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Reply on RC1

Mirjam Schaller and Todd A. Ehlers

Author comment on "Comparison of soil production, chemical weathering, and physical erosion rates along a climate and ecological gradient (Chile) to global observations" by Mirjam Schaller and Todd A. Ehlers, Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-22-AC1, 2021

General comment to the handling editor(s) and both reviewers.

We thank the two anonymous reviewers for their constructive and insightful comments. We appreciate the time they have spent on evaluating this work. Nearly all of the suggested changes are reasonable and we implemented the changes as described in this response to reviews.

General comments were addressed in rewriting chapter 1 Introduction in order to present a coherent text to the reader. In addition, chapter 3 Methodology was adjusted to the suggestion made. Last but not least a chapter 5.3 Study caveats and challenges was added to the discussion. The specific comments are addressed below. The reviewer's comments are in bold, the replies in italics, and changed text in normal font.

Response to reviewer comments 1 for preprint esurf-2021-22

General comments

The manuscript presents soil and saprolite data from a climate gradient in Chile in granitic lithology. It investigates the potential controls of climate and vegetation on rates of soil production, total denudation and chemical/physical weathering. In addition, it compares the Chilean data to a global dataset and models of soil production. The manuscript is generally well written, and the discussion of the data is very detailed covering multiple aspects of how climate and biota can potentially affect soil production and related properties. I also very much appreciated the assessment of previously developed soil production models against real data. These parts of the discussion are very commendable.

Response: We thank the reviewer for her/his enthusiasm about the manuscript, and the constructive comments that follow. We find all find that all of the suggested changes are reasonable and will lead to an improved manuscript.

However, considering the constraints of the Chilean dataset (geochemical variability within sites, limited replication of sites, outlier treatment), there is the real danger of overinterpretation. In context of the global datasets, the interpretability and potential for generalisations (at least for granitic lithologies) improves, but I would still advocate for not overinterpreting the soil production/weathering data as mainly driven by climate/vegetation, since 1) most of the interpretation is based on visual assessments of scatter plots, and 2) other drivers of soil production, like tectonic uplift, should also be considered in the assessment. While the paper is naturally focussed on identifying climate/vegetation as drivers of soil production/weathering, I feel that the discussion does not sufficiently challenge this link. In addition, I highlight a specific issue with an external dataset that gives reason for extra caution when analysing collections of regional case studies. When these regional case studies are subsequently linked to environmental covariates sourced from global models and datasets of coarse spatial resolution (e.g., global climate models, global-scale topographic/vegetation data), mischaracterisation of case study sites can easily occur. In summary, I think there is strong merit in the publication, combining a regional case study with a global perspective on soil production in granitic lithologies, if the constraints in the data and methodology are adequately acknowledged, and a more balanced discussion of the patterns in the data is provided.

Response: We thank the reviewer for highlighting this concern. Throughout the manuscript we do mention and discuss various caveats such as the reviewer mentions (e.g., geochemical variability, different rock uplift rates, etc…). However, our approach was to address these factors in a dispersed way throughout the text that apparently diminished the impact / intent for readers (not our intention). Therefore, to add more emphasis and clarity to potential caveats, in the revised manuscript we will add a new discussion section (5.3 Study Caveats and Challenges) at the end of the document that discusses the items mentioned by the reviewer (and others) into one place.

*Please note that while we agree with the reviewer that geochemical variations and other factors such as geographically different rates of uplift, and resolution of climate and vegetation data used can impact the relationships looked at, there are a few fundamental factors that need to be realized by readers. First, the soil production, denudation, and chemical weathering rates looked at in this study are integrated over thousands to hundreds of thousands of years (i.e. the integration timescale pertinent to interpreting cosmogenic nuclide derived denudation rates). Thus, inherent to our approach is a temporal averaging of results. This means site specific local variations in vegetation or climate are averaged out to some degree. Second, spatial variations in rock uplift rate are indirectly considered in our study via consideration of slope. Topographic slope is strongly dependent on rate of rock uplift (and lithology, which we've accounted for as best as possible by focusing on granitic settings). Third, throughout the study, we focus on the large-scale trends in the data over a range of precipitation rates and vegetation cover. It would be an unusual coincidence if **local** scale variations in chemical composition of bedrock or vegetation / precipitation resulted in a global or even regional (e.g., Chile) trend as we document here. Rather, more likely is that these factors are the cause of the variance in the data along the global trend, rather than the trend itself.*

In summary, in the new discussion section, we discuss the factors mentioned by this reviewer as well as the potential impact they have on our interpretations.

Specific comments

L29-43

I think this could benefit from a restructure and rewrite. Several concepts are presented (regolith, soil erosion, soil production, soil denudation) but the individual sentences are not well linked up into a coherent line of thought. It reads more like a collection of definitions where the reader has to fill in the gaps but not as an introduction.

Response: Sorry about this. The section mentioned was restructured and rewritten with the intention to introduce a coherent line of thought.

L62-76

Given your study is strongly linked to a previous publication, co-authored by you (Oeser et al 2018) and containing similar/same datasets, can you please indicate clearly in the introduction: what is the novel aspect of this new manuscript?

Response: Corrected as suggested. Thanks..

L98

This comments also applies to the other site descriptions:

I would suggest some clarifications here 1) soil horizon thickness: do you combine A and B horizon thicknesses for this? 2) Clay content, pH, bulk density values: Are these profile averages across all soil horizons?

Response: For the clarification of the two points raised by the reviewer we added at the end of Section 2 "Chilean study areas" and before 2.1 "Pan de Azucar" a section indicating in what the different parameters are measured and where the values are reported. Hopefully, this addition clarifies the manuscript. The added section is:" The combined thickness of A- and B-horizons is considered as soil thickness (see Table S1 in Oeser et a., 2018). The reported clay content, pH, and bulk density are the pedon averages of each study area (see Table 3 in Bernhard et al., 2018). The chemical index of alteration (CIA; after Nesbitt and Young, 1982) for bedrocks is a study area average whereas the CIA for regolith is reported for specific horizons (for more details see Table S5 in Oeser et al., 2018). The cosmogenic nuclide-derived denudation rates are reported for South- and North-facing mid-slope positions (see Oeser et al., 2018 and Table S6 in there)."

L124

Regardless of the different ways to define soil, regolith, saprolite etc., an Umbrisol is a soil type and not a regolith type after WRB.

Response: In order to avoid confusion due to the different soil definitions, the sentence has been changed to: "The Umbrisol has soil horizons as thick as 60 to 90 cm and a clay content of 26.2 ±2.6%.". *We hope that this is correct.*

L133

Since saprolite plays an important role in the manuscript, how was it recognised and distinguished from mobile regolith in the field when sampling?

*Response: The following clarification has been added to the text in question: "*The top of

saprolite is considered to be the first encounter of in situ weathered bedrock represented by the C-horizon. This sampling strategy is a common approach for calculation of soil production rates from cosmogenic nuclide measured in pedons (e.g., Dixon et al., 2009). Representative photographs of this horizon from the Chilean study areas are available in Oeser et al., (2018: Figures 3 to 6)."

L144

What are the uncertainties in Tables S2 to S5? Are they all SEM? Not fully clear in the table.

Response: Sorry about that. What the uncertainties represent have now been described in foot notes of Table 2 to 5.

L164

What is SP_{soil}? Do you mean SPR?

Response: Yes, we do mean SPR. The oversight has been corrected.

L165

Are the concentrations of Zr and that of other elements in each soil a weighted average of the entire soil profile (see also the tables in the supplement)? Given that these concentrations can vary significantly throughout the depth profile of a soil, it would be good to clarify what these values that represent each pedon are.

Response: yes – the reported values are the average concentrations for all samples in the layer referred to. We have more explicitly clarified this in the text and reads now like:"…., where Zr_{soil} is the average Zr concentration for soil samples from the pedon. Similarly, Zr_{sap} is the average Zr concentration of the saprolite samples from the pedon. Zr_{rock} is based on the average of all bedrock samples collected in one specific study area (see Table S3 based on Oeser et al., 2018)."

L174, L62-64

I would recommend to directly link the methods in 3.3 to the hypotheses. How are the methods used for testing the individual hypotheses (e.g., correlation estimation between which variables, significance tests). Also, the hypotheses only refer to the Chilean sites, but you appear to test those hypotheses across the global dataset as well. The discussion preamble (L247-249) also does not quite conform to these hypotheses.

Response: We have now explicitly stated this link to hypotheses in section 3.3.

L178

I would recommend making the statistical analysis clearer: you estimated the Pearson correlation coefficient assuming linear relationships between your variables and tested for statistical significance of these relationships (using a t-test I presume, under the assumption of normally distributed data).

Response: Thanks for this suggestion. We've clarified the text, and also mentioned that table S8 contains the R2 and P values.

L179

I would appreciate some more information in the main text on what kind of models these are. The details of each model are well placed in the supplement but some general description of what they are doing would be very helpful to be included in the methods. The would support the understanding of why you include these models in the first place and how this comparison of model predictions and your data contributes to testing the hypotheses.

Response: We have now rewritten section 3.3 and present the basic concepts of each model considered in the main text.

L194

Are the SPR-related uncertainties also SEM?

Response: No, they are not. We have now clarified this (see response above) in the corresponding table S2.

L232-234

Why were other samples that show a negative weathering rate not excluded but only NAPED20?

Response: Ooops. Thanks for catching this. The data were not handled consistently. We now include all samples and do not remove any. The text has been adjusted.

L238

Looking at Figure 2A it seems that La Campana is only different to Nahuelbuta because of the very high value of LCDEP30. This sample was excluded when summarising the data for the La Campana site because of the steep slope (L223, S4). If only this this sample was also excluded from Figure 2 based on its very high values (i.e., outlier because of it unusually steep slope), the differences between La Campana and Nahuelbuta would completely disappear (2A, 2C), and likely won't be statistically significant for any other panel in Figure 2, given the sample size and uncertainties. Considering inherent geochemical variability at each site (as reported in the results section and later discussed below) and differences in topography between sites (e.g., effect of slope), how can you be sure that the differences between the sites, particularly between the 2 most humid sites, are indeed mainly a reflection of differences in climate/vegetation and not of other reasons? It is interesting to note that Oeser et al (2018) also considered differences in uplift rate and topography as reasons for the differences between the two most humid sites (6.1.1 in Oeser et al. 2018). As such, statements as in L307-308 sound less convincing, including claiming the "commonalities" with the global dataset that is interpreted as mainly driven by climate/vegetation (L255+).

Also considering my comment on L232-234, there seems to be a lack of consistency in the treatment of so-called outliers. See also L265-266 – an exclusion for which the reason is not well explained in the text (at least to my understanding).

Response: Thanks for your thoughts on this. It's important to note that with the number of available samples from each Chilean study area (n= 4 to 7) that statistical significance cannot be established as suggested in the comment. Nevertheless, we see your point. We have changed Figure 2 so that the sample locations are color coded by topographic slope (as reported in Table S1). We have also modified the text so to state that the LC study

area shown in Fig. 2 is either equivalent the NA area, or potentially higher but that given other differences between the areas this cannot be accurately resolved. As a side point – it is worth remembering that global data set suggest higher values for this precipitation rate.

L271

The maximum SRP's shown in Fig 3B appear to come from Larsen et al (S7). Having some regional knowledge of their sites, the precipitation values derived from Karger et al. are well below the values from actual observations and those of the regional climate models (e.g., https://niwa.co.nz/climate/national-and-regional-climate-maps/west-coast). For instance, the Rapid Creek sites are less than 10 km from a rainfall gauge (Cropp River) that receives >10 m of MAP (http s://data.wcrc.govt.nz/cgi-bin/HydWebServer.cgi/sites/details?site=81&treecatchment=3). Using a national NZ dataset (the data is available here: https://data.mfe.govt.nz/layer/53314-average-annual-rainfall-19722013/), the SRP maximum at ~3000 mm would disappear for then non-granite sites in Figure 3B and shift to between 7500 and 8000 mm; by using regional climate data, there would be no evidence for a humped relationship between SRP and precipitation. The rainfall data of the national NZ rainfall model should be better adapted to the extreme orographic conditions of NZ's Southern Alps than a global model. See also the general comments.

While I can't comment on data specifics in the compiled granite dataset given the limited review time, the lack of a similar observation in non-granitic lithologies (as far as presented here) reduces the general application of the observations in granite lithologies and should be reflected in the later discussion, including the following paragraph and 5.2.2.

Response: Thank you for your thoughts on this. We have modified the text in the caption to mention that higher precipitation rates are documented for NZ from other data sets you mention. However, we think it is more important to handle all data in the same way when comparing to things such as climate data so that there is a consistent handling of not only how the precipitation data is processed, but also the time span of data used. The ERA-interim data is the basis for the CHELSA climatology we use in this study. ERA-interim reanalysis data is a standard and trusted data product for climatological studies. To not use this one region, or two select different 'regional' climate data sets for each study area would not be recommended. There are simply too many differences (methodological and observational) with how climate data can be processes. We chose to avoid a picking-and-choosing of different climate data sets that can introduce biases in to the analysis. However, much of this discussion is irrelevant because we don't actually use the NZ data in our analysis that the reviewer refers to because they do not come from granitic settings.

While we appreciate the reviewer's interest and knowledge on this topic, we respectfully disagree with this suggestion on several levels. In the spirit of scientific exchange, we elaborate a bit on this (although please keep in mind we are not even using the NZ data in our analysis). First, in climate literature it is frowned upon to compare a single weather station measurement (as suggested above) to a climatological downscaling result. Point measurements from weather stations frequently disagree with climatologies (which present an spatially and temporally integrated average….which is of higher relevance for comparing to temporally average cosmogenic nuclide SPR data). The average of many weather stations within a 'grid box' of a climate data set would be more appropriate, but not possible in this case. Second, the CHELSA data set (Karger et al., 2017) used here is a 30-arc sec resolution (900 m) which is, for climate data, high resolution. The CHELSA

data is a downscaled data product that uses weather station and ERA reanalysis data within it. We couldn't confirm if the station mention by the reviewer is included in it, but given the thoroughness of the ERA data it most likely is if it's over a 30 year time span. One of the advances of the CHELSA data set is also it's consideration of orography and valleys (see Karger et al., 2017). Finally, the CHELSA data set was peer reviewed (Karger et al, 2017) and as far as we could tell, the NZ web sites referred to above are not peer reviewed.

In summary, we've added text to the manuscript stating that local meteorological measurements may differ from what is reported here, but that we focus our analysis on a set of consistently processed, peer reviewed, global data to avoid biases in downscaling results between more regional or local studies.

L349-354

I miss the discussion of tectonic uplift as a potential driver for differences in soil production rates. Could some of the pattern in the SRPs of the global dataset not also be linked to tectonics? I suspect that not all data points in Figure 4 and 5 are subject to similar tectonic uplift rates and this is briefly touched on in L262. Put differently, for given uplift rates, would the same patterns regarding vegetation and climate parameters persist? You have done this for different slope classes in Figure 4, should a similar analysis not also be done for uplift rates?

Response: Thank for mentioning this. We've modified the last paragraph of section 5.1 to explain better how different rates of tectonic uplift are manifested in slope angles.

L277

What does 'This' refer to?

Response: Sentence fixed / clarified.

L317

The comparison with the EEMT approach is only shown in S6 but not discussed in the text. I would recommend discussing them as well to allow for a full cross-model evaluation.

Response: We've now clarified this in the text in section 3.3 (Methods). We prefer to not explicitly discuss it within the text because a) we don't want to attack another study in detail in our manuscript; b) the original study of EEMT does not compare to the same breadth of data (only stream data of Riebe et al); and c) the model does not come close to fitting observations presented here …. So we don't see the point in an already long enough paper to discuss a poor fitting model.

L366-369

This sounds contradictory – first, it is stated that bedrock Zr is lower than soil and saprolite Zr, but then an example is presented, where this is not the case. And there are other examples in the data where Zr in soil or saprolite is lower than in the rock (e.g., see AZPED sites).

Response: We thank you for pointing this out. We have clarified the text and implications for this.

L374-375

The large uncertainties around Zr are acknowledged in the preceding sentences, but then the 50% of CDF (a variable heavily depended on Zr) is rather firmly interpreted as the ceiling for CDF values (for sites "where chemical weathering happens"). I would recommend to word this accordingly to reflect the uncertainty of the Zr data. This also applies to the discussion of the differences in CDF and Wtotal between the two wettest sites (L387-416). It goes to great lengths in explaining the potential drivers behind the differences in the data, but I think it should also be acknowledged that because of the chemical variability and the limited replication, the differences between sites may not be only a reflection of climate/biota but also of other factors. This comment is similar to a previous one regarding Figure 2.

Response: Thanks for the comment. The text has been modified to mention this and tone it down.