Earth Surface Dynamics Discussions

Earth Surf. Dynam. Discuss., referee comment RC1
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Comment on esurf-2021-22

Anonymous Referee #1

Referee 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 Alan Ehlers, Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-22-RC1, 2021

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. 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.

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.

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?

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?

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.

L133

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

L144

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

L164

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

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.

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.

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).

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.

L194

Are the SPR-related uncertainties also SEM?

L232-234

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

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).

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 (https://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.

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?

L277

What does ’This’ refer to?

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.

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).

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.