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.

<u>Response to reviewer comments 1 for preprint esurf-2021-22</u>

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 (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.

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.

Schaller and Ehlers combine new and existing measurements and calculations of soil production, chemical weathering, and physical erosion rates to examine trends between climate, vegetation, weathering, and erosion at four sites along a transect of the Andes spanning diverse precipitation, temperature, and vegetation zones. They analyze trends at these four well-characterized sites and extend their analyses to a global compilation of similar measurements in granitic catchments worldwide to demonstrate the nonlinear and non-monotonic relationships between climate, vegetation, weathering, and erosion. I found the manuscript to be well organized and written, with high quality figures clearly conveying the results. More importantly, I also found the analyses and discussion to be interesting and well conceived — not only to address the hypotheses of the study, but also to examine the complexities within the large and diverse datasets it aggregates.

Response: Thank you for your time in reviewing the manuscript and your enthusiasm for it.

That said, my most substantive comment is that, in a few places (noted below), I felt that greater explanations of how the hypotheses and results presented here relate to previous work are needed to properly put the results in context. For instance, the hypotheses follow a summary of past research in the region that largely contradicts the hypotheses, yet no explanations or references substantiating the hypothesized relationships between soil production, erosion, climate, and vegetation are provided. Of course, these are largely explained (and even demonstrated in the figures later) by comparing the data to various empirical predictions in the results section, but I believe proper context and referencing is needed in the introduction. Moreover, many references in the discussion seemed to point readers to past studies rather than identify and explain the relevant connections between the presented results and this past work.

Response: We are a little bit confused on what exactly the reviewer is referring to without an example or two. Nevertheless, we've tried to clarify things by modifying the last paragraph of the introduction where hypotheses are presented to reference other literature, and also allude to the results we present. We have also modified different parts of the discussion section, as suggested, to mention when our results agree or disagree with previous studies.

GENERAL COMMENTS:

- I appreciate that paleoclimate considerations are discussed in lines 86-94 and I wonder if other paleo-environmental conditions (and potential changes) that may affect your results and interpretations are worth considering. For instance, I presume measurement of the various factors that may explain the low contribution of chemical weathering to total denudation in the southernmost study site (e.g. solute fluxes, organic acid concentrations, soil thickness, microbial abundance) are based on modern observations, but have these also

remained constant (or at least similar in pattern amongst the study sites) over the integration timescales of cosmogenic erosion rates?

Response: Well…. Very interesting point! Unfortunately – this a large problem facing these types of studies because, with the exception of paleoclimate predictions (which are also often quite sparse) there simply are not data sets available for the other items mentioned like paleo solute fluxes, paleo microbial abundances, etc. However, the point raised is a good one. In the new discussion section 5.3 (Study caveats and challenges), that was added in response to the other reviewer, we have added text addressing this comment and that future studies could also address this as it adds an unknown to our (and every other paper on this topic) analysis.

- It seems worth addressing how slopes have been calculated in the studies utilized in the global compilation (e.g. if they have been calculated from similar resolution DEMs and over similar length scales and differencing or averaging schemes e.g. topographic slopes vs steepest-descent slopes)…unless standardized slope measurements from GTOPO30 have been used (column 15 in table S8)? I don't suspect that differences in calculation would dramatically affect the binning into (rather generously sized) 10 deg slope bins, but given the significance of this binning on your analyses and known e.g. scale-dependence of slope measurements, I do think this warrants some discussion.

Response: We have now clarified how slopes were computed in the supplemental material where we already discuss this, and also in the figure captions. For brevity – figures NOT involving a correlation analysis use the original reported slope by the study (when available). If the original study did not report a slope the symbols were not color coded to indicate this. For the correlation analysis we wanted to avoid differences between how individual studies calculated slopes and treated all data the same – so for the correlation analysis we extracted the slopes form GTOPO30.

MAIN TEXT LINE COMMENTS:

Throughout: numerous places where north and south are unnecessarily capitalized (e.g. in "North- and South-facing" and e.g. line 256). Clauses beginning with "which" should be preceded by a comma.

Response: Thanks. Fixed. The document was scanned for North and South and changed to north and south where necessary. In addition, commas were added to clauses beginning with which.

Line 20: right parenthesis missing at end of sentence

Missing parenthesis was added.

Line 42: I am a bit confused here if denudation rate refers to chemical denudation rate specifically or to total denudation rates (guessing the latter)…particularly since "total denudation rates" are referenced in lines 55-56. Perhaps add "total" before denudation rate or "(physical plus chemical denudation)" after?

Response: Yes, we meant total denudation rates and corrected the section in question.

Lines 62-64: I'm a bit confused about the hypotheses since they seem to partly disagree with the previous observations just discussed…? Perhaps this should be explained briefly (or appropriate citations added supporting these ideas)?

Response: Thank you for highlighting this. We've done a massive rewrite of the introduction to make it clearer and have specifically set up more clearly the contradictions in previous work and how our hypotheses stem from them.

Lines 80-81: Minor point, but I'd suggest rephrasing "where the neighbouring…" since I think "due to subduction of the Nazca Plate" does not fully explain the along-strike similarity to which you are referring (the Nazca Plate also subducts below the Northern Andes, where you have flat slab subduction) and it's worth noting that the tectonic regime within the study regions is similar beyond just Nazca Plate subduction

Response: Rephrased to state similar geometry and orientation of the subducting Nazca plate near the coast.

Line 83: delete comma

Response: Comma was removed

Line 88: delete comma

Response: Comma was removed

Line 90: exist → existed

Response: Error was corrected

Line 109-110: add comma after 43 and delete comma after citation

Response: Comma was added respectively removed

Line 151: missing right parenthesis after units of soil production rate. Add "are" before "the mean…"

Response: Missing parenthesis was added and the "are" inserted.

Line 152: length → lengths

Response: S was added

Line 157: Is there some new meaning to the square brackets used to enclose variable units? If not (as I presume), I'd suggest standardizing throughout

Response: In physics / geophysics literature units are typically (not always) reported in square brackets. We changed it to be consistent throughout the manuscript.

Lines 178-180: Seems like this sentence should only state that the leaf area index LAI and SPRs at sample locations were compared to model predictions (if I understand correctly)

Response: The sentence was corrected as suggested.

Line 232: missing right parenthesis after first "yr"

Response: Parenthesis was added

Line 240: Why do you only point out the similarity in rates between La Campana and Nahuelbuta for physical erosion rates? The majority of the rates also appear to agree between the two sites for soil production and chemical weathering, no?

Response: Thanks for catching this. We've modified the other relevant parts of the text to address this.

Line 247: you've been using an oxford comma up to here…add comma after "weathering" for consistency

Response: Comma was added

Lines 273-275: Optional, but it would be helpful to briefly summarize e.g. Heimsath and Whipple's finding about the influence of lithology and rock strength variations on SPRs here and move the citation to the end of the sentence, instead of simply directing readers to that paper

Response: The reference has been added to the end of the sentence.)

Line 277-278: Couldn't it alternately suggest that MAP simply influences SPR non-monotonically, even in the absence of other processes/environmental differences? I'd at very least suggest rephrasing "this observation could suggest that…" Perhaps "processes" should also be changed to e.g. "factors" since MAP is not a process.

Response: Sentence changed as suggested.

Lines 279-280: move citations to end of sentence

Response: Citations were moved to the end of sentence

Line 280: "relationship in" → "relationship between"

Response: Suggested change was made

Line 289: weekly → weakly

Response: Correction was made. Thank you.

Line 290: "suggest an even weaker correlation" → "correlate even more weakly"

Response: Sentence was changed as suggested

Line 291: missing oxford comma

Response: Missing comma was added

Line 295-296: "hillslopes with slopes >30 deg…decrease with decreasing slope"?? I'm very confused about what this refers to since Figure S4 appears to show increasing trends with slope. Perhaps this should be a citation to Figure 4 and say "SPRs within the highest slope bins decrease with slope"?

Response: Latter half of sentence reworded to clarify meaning.

Line 298: I'm not sure it's necessary to say "qualitatively" since you are showing a quantitative trend here. Suggest deleting.

Response: "Qualitatively" was deleted

Line 299: where by → whereby, SPR →SPRs

Response: Both errors were corrected

Line 308-309: I find "SPR is a process of chemical weathering and physical erosion" to be a bit awkwardly worded. Perhaps change "SPR" to "soil production" or "SPR depends on both chemical weathering and physical erosion"?

Response: Sentence corrected as suggested.

Line 317: Should Figure 5B be referenced here too?

Response: Text was rearranged and reference is not needed anymore.

Lines 322-323: These two sentences confused me, since the black bold line in 5A shows the empirical prediction, no? I suggest rephrasing to clarify this (perhaps "...are predicted to increase rapidly with..."

Response: Corrected as suggested by the reviewer.

Lines 349-350: suggest changing "a combination of increasing...vegetation" to "variations in MAP, soil depth, and vegetation worldwide" since your data and the model-data comparisons show that non-monotonic relationships and co-variation of these factors can perhaps explain the global variations

Response: The sentence was changed as suggested and reads now as:" In summary, based on the previous considerations, we find that global variations in observed SPRs can be explained by variations in MAP, soil depth, and vegetation worldwide (Fig. 5)."

Line 354: occurs → occur, "for different slope areas observed" → "across different hillslope gradients" or simply "across the different slopes observed" perhaps (in any case, I find "slope areas" to be confusing since it could be confused for some metric of drainage area, too)

Response: Sentence was adjusted and reads now as:" We note that although high-slope settings produce the highest SPRs, the trends in MAP and vegetation causing an increase and then decrease in SPRs as MAP or LAI increase occur across different hillslope gradients (Fig. 4). "

Lines 366-367: I think this statement should be qualified a bit ("may still be meaningful") since the extent to which the measured Zr concentrations of bedrock truly reflect the Zr concentration of the parent rock from which soil and saprolite derived is still uncertain, even if the sign of change is correct

Response: Section has been reworded to address this comment.

Lines 368-369: suggest combining these sentences "...is negative because the ZR concentration in the saprolite is lower than..."

Response: Sentence was corrected as suggested and reads now as:" In contrast, the calculated fraction of weathering in saprolite for sample NAPED20 in Nahuelbuta is negative because the Zr concentration in the saprolite is lower than the concentration in the bedrock. "

Line 370: "over" → "to"

Response: Corrected as suggested.

Line 371: "as, for instance, in…"

Response: Corrected as suggested.

Line 375: add comma after "happens"

Response: Corrected as suggested.

Line 378: "orth" → "north"

Response: Corrected as suggested.

Line 379: "neither" → "not"," "nor" → "nor by"

Response: Corrected as suggested.

Line 389-390: by effectively diluting it? Perhaps change "diminish" to "dilute" if so…?

Response: "diminish" was replaced by "dilute" as suggested.

Line 398: "leading to an" → "which may underestimate Wtotal and overestimate Esoil"

Response: Sentence corrected as suggested and reads now as:" Due to this possible Zr loss in regolith, the calculated W_{soil} is a minimum value which may underestimate W_{total} and overestimate E_{soil}.*"*

Line 403: "As with" → "With"

Response: Corrected as suggested.

Line 406: the passive voice here makes it unclear if the attribution of the stabilizing effect of plants on the decrease in physical erosion rates has been proposed previously in other studies (guessing not?). "We attribute the…" or perhaps "The decrease in physical erosion rate may result from…"

Response: Sentence corrected and reads now as:" The decrease in physical erosion rate may result from stabilizing effects of plants.*"*

Lines 407-408: "increases" → "increase", "is lower again" → "decrease again"

Response: Sentence was adjusted as suggested.

Line 410: suggest flipping sentence "Precipitation, temperature, and pH all affect microbial abundance"

Response: Sentence corrected and reads now as:" Precipitation, temperature, and pH all affect microbial abundance (e.g., Fierer and Jackson, 2006; Bahram et al., 2018; Tan et al., 2020).*"*

Line 412: "in" → "along", "increase" → "increases", "decrease" →"decreases"

Response: Sentence was corrected.

Line 414: perhaps this statement should be qualified "which may explain the decreasing pH values from north to south and the lower bacterial abundance…"

Response: The corrected sentence reads now as:" A comparable increase in MAP and decrease in MAT is observed in the Chilean Coastal Cordillera, which may explain the decreasing pH values from north to south and the lower bacterial abundance in Nahuelbuta than La Campana (Bernhard et al., 2018; Oeser et al., 2018).*"*

Line 415: "made observations are unique" → "hypotheses are valid"

Response: Sentence was changed to address the comment.

Line 420-421: I think this sentence should be split to clarify that the observed corrrelations between denudation rates and chemical weathering do not derive from this same global compilation.

Response: Sentence fixed/clarified.

Line 425: "is absent or reduced" → "is not operative or occurs only at low rates"

Response: Sentence was adjusted.

Line 430: "increasing" → "to increase", "diminishing" → "diminish"

Response: Adjusted as requested.

Line 431: "not only do climate and vegetation…, but so does topography"

Response: Sentence was corrected.

Line 443: delete "setting with"

Response: Deleted.

Line 450: missing right parenthesis at end of sentence

Response: Missing parenthesis added.

Line 458: add "monotonically" since they do increase with MAP at lower MAP values

Response: Suggested correction made.

Line 459: "vegetation" → "vegetation cover"

Response: Missing word added.

Line 459-460: I think this citation needs to be explained more clearly/thoroughly - particularly as it related to trends between chemical weathering rates (and SPRs) and MAP and/or vegetation

Response: Section in question was reworded.

Line 471: I'm not sure "stabilize" sufficiently/properly describes the trend. "increase and then stabilize"

Response: Sentence corrected as suggested.

Line 474: "the low contribution of chemical weathering to total denudation"

Response: Corrected as suggested.

Line 475: "where solute fluxes are high and soils and saprolites are rich in organic acids"

Response: Corrected as suggested.

Line 478: I'd suggest also adding "and non-monotonically"

Response: Suggestion was incorporated.

Line 640: "bin slopes" → "slope bins"

Response: Corrected as suggested.

Line 641: "two-polynomial" → "binomial"

Response: Changes was integrated.

Line 647: 'for different mean annual precipitations and zero soil depth and blue lines are for different MAP and soil depths, assumed to covary (Supplemental Text 2)" Is that correct or is one MAP assumed for the blue lines?

Response: Fixed. Panel B was adjusted to panel A and figure captions adjusted.

Line 652: Pluralize all (or none) of the y-axis variables

Response: Corrected as suggested.

Figure 2 - Optional, but it seems like with both different marker colors and symbols you could also display slope aspect and relative elevation information cited in the text (which might help illustrate the trends you discuss)

Response: Yes – we color coded the symbols by slope.

SUPPLEMENT LINE COMMENTS:

Line 57: delete extra right parenthesis

Response: Deleted.

Eq. S4: EMT → EEMT

Response: Corrected.

Lines 60-61: I'm guessing the S5s and S6s here should be changed to S3 and S4?

Response: Corrected.

Figure S3: What are the dashed lines in A?

Response: these dashed lines are the 95% confidence level. We added this to the figure caption.