

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

Ralf A. Oeser and Friedhelm von Blanckenburg

oeser@gfz-potsdam.de

Received and published: 8 May 2020

*A note upfront from the submitting person: This review was prepared by four master students in geography at the University of Zurich. The review was part of an exercise during a second semester master level seminar on “the biogeochemistry of plant-soil systems in a changing world”, which is organized by prof. Dr. Michael Schmidt and myself. We would like to highlight that the depth of scientific knowledge and technical understanding of these reviewers represents that of master students. We enjoyed discussing the manuscript in the seminar, and hope that the comments will be helpful

C1

for the authors.*

REPLY We thank Marjin Van de Broek and in particular his master students for their detailed comments that indeed will be very helpful to revise our manuscript. We highly appreciate these comments as they point to some structural weaknesses in the manuscript in a constructive manner. In particular you seem to realize the innovative way of tackling the bio-geo interactions in the Critical Zone using novel metrics. You seem to have very good master students at Zürich!

We will REPLY point by point to the comments.

2. General Comments

COMMENT First of all, it is a very interesting and relevant study with the goal to investigate processes that are not yet fully understood. By investigating four climatologically different locations and corresponding different vegetation, the authors have taken a good approach to their research question, where Chile seems to be an ideal research site. However, in the paper a lot of different aspects and factors were included. We were wondering if some topics go beyond the scope. Therefore, you might consider to narrow down some aspects and factors.

REPLY: We are grateful for the reviewers view on our study design and approach to resolve the interactions between ecosystem productivity and silicate weathering. We acknowledge the reviewer’s comment on the multitude of (or too many) aspects of this manuscript. But please bear in mind that we employed a multitude of in part novel metrics to quantify the geological and biological fluxes in ecosystems. These itself and their context in the Critical Zone require a detailed description.

We will address the reviewers concern in a future version of the manuscript by restructuring certain parts of the text. In particular we will rephrase our research hypothesis at the end of the introduction and make better use of topic sentences to guide the reader through the manuscript. We will scrutinise the text for components that can be removed for now without loss of quality.

C2

COMMENT The discussion part is really interesting but without a lot of notes and rereading the text, it is really hard to understand and follow the main messages. Our general opinion is that some parts of the paper are not structured clearly, especially in the beginning, which makes it hard to keep track. By improving that, a lot of our following criticism will be redundant.

REPLY: We will better structure the manuscript and have other colleagues read it before re-submission.

COMMENT We do not comment on the applied measurements and analyses methods, since we do not have the appropriate expertise to evaluate these. However, what caught our eye was that no statistical method was mentioned, which could be included additionally.

REPLY: Concerning the note that no statistical evaluation is lacking we note that rigorous within-site replication exists. At each site two regolith profiles situated on opposing slopes have been studied (L176) to account for variations in substrate and/or effects of insolation and microclimate on weathering and nutrient uptake by plants. These two regolith profiles are natural replicates. The site description and description of the regolith profiles is based on Oeser et al. (2018) where four profiles at each site were measured. We can use this resource to develop an uncertainty estimation on the element fluxes.

COMMENT Having said that, the structure improved towards the end. We appreciated that in the results and discussion subtitles are used which makes it easier to navigate and keep up with the text. We found that the usage of literature is very good on which the argumentation is based on. Furthermore, we really enjoyed the graphs, they are nicely visualized. We thought that the title may be a bit long but seems appropriate to introduce the study.

REPLY: We are open to suggestions for a shorter title. Would it help if we removed the

C3

first part “Decoupling silicate weathering from primary productivity – “?
Language and definitions:

COMMENT We often find that the authors have written in a complicated manner where the sentences are too long and interlaced. It is very hard to follow the statement and thoughts. - We would suggest coming up with shorter and more precise sentences

REPLY: We will adjust our manuscript accordingly, and will ask a native speaker for advice.

COMMENT Also you might try to include more topic sentences

REPLY: Good suggestion. We will make better use of topic sentences to guide the reader through the manuscript.

COMMENT Furthermore, we found some concepts, short forms etc. without sufficient references or definitions. This leads to some difficulties during reading, and we propose to be more consistent when introducing short forms and concepts. For example: - p. 3, line 86: for the reader it is not that clear what EarthShape is, maybe introduce it shortly with a few sentences as a long term research project; especially since concepts of that project are important for the presented study

REPLY: We regret that the introduction of the EarthShape project – a large interdisciplinary research network - was not sufficient in the manuscript’s introduction. We will provide this information.

COMMENT Wxregolith etc. is not comprehensible = weathering from regolith and bedrock. Would it not be better to call it Wxsupply?

REPLY: Throughout our manuscript we applied the metrics to describe and quantify the element fluxes in the Critical Zone as introduced by Uhlig and von Blanckenburg (2019). In our manuscript we do only briefly introduce them to the reader and refer to Uhlig and von Blanckenburg (2019) for the detailed derivation and description of

C4

these metrics. Therefore, we prefer to retain to the terminology of Uhlig and von Blanckenburg (2019).

COMMENT p. 11, line 316: define new introduced short form U as uptake more clearly similar to the previous paragraphs

REPLY: In section 3.3 (L253) we introduce the metrics we use to parameterize the geogenic nutrient pathway and the organic nutrient cycle. We briefly refer to Table 2 and the Appendix. In a future version of the manuscript we will make sure that first used abbreviations will refer to Table 2 and the Appendix as well.

Typos:

COMMENT We found some typos which are listed below, therefore we suggest some revision grammar- and spelling-wise:

REPLY: We thank the reviewers for their careful reading! We will correct the typos and grammar errors accordingly.

3. Comments on manuscript Introduction

COMMENT In our opinion, the introduction seems too long and weakly structured. We guess not all the information is directly relevant for the specific topic of the article.

REPLY: We will condense the introduction. In particular we will put our emphasis on a clear definition of our research questions and hypothesis (see also below).

COMMENT p. 4, line 100-103: The research questions could be formulated more clearly. Maybe use 2-3 sentences to make it clearer. As we understand the question (1): "Is weathering increasing from north to south along the EarthShape precipitation gradient? We want to investigate the differences, although (or precisely because. . .?), there are similarities concerning mineral supply, dissolution kinetics of solids due to erosion rate and lithology."

C5

REPLY: We believe that our two principle research questions formulated at the end of the discussion are clear and concise and for clarity we would prefer not to extend them. They are: (1) Does weathering increase from north to south along the along the EarthShape precipitation gradient, because runoff increases while other factors like mineral supply and dissolution kinetics are similar due to the similarities in erosion rate and lithology? (2) Is the increase in net primary productivity (NPP) from north to south accommodated by additional nutrient supply from weathering? (3) Does the nutrient reservoir sets plant stoichiometry?

COMMENT p. 4, line 103-104: Why are the research questions already answered in the introduction? We think this belongs to different text bodies with some links to these questions here. - p. 4, line 104-106: In our opinion, this should be an outcome of the results, discussion and to conclude at the end. If you were able to identify something else than planned from the beginning, why not add or adjust the research question?

REPLY: We answered the research questions at the end of the introduction as a stylistic element and to trigger the readers' curiosity. We would prefer to retain this, but we do not insist.

Study area and previous results

COMMENT p.4, line 108: "Previous results" in the title is not clear for us. Is this meant as results from previous studies? If yes, we would recommend to put citations or the few relevant points for this continuative study. This could be the main part of the introduction.

REPLY: We will add: "This study is based on the findings of two previous studies that introduced the field area, its pedogenic and weathering characteristics, and a massive set of soil- and geochemical data Bernhard et al. 2018, Oeser et al. 2018)." Sampling

C6

COMMENT We can't find any explanations why there were different amounts of samples (bedrock and regolith) taken depending on the locations. e.g. there are less samples for Nahuelbuta

REPLY: The number of regolith samples depends on the depth of the dug regolith profiles (see Oeser et al., 2018 Fig. 3 to 6 for representative photographs of the regolith profiles at each site). Regarding the bedrock samples, we took as many representative samples as possible. However, access to these samples was highly variable and particularly difficult in the densely vegetated Nahuelbuta.

COMMENT Descriptions of the sites, sampling and why at some sites specific samples could not be taken could be written in the method part and not later in the text (e.g. discussion) e.g. p.18, line 544: Santa Gracia - the absence of a litter layer

REPLY: The architecture of the regolith profiles, mineralogy and chemistry has been extensively described by Bernhard et al. (2018); Oeser et al. (2018). However, we summarized these results in the section "Study area and previous results". We mention the absence of a distinct O-horizon in Santa Gracia (L155) and Pan de Azúcar (L153) but indeed do not mention the absence of a litter layer. We will add this information to the manuscript.

Results and discussion

COMMENT p.10, line 304: "Bio-availability of most elements in soil, bar a few exceptions, increase from Pan de Azúcar to La Campana and is lowest in Nahuelbuta." - We are not sure if you can generalize this like that, there are a lot of exceptions: Ca/Na is rather decreasing, Mg is lower in La Campana, Si is the same for Pan Azucar and Santa Gracia and Sr the same for Santa Gracia and La Campana. The only nutrients which increase are Al, Mn, P and kind of Fe, but there the value for Pan de Azucar is missing

C7

REPLY: The reviewers are right. This generalization does not meet the data. We will rephrase this section starting from line 301 accordingly: Bio-availability of the remaining mineral nutrients in saprolite generally decreases from north to south. Accordingly, the total inventory is highest in Pan de Azúcar (5100 g m^{-2}), intermediate in Santa Gracia (2100 g m^{-2}) and La Campana (1600 g m^{-2}), and lowest in Nahuelbuta (140 g m^{-2} ; Table 4). Note that $I_{bio-av, sap}^X$ was calculated over the uppermost 1 m of saprolite, whereas in fact the zone of mineral extraction might extend much deeper. Bio-availability in soil features a similar trend. Total availability is highest in Pan de Azúcar (2100 g m^{-2}), on par in Santa Gracia (960 g m^{-2}) and La Campana (1000 g m^{-2}), and lowest in Nahuelbuta (200 g m^{-2}). However, especially P and K deviate from this general trend. . . .

COMMENT p.10, line 310: "Average elemental concentrations in bulk plants decrease from Pan de Azúcar towards Nahuelbuta." → We think there are a lot of exceptions too: There are higher values in Santa Gracia (17'800) than in Pan Azucar (15'200) for Ca, higher values in La Campana for Fe,Sr, K and higher values in Nahuelbuta for Mn and Mg

REPLY: We will address this point by rephrasing this paragraph accordingly: (L310) Average elemental concentrations in bulk plants generally decrease from Pan de Azúcar towards Nahuelbuta. However, element specific deviations from this pattern do exist (Table 5). The most prominent exceptions are those of P and K. Average P concentration increases from. . .

COMMENT p.11, line 315: "The nutrient-uptake fluxes of the two most important rock-derived mineral nutrients to plants, P and K, increase steadily from north to south. . ." - we wondered if "steadily" is the right word since there is an exception for Nahuelbuta (1400) which is lower than for La Campana (2900) for K

REPLY: Taken the standard deviation into account our argument holds true. However, we will not use the word "steadily" in a future version of the text.

C8

COMMENT p.13, line 377: “As an evaluation of the hypothesis that the nutrient reservoir sets plant stoichiometry ...” → We would recommend that you formulate your hypotheses already in the introduction and not only in the discussion part (like on p.1, line 30)

REPLY: In order to better structure this manuscript we will relocate our hypothesis to the end of our introduction.

COMMENT p.16, line 480: Why do you introduce the turnover time differences in the discussion part and not in the results part? You might want to consider introducing this term as well because you use the term a lot in the discussion part.

REPLY: There is a delicate balance between a result and their interpretation. Only the element composition and radiogenic Sr isotopes in bulk samples, the bio-available fraction, and in plants are results *sensu stricto*. Element fluxes, weathering rates, etc. are parameterized and an interpretation of this data. The same holds true for the turnover times. Also the turnover times are a derived parameters based on assumptions (i.e. they rely on the parameterized fluxes U_{tot}^X and $W_{regolith}^X$). Thus, we prefer to retain in the discussions section. The same holds true for the nutrient recycling factor RecX.

COMMENT p.17, line 513: “We speculate that the effect of vegetation might even compensate for a potential increase in weathering that would be caused by the increase in MAP, essentially damping the geogenic pathway.” - Would it not be better to include this part in the conclusion?

REPLY: There is a high level of speculation in this part- meant to trigger discussion within the community so as to appreciate the complexity of these feedbacks. The point is truly “discussion” rather than a firm “conclusion”. Thus we would prefer to retain this in the discussion section.

Conclusion

C9

COMMENT We found that you did not clearly answered the research questions from the introduction

REPLY: We think the research questions are answered, But as we will revise the research questions anyway we will also increase the recognition of their answers in the conclusion section.

Figures and Tables (only replies to substantial comments)

COMMENT For us, an illustration summarizing the results would help the understanding of the paper (maybe like figure 1)

REPLY: We are limited to a certain amount of publication units and must condense the manuscript, figures and tables as much as possible. If BG offers to submit a graphical abstract, we will create one.

References

Bernhard, N., Moskwa, L.-M., Schmidt, K., Oeser, R. A., Aburto, F., Bader, M. Y., Baumann, K., von Blanckenburg, F., Boy, J., van den Brink, L., Brucker, E., Canessa, R., Dippold, M. A., Ehlers, T. A., Fuentes, J. P., Godoy, R., Köster, M., Kuzyakov, Y., Leinweber, P., Neidhard, H., Matus, F., Mueller, C. W., Oelmann, Y., Osés, R., Osses, P., Paulino, L., Schaller, M., Schmid, M., Spielvogel, S., Spohn, M., Stock, S., Stroncik, N., Tielbörger, K., Übernickel, K., Scholten, T., Seguel, O., Wagner, D., and Kühn, P.: Pedogenic and microbial interrelations to regional climate and local topography: new insights from a climate gradient (arid to humid) along the Coastal Cordillera of Chile, *Catena*, 170, 10.1016/j.catena.2018.06.018, 2018.

Oeser, R. A., Stroncik, N., Moskwa, L.-M., Bernhard, N., Schaller, M., Canessa, R., van den Brink, L., Köster, M., Brucker, E., Stock, S., Fuentes, J. P., Godoy, R., Matus, F. J., Osés Pedraza, R., Osses McIntyre, P., Paulino, L., Seguel, O., Bader, M. Y., Boy, J., Dippold, M. A., Ehlers, T. A., Kühn, P., Kuzyakov, Y., Leinweber, P.,

C10

Scholten, T., Spielvogel, S., Spohn, M., Übernickel, K., Tielbörger, K., Wagner, D., and von Blanckenburg, F.: Chemistry and Microbiology of the Critical Zone along a steep climate and vegetation gradient in the Chilean Coastal Cordillera., *Catena*, 170, 183-203, 10.1016/j.catena.2018.06.002, 2018.

Uhlig, D., and von Blanckenburg, F.: How Slow Rock Weathering Balances Nutrient Loss During Fast Forest Floor Turnover in Montane, Temperate Forest Ecosystems, *Frontiers in Earth Science*, 7, 10.3389/feart.2019.00159, 2019.

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