

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 #2

Received and published: 11 May 2020

In this manuscript, the authors present total and bioavailable stocks of a number of nutrients and some other elements, in soil, saprolith, and vegetation along a steep climatic gradient in the coastal cordillera of Chile. From their data, they calculated a number of fluxes between the various ecosystem compartments and in and out of the whole system. Their main interpretations are that the weathering rates – which they term abiogenic – did not change substantially along the transect in spite of the marked climatic differences while the nutrient uptake rates increased with standing biomass following the rising rainfall rates. I think that the authors produced high-quality data

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from an interesting environmental gradient but I disagree with large parts of their data interpretation.

1. It is hard to believe that weathering rates do not respond to the strong climatic gradient. From the explanation in l. 603-607, I read that the “most negative tau values from the shallowest mineral soil sample of each regolith profile were used” [to calculate weathering rates] and that these values are shown in red in Fig. A1. I do not understand this. The red symbols in Fig. A1 do neither refer to the shallowest horizon nor are they consistently the most negative value. Why did you choose the most negative tau value and not the thickness-weighted average tau value of the whole regolith profile? I anyway think that the tau values cannot be used to estimate weathering, because they lump together a complex mixture of many processes including e.g., leaching losses, deposition input, plant uptake, or soil-internal redistribution. The latter is particularly pronounced in Nahuelbuta, where Podzols occur which show strong depletion (E) horizons and strong accumulation (Bhs) horizons. Instead, I think that an equation such as that proposed by Likens (2013), Biogeochemistry of a forested ecosystem (3rd ed). Springer, New York, USA should be used: $TD_i + W_i = ST_i \pm \Delta B_i \pm \Delta OM_i \pm \Delta X_i \pm \Delta M_i$ where TD is total deposition, i a selected element, W is weathering release from primary minerals, Delta is change, B is storage in biomass, OM is long-term storage in soil organic matter, X is the exchangeable pool and M is the secondary mineral pool. I particularly think that the immobilization in soil organic matter and the formation of secondary minerals belong to the weathering rates. An advancing weathering from N to S is also reflected by the soil development ranging from Regosols (initial A-C soils) via Cambisols (A-B-C soils) to Podzols (A-E-Bhs-(Bw-)C soils), which goes along with increasing mineral formation and in the most advanced stage also translocation of organic matter and soil minerals. This is in line with the cited soil production rates.

2. I have also difficulties to understand what your “ecosystem nutrient uptake fluxes” are. Is this the current annual uptake or is it a mean of a certain period? In l. 613,

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you mention net primary production (NPP) and one line later “GrowthRate”. Total nutrient uptake would, however, be related with gross primary production and growth rate sounds as if the standing nutrient stock is disregarded. Please clarify. What you call “recycling rate” (i.e. uptake rate divided by weathering rate) seems to me to be more of an “accumulation rate”, because the vegetation accumulates part of the nutrients released by weathering and deposited from the atmosphere. When enough nutrients have accumulated to support a mature forest, the majority of these nutrients is recycled with losses smaller than the weathering release (or the deposition from the atmosphere on very poor soils) until the forest starts to break down and rejuvenate.

3. Because all sites are about 80 km away from the Pacific Ocean, they should be similarly affected by Sea Spray. I would even expect an increasing Sea Spray deposition with increasing rainfall. This can, however, possibly not be detected because of the simultaneously higher leaching rates at the wetter sites. Nevertheless, the Sr isotope ratio in the plants can be explained as a mixture of the isotope ratio of the rock and Sea Spray at three of the four sites. The only exception is La Campana, where perhaps indeed the uptake from the subsoil dominates the Sr isotope ratio in the plant. Please also mind that plants can take up their nutrients with all surfaces and forests therefore retain nutrients in their canopy. Besides Sea Spray, I would also expect aeolian redistribution of soil material at the driest site. How can this be considered in your budgets?

4. The question “Are nutrient sources setting plant stoichiometry?” can be clearly answered with “no” based on well-established textbook knowledge. As an example, I cite an introductory sentence from the textbook “Regulation of Plant Nutrient Uptake” by G.N. Mitra (2015, Springer): “To cope with wide variations in mineral concentrations in soil, plants have evolved mechanisms so that net intake of a nutrient depends on the plant’s need for this element rather than its concentration in the rooting medium.” I could have cited any other textbook on plant nutrition. The author’s use of plant nutritional terms is in large parts wrong. While Fe is an essential micronutrient (not only

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beneficial), Al can only be considered as beneficial for some plants at low concentrations in soil solution. For most plants it can instead be toxic. All essential elements are needed at the same time so that there is not a “most plant-essential nutrient”. However, the plant requires different amounts of each nutrient. In plant tissue, e.g., the mean molar ratio of the K:P concentrations is 4.2 and the mass ratio is 5.3 (Marschner, 1995, Nutrition of Higher Plants, Academic Press). Nevertheless, P is more frequently growth limiting than K. In most cases, forests are limited by one nutrient or co-limited by two and sometimes even three nutrients. You cite Stock et al. (2019) who stated that your study ecosystems are N-limited, which is in line with the global mapping of nutrient limitation by Du et al. (2020, Nat. Geosci. 13, 221-225). If this is true, no other element than N will be growth limiting. Finally, nutrient concentrations in plants usually vary by less than one order of magnitude. If you now allow for a range of two orders of magnitude (as done in Fig. 6), you will for sure get an overlap between the soil and plant stoichiometry, which is, however, meaningless. Although the organic layer will more closely reflect the plant stoichiometry, its composition is still different from that of the plants, because some nutrients are to a large degree retranslocated prior to leaf/needle abscission (e.g., P) and others are quickly leached from the organic layer (e.g., K). Overall, I think that this part of the discussion must be completely revised or omitted. It would likely make sense to seek the support of a plant nutrition specialist.

5. I don't think that your view of “abiogenic” weathering is correct. The weathering in the deep subsoil (which you call saprolith) is strongly influenced by the acids produced by biological activity. The CO₂ concentration in soil air is frequently one order of magnitude higher than in the free atmosphere resulting in the formation of carbonic acid which is leached to the weathering front together with organic acids driving the chemical weathering. Moreover, it is highly unlikely that your saprolith is free of biological activity (i.e. plant roots, fungi, bacteria, soil animals), which would result in a direct biological acidification by the release of protons during cationic nutrient uptake, the release of organic acids, and the production of CO₂ by respiration. If there are roots, as suggested for La Campana, there would even be mechanical weathering by the plants.

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The source of acids originating from CO₂ dissolved in rainfall is much smaller than the biological CO₂ sources and it is already buffered in the vegetation canopy and the topsoil and does not reach the weathering front. I would therefore even turn around your conclusion, stating that in vegetated areas, there is likely hardly any abiogenic weathering. The latter might be restricted to not vegetated areas and mainly driven by insolation, salt and ice blasting.

6. The cation-exchange/carbonic acid proton buffer system has a small capacity and a high buffer rate. It is therefore quickly passed. More important are the carbonate/carbonic acid (pH 7-8), the Al oxide/strong acid (pH 3-5), and in the E horizon of the Podzol at Nahuelbuta the Fe oxide/strong acid (pH 2-3) buffer systems. All soils acidify in the course of their development because of a number of proton sources of which the carbonic acid formed by the much higher CO₂ concentration in the soil air than in the free atmosphere is the largest one. This acidification results in acid soils with a pH < 5 in the dissolution of Al oxides and the subsequent replacement of exchangeable base metals by Al³⁺. I don't understand how the cation-exchange capacity can exceed the bioavailable cation pool (determined as the sum of water-extractable and NH₄OAc-extractable fractions – if I understood the description of the methods correctly, where it is not described that the two fractions are summed up), because in one of the standard methods to determine the cation-exchange capacity NH₄OAc is used as extractant (and anyway any salt added in excess will exchange all exchangeable cations from the soil). The apparent difference can possibly be explained by the spatial heterogeneity but not by any mechanistic background. Exchangeable cations are entirely considered plant-available. The cation-exchange process is very fast so that the composition of the ion mixture on the cation exchanger surfaces is always in equilibrium with the soil solution.

7. The finding that the more developed the soils are, the larger is the part of the nutrients needed to satisfy the plant demand that is directly cycled between the vegetation and the organic layers/mineral topsoils and thus decoupled from weathering is known

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since the early 1980s going back to the work of e.g., Jordan (1982, Ecology 63, 647-654).

8. Finally, a merely formal issue: You present parts of your results threefold, i.e. in a figure, a table, and mention some numbers even additionally in the text (e.g., Fig. 4 and Table 3 or Fig. 5 and Table 6). I suggest to keep the figures and move the tables to the Supplementary Material to render the manuscript more concise.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-69>, 2020.

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