conclusion as supported by the data.

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.

Figure 1 – It would be great to see error bars on these plots.

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.

Figure A2 – I am not aware of a method that uses NH_4OAc to extract “bioavailable” P.

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.

Table 4 – If you don’t include the whole weathering zone how can you know how much weathering is occurring?

Table 5 – Why would grasses and trees have the same leaf:stem biomass (5:95)?

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