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Comment on egusphere-2022-619

Emma Lodes et al.

Author comment on "The story of a summit nucleus: Hillslope boulders and their effect on erosional patterns and landscape morphology in the Chilean Coastal Cordillera" by Emma Lodes et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-619-AC1, 2022

Author Comment – Lodes et al.

Combined response to Referee 1 and 2

The author's text is bold and the referee text is italicised. For separation by color, see the attached file.

Response to Referee 1

We thank the reviewer for taking the time to carefully review our manuscript, and plan on implementing most of the suggestions into our next version of the manuscript. Below, we respond in more detail and explain the changes we have made to the manuscript.

This study leverages existing datasets from relatively well-studied landscapes to examine the role of boulders and fractures on hillslope rates. The authors use measurements of differential erosion of boulders, soils and bedrock outcrops alongside observations of stream orientations to argue that fracturing exerts first-order control on hillslope processes resulting in topographic patterns. Unfortunately, the authors fail to connect the novel contributions of the study (a new framework for assessing differential denudation of boulders and surrounding finer-grained mobile regolith and measurements thereof) to the stated hypotheses and motivation of this work (understanding the role bedrock fractures and resulting grains play in modulating erosion rates). Instead, the task falls to the reader to draw connections and make logical leaps between the particular datasets presented in this study with the broader knowledge gaps the authors address.

We appreciated the open criticism and decided to de-emphasize the fracture part of our manuscript. In particular, we understand that our data does not allow to directly test the influence of fractures on landscape morphology. However, we do want to keep the argument that fracture density patterns can explain the differential erosion rates and landscape morphologies we see in our landscapes. We clarified our logic with regard to this, which is essentially that fractures influence hillslope grain size, and larger grains (boulders) are harder to transport downslope and can reduce erosion rates locally; therefore, areas with wider-spaced fractures on hillslopes can become topographic highs. We edited the introduction to reflect this refocusing, and stated clearly the contribution

that the reviewer suggests this paper offers in line 84-87. We rewrote section 5.3, which discusses the link between bedrock fractures and denudation rates; now we clarify the links between our data sets and refer to existing studies that can help fill the missing logical gaps.

Some of my confusion stems from the figures (those featuring cosmogenic data in particular), which are not designed to easily communicate which variables are being changed (and are thus important for hypothesis testing), which are being held constant (and are not important for the hypothesis), and the results that allow you to reject or accept the hypotheses. I actually ended up using their data table to make my own figures because I found the figures as presented to be largely unhelpful in answering the stated hypotheses. For example, the x axis of Figure 4 is essentially irrelevant, and we really just need to see the erosion rate and differential for each site pair. I recommend major overhaul of these data figures.

Thanks for pointing out some confusion with Figure 4, which we modified for the revised version. The new version of Figure 4 does not have a numerical x-axis, and data from each sampling site is stacked vertically. We also added a new figure (now Fig. 5) that shows fracture density versus ^{10}Be concentration, where the x-axis is continuous (Fig. 5A), fracture spacing vs boulder size (5B), average boulder protrusion height against ^{10}Be concentration with error bars showing the standard deviation of all protrusion height measurements (5C), and average boulder protrusion height against slope angle (5D), and here the points for LC show a trend. We discussed the other figures, but concluded to leave them as they are, with some smaller changes or additions.

The text suffers from similar issues as the reader must accept at face value that data lead to certain conclusions; for example, that the fact that the orientation of channels in the study area match the orientations of mapped faults (which itself is a common phenomenon enough) indicates the control of fractures on hillslope processes, without exploring among other things whether the scale of "fractures" in this sense is the same or how fluvial processes connect back to bedrock fracturing. I have peppered in some literature review suggestions to flesh out where these jumps may be founded in literature, as well as flagged some of these logical leaps that I don't think bear out in previous work.

We thank the reviewer for pointing out the lack in clarity in our explanations and also for suggesting literature which we have now cited in the new version of the manuscript. We expanded the discussion to be clearer about how our observations and results lead to certain conclusions. We added discussion of additional literature that we believe supports our argument and helps the reader make connections between regional faulting, bedrock fracture density and orientation, bedrock surficial sediment size, erosion and weathering, and stream incision in section 5.3.

So, I tried instead to consider the raw data and what it does and does not say, and how the authors might restructure their paper accordingly. What I think I've found is a study that is actually quite interesting and presents a test of what on its face seems like a straightforward answer – if boulders are sticking out on a landscape, they must be eroding more slowly than the surrounding soil…right? The authors elegantly present a new analytic model to demonstrate the resulting differential concentrations of 10Be if their framework is true, in which protrusion height of boulder grows as soil denudation is faster than boulder denudation. The authors then collect 11 boulder-soil pairs (an extra used a nearby pit), and measure concentrations of 10Be and calculate "actual" erosion rates of boulders and soil based on the fact that boulders and soil experience a complex relationship once boulders are exhumed (their new model).

Of the 12 sites, only 7 result in boulders with slower erosion rates than surrounding soils, and (this is where I made my own plots) protrusion height is unrelated to the magnitude of difference between boulders and soils in both the raw 10Be concentrations as well as their modeled differential erosion, which has protrusion built in (and is alluded to in Lines 245-247). (I do have an issue with protrusion values as presented here: surely there is not a single number to the centimeter for a whole boulder? Mean? Max? measured on upslope or downslope a la Glade hogback model? Indeed when plotted, protrusion decreases with increasing hillslope angle suggesting upslope sediment damming/reburial perhaps? This needs to be described in a future iteration).

Great suggestion to describe the measurements of protrusion better. We clarified this in the methods section of the new version of the manuscript (lines 146-152).

The relationship between protrusion and increasing hillslope angle is an interesting thought, especially when combined with the idea of soil pooling from upslope (Glade model). For the LC samples there is a correlation between protrusion height and slope with R^2=0.7.

New plots in (5C and 5D) show boulder protrusion heights plotted against ^{10}Be concentration and slope, and show the standard deviation of protrusion height measurements in the error bars, which hopefully also adds information regarding protrusion height measurement and relationship to other parameters. We discuss the relationships shown in these plots in section 5.1. We also added a discussion of potential pooled soil behind boulders causing them to be shielded in section 5.1.1 (lines 368-370) and 5.1.2 (first paragraph), and have also discussed the potential for boulders to block soil in the methods section (line 152).

Then, as for "the role of boulders and their effect on erosional patterns," based on Figure 7 I see that for SG and LC (I am reinserting the "problematic" datapoints for which boulders are eroding faster than soils that were removed for the LC figure) the catchment-averaged values are on pace with the boulders, meaning only in NA are boulders potentially building relief, but LC and SG are the steep landscapes, so that's confusing.

Thanks for pointing out the confusion about the different sites. It's true that all three sites are different environments and are shaped by different geomorphic processes.

We actually did address the differences between sites in our discussion, but apparently, we did not manage to communicate this clearly enough. SG and (especially) LC are indeed steeper landscapes than NA. In the case of SG, we believe the steeper channels and slopes may be related to the aridity (see section 5.2). In the case of LC, as we described in section 5.1, we believe the boulders with a high erosion rate could have either toppled or been covered by another boulder or soil from upslope. However, none of the LC catchment average samples have a higher ^{10}Be concentration than our LC hillslope samples. Therefore, based on the information we have, we would argue that differential erosion is taking place and relief is indeed building in LC, which is in line with our hypothesis, and in LC there are simply high denudation rates across the board. On the other hand, the catchment average denudation rate from LC is collected from a sub-catchment that does not drain the hillslopes we collected samples for, so it is difficult to directly compare the values. Our best guess for what is happening in LC, is that due to the steeper slopes and the generally higher denudation rates, the erosional regime is more stochastic than the regime in NA or SG, and the effects of toppling and covering of boulders by other boulders or soil are more likely (we also mention this in the discussion).

In the revised version of the manuscript we better highlighted the differences between landscapes and how that is reflected in our results. We split section 5.2 by field area to highlight the differences in geomorphological processes operating in each landscape, and in the conclusions, reiterated that the landscapes have different erosional regimes in the conclusions and more directly addressed the erosional pattern in LC where some boulders erode more rapidly than the surrounding soil.

As far as I know this is the first study to collect data that tried to answer the question of how corestones(? Certainly the authors have illustrated corestones, but I wonder how many are tors based on field photos, and I actually think this distinction is important for the process they describe since some have bedrock "roots" and others do not) are exhumed relative to their surrounding regolith, formulates a model to test this, and the confusing and negative results are actually quite interesting – maybe bigger boulders do not tell us anything about differential erosion, and maybe bigger boulders are not eroding much more slowly than a catchment average. That's your paper!

But then the paper tends to veer into discussion of fracture spacing and density on controls on erosion rate, but it lacks the data to do so. We are told upfront (Lines 122-123) that two of the three sites lack fracture measurements. Then the authors state that they observe increased erosion rates with increased fracture spacing (Lines 212-214) and then a tiny section of Figures 4 and 7 is devoted to showing that 4 of 5 measurements (two of which overlap in uncertainty) increase with "increasing fracture density" (as an annotation; fracture spacing measurements are not presented as a continuous x axis and none at all in Figure 7 so we don't know how different they are). Ostensibly no other portion of the data are related to fracture spacing other than that boulders must come from fractured bedrock and fracture spacing sets maximum boulder size. Rivers following faults is not the same thing as bedrock factures of certain density producing boulders and I cannot make the logical connection the reader is meant to make there. The differential erosion also appears unconnected to fracturing – are we meant to infer that denser fractures lead to smaller protrusion which means less differential erosion (which I don't think we can say with this data)? Yet the introduction and discussion dwell on fractures and their role. I think the role of fracturing is ambiguous at best based on the data presented, and the paper should be re-framed accordingly.

I suggest refocusing paper on differential erosion theory development and related data (which is novel enough that it deserves more discussion and plots), the relationship between boulder, soil and catchment-averaged erosion rates, and the potential influence of fracturing on that story. Then, re-write introduction and hypotheses in particular to reflect this refocusing (all but dropping fracture-versus-erosion-rate angle). The differential erosion story is like a corestone being exhumed through a regolith profile – strong material that will begin to stand out as less-pertinent data and discussions are denuded.

We understand the concern that we exposed the research too much in terms of fracture spacing despite the fact that we do not present much direct data on fracture spacing. We actually did start this research with the hope to collect more data on fracture spacing than we ended up doing. However, we also realized that fracture spacing really is a principal control on hillslope sediment size, together with any further reduction by weathering processes, and we exposed this logic in the Introduction. Nevertheless, we restructured the intro and discussion to de-emphasize fractures, but at the same time, as the reviewer suggests, keep the fractures in the discussion as they are the key element for the hillslope sediment size. In the revised version we now refer to fracture density as a potential explanation for the distribution of bedrock and boulders vs soil in our sites and therefore patterns of differential erosion.

To clarify: It’s not that protrusion height itself impacts differential denudation, but (i) the extent of boulders that cover a surface, versus the extent of soil, as larger boulders can locally reduce hillslope erosion, and (ii) as smaller hillslope sediments are easier to transport, they will likely move down a hillslope faster than larger sediment, causing more rapid hillslope erosion in that location. The connection to fracture density is through grain size, and that fracture spacing has a large influence on grain size in bedrock landscapes. We explain this in section 5.3 with new support from Buss et al. (2013) study in Puerto Rico in which corestone size is correlated with fracture spacing. We also added a short paragraph where we suggest how future studies can more thoroughly measure fracture density in bedrock landscapes where there is soil cover to more thoroughly address the question of how fracture density affects landscape morphology (lines 628-632).

In terms of rivers, the logic should be the same; that fractures affect grain size by dissecting bedrock, and in both settings, larger grains are harder to transport and smaller grains are easier to transport. As explained above, in the revised version of the manuscript we attempted to better explain the logical connection between fractures affecting erosion on hillslopes and fractures affecting stream incision in the introduction and discussion, and added more references.

Since I think the paper needs major refocusing and I’ve addressed the broad concerns above I will focus on line comments that will be most important for the next iteration:

Lines 48-50: Relationship of this work to that of Puerto Rico work should be addressed, starting with: Fletcher, R. C., and S. L. Brantley. "Reduction of bedrock blocks as corestones in the weathering profile: Observations and model." American Journal of Science 310.3 (2010): 131-164. and subsequent publications

Thanks for pointing us toward the work in Puerto Rico; indeed, it is very relevant. In the revised version of the manuscript we have referenced to Fletcher and Brantley (2010), as well as Buss et al. (2013).

Line 50: I actually think it’s pretty important to distinguish between big rocks that are “rooted” in the underlying bedrock and those that are truly corestones that are free to slowly creep downslope, since one denudes purely by little chunks falling off and the other can technically be a mass transported downslope. This is where “erosion rate” is less precise than “residence time” for ambiguously-denuding landscape features (see treatment of boulders and soil in central Pennsylvania in Denn et al 2017 and Del Vecchio et al 2018 and Chilton and Spotila 2020 for a mechanics-heavy investigation, from which I think this paper could benefit).

We agree that it is important to more specifically describe the nature of the boulders we sampled. In the first paragraph of the methods section, we now discuss whether our sampled boulders can be considered corestones or if they are connected to bedrock roots (lines 140-144). In the discussion, in several places, we discuss whether the LC sampled boulders were really in situ. We thank the reviewer for suggesting these articles as they are interesting and relevant, and we have cited them in the revised version of the manuscript.

Line 57: The Granger et al 2001 is relevant to this paper and deserves a little more consideration than this single citation, as do subsequent studies that cite that paper (check https://doi.org/10.1016/j.earscirev.2021.103717)

We agree that Granger et al 2001 is very relevant, and does deserve more attention. In the revised manuscript (introduction section), we acknowledge

their contribution to understand differential erosion (bedrock versus catchment average denudation rates) along with and others (line 71).

Line 60: The Glade and Shobe studies are models of hypothesized relationships between blocky debris and surface processes, and they call field geologists to action to use geochronologic tools to test those hypotheses and to explore the full range of the natural phenomenon; your mileage may vary when it comes to applying these to your own study (see comment for Lines 340-347), so be sure to insert additional literature citations of other field and modeling exercises of blocky debris and surface processes (again https://doi.org/10.1016/j.earscirev.2021.103717).

We agree that there should be more references to field and modeling exercises of blocky debris and surface processes. In the introduction we have added references to Fletcher and Brantley (2010), Buss et al. (2013), Chilton and Spotlia (2020), and Thaler and Covington (2016).

Line 115: As previously stated we need to know the method by which protrusion was determined as it plays a major role in modeled erosion rates and probably varies across a boulder.

We have now added more information regarding how we measured protrusion in the methods section (lines 146-152).

Line 117-128: Very minor but you can drop quotes and just call your sites whatever you'd like, as long as you identify them in a map.

Thanks for the suggestion, we have removed the quotation marks.

Line 167: Does the density difference between production in boulders versus sediment matter when you produce deep 10Be in soil versus boulders? Where is the sediment density for the soil denudation rate?

The density of soil (or saprolite) surrounding exhuming boulders does affect calculated denudation rates, and ideally, our model would include density differences of the material surrounding the boulder as it is exhumed. However, soil depth and density in our field sites are almost certainly variable, and density is also variable. There are soil pits in our field sites, but they are point measurements of the density and depth to the soil-saprolite interface, and those values cannot be extrapolated to our other sampling sites, which are kilometers apart and on different hillslope positions, etc. In addition, no information exists on the material that has already been eroded from around the boulder (this is the material that a density value would apply to). Therefore, we chose to use the same value for density for boulders and soil in our model. That is the approach used by Balco et al. (2011) in their model for corestone exhumation, and we feel that adding complexity that is not fully constrained would ultimately undermine the model.

However, we understand that how we dealt with assigning a value to soil density was unclear in our manuscript, and therefore we have added an explanation for clarity in the methods section (lines 213-219). We also acknowledge the limitation of choosing a uniform material density in section 5.1.2 (lines 400-402).

Lines 223-225: "boulder samples from slope locations have usually lower 10Be concentrations compared to boulder samples from ridge locations, when accounting for their protrusion height as a relative indicator for exposure time." I've been confused by *this because your protrusion is a function of time and rate, no? Protrusion height as relative indicator for time exposed only works if the differential erosion is a consistent differential across the landscape (since a super-fast-eroding soil compared to the boulder would expose a boulder very quickly). As the paper is written I can't tell if we're interested in the how the differential erosion changes across the landscape and if it matters for any interpretation, though.*

We agree that this is a confusing sentence and have changed it to be simpler and clearer (now line 280). We also added more discussion of differential erosion between boulders and soil in the three sites, in sections 5.1 and 5.2.

So I plotted protrusion versus the boulders' 10Be concentration and it's a decent positive correlation (r2=0.5), implying maybe it's more of a time story. Protruding boulders are older boulders. It's easy for me to conceive of this when I make 10Be residence time. I think that the problem I'm having is that the exposure time of the boulder is back-calculated from the difference in modeled erosion rates and protrusion rather than being calculated from the 10Be concentration of the boulder which is a more direct indicator of its exposure time (literally!).

We believe this suggestion makes sense, although when we plot protrusion height vs. ^{10}Be concentration we do not observe such a good correlation. However, we think it would be good to show this relationship, and so we have also added a figure that shows protrusion versus ^{10}Be (5C), and have discussed relationships between protrusion and concentration where multiple boulder protrusion heights are sampled from the same location in section 5.1.2 (lines 412-424).

Lines 340-347: Hard to square the corestone exhumation model (which would exhume boulders) with the Glade hogback model (which buries boulders with upslope soil), and corestone exhumation model as presented does not change rate of upslope soil transport despite that being the primary means by which landscape form is controlled by boulders in the Glade model. Soil pits are only faster than soils around boulders at one of three sites. Methods do not describe that soil was sampled upslope of boulders; cartoon does not indicate slope direction's importance.

As explained above, we have added a discussion of potential pooled soil behind boulders causing them to be shielded in section 5.1.1 (lines 368-370) and 5.1.2 (first paragraph), and have also discussed the potential for boulders to block soil in the methods section (line 152).

We also recognize that our model does not consider the lowering of soil transportation rate when soil is blocked by a hillslope boulder. The blocking of soil by boulders may be reflected in our measured soil concentrations, which are almost all higher than our modelled soil concentrations (see section 5.1.2 and Table 2).

Finally, the reviewer correctly points out that the soil pits only have a faster erosion rate than soil surrounding blocks in NA. We address the relationship between soil pit denudation rates, hillslope denudation rates and catchment average denudation rates in section 5.2, separately for each field site. To address this further, we have added a discussion of how on SG slopes, measured soil concentrations are higher than modelled soil concentrations, which suggests that boulders may be retarding the movement of soil downhill (lines 550-554). For LC, we added that we noticed some soil damming of hillslope boulders in LC, and that in all cases in LC the modelled soil denudation rates are faster than measured soil denudation rates (lines 512-514).

Line 504 – by "data" I presume you also mean the scripts you used to calculate differential erosion and the scripts you used to create your differential erosion figures.

We will include our MATLAB scripts in a data publication that we will publish with GFZ Data Services as soon as this manuscript is accepted.

Response to Referee 2

We thank the reviewer for the in-depth review, and have implemented many of the suggestions into the revised version of our manuscript.

There is much to like about this paper. It is based on a simple study design, comparing granitic landscapes along a climate gradient, where rates of tectonic forcing are low relative to many mountain landscapes. It addresses an important knowledge gap, how local erosion rates may differ in landscapes with mixed soil and rock exposure, and the implications for transient evolution of topography. The authors have produced a valuable data set, with new measurements of 10Be concentration in soils, boulders and bedrock, to complement previous measurements in soil pits and streams in the same landscapes. Much of the presentation is compelling, such as the first two figures showing maps and photos of the sampling locations, and the figure with panels comparing fault and stream orientations. I am sympathetic to the aims of this paper, and expect that it will be suitable for publication after major revisions to address a number of significant weaknesses. I offer the following comments with the hope that they will be useful in improving the manuscript, and hope they are received in the same constructive spirit.

Major comments: Two major weaknesses concern the conceptual model for boulder exhumation and the extensive speculative interpretation largely divorced from both the quantitative data and relevant previous work.

[1] Conceptual model: The conceptual model presented in Figure 3 raised many questions in my mind.

First, implicit in this model is the assumption that at some time in the recent past, these hillslopes were entirely soil mantled, and that something changed causing the soil to drain away, exposing the boulders and bedrock.

What changed, when, and why? Did the climate change in ways that reduced rates of soil production relative to transport? Did baselevel lowering accelerate? Were the nature and timing of the changes similar in all three landscapes? How could the data be used to test the assumption of initial soil cover, rather than simply assuming it must be correct and data that are inconsistent (4 sites) are anomalies and can be excluded. Could events in the past (shifts in climate or uplift) be correlated with the elapsed time implied by differential erosion between rock and soil? What are the durations of the transient implied by the erosion rate differences? How is the conceptual model of transient acceleration of soil removal represented in the quantitative interpretation of the nuclide concentrations (as opposed to a steady state assumption for calculating the nuclide attenuation with depth)? Does this conceptual model also assume that at some time in the future all soil will be removed, and only bare bedrock (and boulders) will be left? Why have these landscapes not already reached that terminal state (i.e. why are they in this current interval between soil and rock end members)? Are there examples (nearby or in other granitic landscapes) that might represent this final state of the transient evolution? How long is the window in time between the initiation of bedrock exposure and the total loss of soil? Do tectonic and climatic boundary conditions remain steady long enough for this to occur? What changes might shift the direction of transient evolution and produce greater *soil cover? What would be the pattern in measured nuclide concentrations in that case? What if this assumption of transient evolution away from a recent complete soil mantle is not correct? How would you know? An alternative (perhaps null) hypothesis would be long-term steady state, in which these landscapes have had a mix of rock and soil at the surface for a much longer time than that implied by the protrusion of the current outcrops. Erosion could be locally variable as core-stones are exhumed in different locations, but the variation could be around a long-term stable average. Indeed, the data from SG may be more consistent with this reference model than the alternative transient model; similar erosion rates for all sample types, and yet boulders protrude... The manuscript will benefit from a more thoughtful consideration of potential explanations for the current state of bedrock and boulder exposure, particularly if the data can be used to quantitatively distinguish among different possibilities.*

Many recent papers investigating weathering and erosion in mixed soil-rock landscapes do not rely on an assumption of transient acceleration of soil export to explain patchy soil cover. For example, two such papers (not cited) are Heimsath et al., 2012 and Benjaram et al., 2022. The conceptual framework would benefit from a firmer grounding in the literature represented by these papers and others cited therein.

We thank the reviewer for bringing up this concern. We did not intend to imply a transient acceleration of soil removal in the conceptual model or in the quantitative model, and in the revised version of the manuscript we attempt to make this clearer. The current spatial distribution of soil, boulders and bedrock on the surface in our field areas is mixed, and therefore in some parts of the landscape, boulders are exhuming, and in other parts, boulders are buried with soil and will be exhumed in the future. The quantitative model calculates one single soil erosion rate per field area that has the same value before the corestone breached the surface, and as the corestone is exhumed. Indeed our hypothesis is the null hypothesis the reviewer describes: that there is a long-term steady state in which these landscapes have had a mix of rock and soil at the surface for a long time, and that erosion is locally variable as corestones are exhumed in different locations, but the variation is around a long term stable average. Because we do not assume a transient shift in soil erosion, we cannot answer many of the questions that the reviewer poses.

However, the reviewer has helped us to recognize that we have failed to present our conceptual model in a sufficiently understandable way, and therefore we have made several changes to the revised version of the manuscript to clarify our conceptual model in the text as well as in Fig. 3.

In the revised version of the manuscript, we have made the following changes to clarify our conceptual model:

Introduction: We clarify that we assume soil has been eroding at a constant rate throughout boulder exhumation at the end of the introduction, where we state our hypothesis (line 93-94). We also clarify that in NA, samples were taken from above knickpoints, where erosion rates have not adjusted to the new uplift rates (field areas section lines 115-120).

Methods section: we added a paragraph that outlines the assumptions of the model, which include that soil erodes at a constant rate throughout boulder exhumation, and that the landscape is in a long term steady state (last paragraph of the methods section). We also describe the potential violations of the model assumptions, which include cases where boulders are not in situ and roll down the hillslope, and cases where boulders are shielded.

Results section: We identified a potentially confusing phrases in section 3.1.3, where the conceptual model is described, and attempted to clarify the phrasing.

Discussion section: At the end of section 5.1.1, we now describe a cycle of boulders being exhumed and then being transported downhill, eventually ending up in streams, to clarify that we expect that a mixed soil, boulder and bedrock cover has persisted in our field sites in the past and will continue to persist in the future. We argue that it may occur at a faster rate in LC due to overall higher denudation rates. In section 5.1.2 (second paragraph), we added a discussion of the relationship between boulder protrusion and concentration. In the following paragraph, we added a paragraph (second-to-last paragraph of section 5.1.2), in which we assess the possibility that climactic changes in Chile have had an effect on our soil denudation rates, referring to existing literature. In section 5.2.2 we also now acknowledge that it is plausible that boulders in LC experience a more rapid cycle of being exhumed and transported downslope.

Table 2 now includes the time needed for each boulder to exhume along with the differential erosion rates between boulders and soils for each boulder sample.

Figure 3: We have revised it to include bedrock and saprolite and to better reflect our conceptual model.

Below we attempt to answer some of the reviewer's questions to the best of our ability:

We do not assume any major environmental change in our landscapes. The length of time it took for our sampled boulders to be exposed to their current (measured) protrusion height varies from ~20-800 ka (see new table 2). That is too short for any major tectonic changes but does encapsulate the last glacial maximum. However, none of our field areas were glaciated during the LGM, and as far as we know there is no consensus in the literature for major erosional changes in our field areas over the last that time period (Carretier et al., 2018). In NA there was a pulse of uplift at 4 Ma that we mention in the introduction (field areas section). However, denudation rates on the NA plateau are on par with pre-4 Ma uplift rates, making it a relict landscape, not yet adjusted to the new uplift rates. Therefore, no significant soil stripping event has happened in recent years.

In order for there to be an evolution toward a bedrock dominated landscape, there would have to be a pulse of uplift where the soil is stripped away faster than the soil production rate. The conceptual model assumes that the landscape is in steady state, but has local changes in erosion rate that are related to the material exhumed (boulders mostly have a slightly lower erosion rate than soil). But then once those boulders are totally exhumed, they can topple down the hill and soil or boulders below are exposed. Differential erosion rates are the highest in LC, and we do observe large boulders sitting in stream channel that drains the sampled hillslopes, that probably rolled down from above. However the boulders are eroding too, and eventually they will reduce in size enough to be exported by the streams. We have no indication of any recent increase in uplift rates except in NA, and as we previously stated, the sample sites in NA are all located in the relict landscape above any knickpoints. On the other hand, soil thickness may increase if the climate became more humid or if uplift rates lowered. Thicker soil could cause corestones to reduce in size more in the weathering zone prior to exhumation. That is shown in the work of Buss et al (2013) in Puerto Rico; where the corestones were only exhumed near an incising stream and otherwise were buried beneath a layer of soil. In a case of thicker soil, the pattern in

measured nuclides would be the same; corestones would accumulate nuclides first at the rate of soil erosion and then once exhumed, at the rate of corestone erosion. Perhaps soil erosion rates would be lower.

If we had taken samples at different heights on individual boulders (like in Heimsath et al., 2000 or in Raab et al., 2019), then maybe we could track any potential changes in soil erosion rate, but we did not sample in this way. Also if we had paired ^{10}Be erosion rates with another nuclide with a shorter half-life like ^{14}C we may have detected temporal changes in soil erosion, but we also did not measure other nuclides. So given the evidence we have there is no reason to believe that our field sites are experiencing an acceleration of soil erosion rates.

Second, the figure and text suggest that soils form deep within boulder-mantled hillslopes, including directly beneath partially exhumed core-stones. A more realistic model would recognize the distinction between soil and saprolite, and include an interface between the two materials at a finite soil depth. The distinction is important for many reasons, including because of the often large density difference as soil production processes dilate disturbed saprolite. It's not clear whether density differences are included in the model presented in this paper for interpreting 10Be concentrations measured in soil and exposed rock. Previous quantitative analyses of core-stone exhumation, such as the seminal work of Fletcher and Brantley, 2010, show that core-stone boulders form within saprolite, not soil.

Hillslopes with a mix of soil mantle and rock exposure typically have shallow soil depths on average, much less deep than implied by Figure 3, with high local variability. For example, Callahan et al., 2020 recently published detailed measurements of subsurface porosity in soils, saprolite and unweathered bedrock in a similar granitic landscape, showing high variability in local weathering and core-stone exhumation.

We understand the reviewer's concern, and agree that a more realistic or complete model would include density differences of the material surrounding the corestone / boulder as it is exhumed. We also agree that soil depth in our field sites is variable, and that density is also variable, and it is true that corestones can also be exhumed from saprolite. There do exist soil pits in our field sites, but they are point measurements of the depth to the soil-saprolite interface, and those values cannot be extrapolated to our other sampling sites, which are kilometers apart and on different hillslope positions, and some sites have no soil cover. Because soil depth and soil / saprolite density are highly variable, and also because no information exists on the material that has already been eroded from around the corestone (this is the material that a density value would apply to), we chose to use the same value for density for boulders and soil in our model. That is the approach used by Balco et al. (2011) in their model for corestone exhumation, and we feel that adding complexity that is not fully constrained would ultimately undermine the model.

However, we understand that how we dealt with assigning a value to density was unclear in our manuscript, and therefore we have added an explanation for clarity in the methods section, where we have also cited Callahan (2020) and acknowledge that both soil and saprolite can surround the boulder when it is still buried. We also edited Fig. 3; now the soil depicted in the figure fades to the lighter brown color of the boulder, which hopefully helps give the reader the impression that soil and saprolite both surround the boulder. We also acknowledge the limitation of choosing a uniform material density in section 5.1.2 (lines 400-402).

In a related paper, Callahan et al., 2022 show that differences in the concentration of *specific minerals within otherwise similar granitic rocks have strong effects on weathering, which feedback on soil production by influencing forest ecosystem productivity. In the extreme case, large expanses of bedrock exposure occur in unglaciated granitic rock that lacks phosphorus, as shown by Hahm et al., 2014. Some of the differences between sites in this study may be due to similar mineralogical effects, a possibility that could be evaluated with thin section or XRF/XRD analyses of bedrock composition. Perhaps such data have already been collected for these sites or the plutons where they are located?*

Yes it's a great idea, but unfortunately we do not have XRF or XRD data for these sites. However, we do plan on conducting such as study in the future.

[2] Speculative interpretations

The admittedly "speculative" hypothesis of a positive feedback between fracturing, weathering, soil development, and topographic evolution shown in Figure 8 and argued for at the NA site is intriguing. However, as much as I might like these ideas, they are not aligned with either the experimental design of this study or the focus on nuclide concentration measurements in the main manuscript. Moreover, these ideas are not entirely novel, and should be grounded in the important previous work not considered here.

We appreciated the open criticism and decided to de-emphasize the fracture part of our manuscript – in accordance with a similar comment by reviewer 1. In particular, we understand that our data does not allow to directly test the influence of fractures on landscape morphology. However, we do want to keep the argument that fracture density patterns can explain the differential erosion rates and landscape morphologies we see in our landscapes. We edited the introduction, hypothesis, and conclusion to reflect this refocusing.

Although hypothesized to be the key independent variable, fracture spacing is not systematically quantified in this study, and is only measured in one of the three sites. Moreover, if I understand it correctly, the Figure 8 idea is that it's not the mean fracture spacing but the high end of the distribution of spacings that corresponds to where weathering is suppressed, bedrock emerges, and topographic highs evolve. The measured spacing would represent this tail of the distribution. Thus, it may be the spread in fracture spacing that matters more than the central tendency. The idea that boulder size can be used to infer fracture spacing is also intriguing, but not well tied to the relevant literature. For example, Verdian et al. 2021 provide the best published evidence for the correspondence between the distribution of fractures and boulder size, but show that the spread in distributions is typically wide requiring a large number of measurements to adequately constrain the tails. In another relevant paper, Marshall and Roering, 2014, document how locally wide fracture spacing prevents trees from rooting, suppressing soil development and creating large bedrock exposures.

Indeed we believe that the measured fracture spacing in bedrock outcrops represents the parts of the landscape where bedrock fracture density is the lowest. We think that bedrock fracture density is highest under the soil mantled parts of the landscapes, but we cannot measure fracture spacing there because the fractures are not exposed. We agree that it is a good idea to consider the full range of fracture spacing measurements. Therefore we have added a new plot to the revised version of the manuscript (Fig. 5B). In this figure we show two overlapping histograms, one with fracture spacing measurements and another with boulder size measurements (since they are still being exhumed, we used the average of the x and y axes of each boulder, if z is the protrusion height).

This simple histogram is made in the spirit of Verdian et al. 2021 and shows that our boulders are generally smaller than fracture spacing. We added a paragraph to the discussion (first paragraph of section 5.3), where we discuss the connection between boulders and fracture spacing, and refer to work of Verdian et al. and the concept of a spectrum between unweathered sediment delineated by fractures on one end and sediment that has been weathered in the weathering zone on the other. Further along in section 5.3 we have added support of other literature, including Buss et al. (2013), a study in Puerto Rico in which corestone size is correlated with fracture spacing.

In addition, we argue that larger boulders may locally suppress erosion and locally affect denudation rates by suppressing erosion of the underlying soil, or potentially causing soil to collect from upslope. In the revised version of the manuscript we have gone into more detail describing the mechanisms by which large boulders can suppress erosion in the discussion section. For example, we have added a discussion of potential pooled soil behind boulders causing them to be shielded in section 5.1.1 (lines 368-370) and 5.1.2 (first paragraph), and have also discussed the potential for boulders to block soil in the methods section (line 152).

The treatment of non-topographic stresses also deserves better grounding in previous work, for example the depiction in Fig 8 of fractures as mostly surface normal conflicts with the common observation of surface parallel fracturing in granitic rocks at summits, as explained mechanistically by Martel, 2011 and developed further by Moon et al., 2017.

We agree that surface parallel fracturing in granitic rocks is a common phenomenon and have edited Fig. 8 (now Fig. 9) to include surface parallel cracks. We now also cite Martel, 2011 in the figure caption.

The speculations that form the climax of the current manuscript could still have a place in a revised discussion, if much more closely tied to previous work, and limited to the context of suggestions for future work. For example, testing this hypothesis might involve measuring fracture spacing much more thoroughly, and documenting the differences in degree of bedrock exposure between ridges and hillslopes across a wide area. The primary conclusions of the paper should be focused more narrowly on the non-speculative, quantitative and (hopefully) repeatable findings of the differences in erosion rates documented with cosmogenic nuclides.

We understand the concern that we exposed the research too much in terms of fracture spacing despite the fact that we do not present much direct data on fracture spacing. We actually did start this research with the hope to collect more data on fracture spacing than we ended up doing. However, we also realized that fracture spacing really is a principal control on hillslope sediment size, together with any further reduction by weathering processes, and we exposed this logic in the Introduction. Nevertheless, we restructured the intro and discussion to de-emphasize fractures, but at the same time keep the fractures in the discussion as they are the key element for the hillslope sediment size. In the revised manuscript we instead refer to fracture density as a potential explanation for the distribution of bedrock and boulders vs soil in our sites and therefore patterns of differential erosion. We also added a short paragraph where we suggest how future studies can more thoroughly measure fracture density in bedrock landscapes where there is soil cover to more thoroughly address the question of how fracture density affects landscape morphology (lines 628-632).

Line-by-line comments

Line 45: This passage describes two end members, with weathering dominated for slow erosion and fracture dominated for fast erosion. Highly relevant for this paper are the observations of Sklar et. al. 2020 which showed that these two end members can coexist and overlap in a single granitic catchment, due to a climate gradient that results from elevation differences.

Good idea, we have now added this comment to the introduction and cited Sklar et al. (2020) – now line 53. We also now cite Verdian et al. (2021) in the introduction and in the discussion (556-573), when referring to the endmember of sediment size that is controlled by bedrock fracture spacing.

Line 50: A tor is technically not 'sediment' but intact bedrock, whereas 'corestone' refers to a type of boulder formed by weathering along fractures and subsequent exhumation.

We agree with this comment and in the revised manuscript do not use the term 'tor.' In addition, we more clearly explain what we mean by hillslope boulders in the newest version of the manuscript. We added more discussion regarding whether our sampled boulders are embedded in the ground in the methods section (line 140-144). In the discussion, in several places, we discuss whether the LC sampled boulders were really in situ.

Line 92: This paragraph lists uplift rates and previously measured denudation rates for the three landscapes, without any helpful interpretation. The NA site is on a relict plateau, eroding at 30 m/Myr, which roughly matches the pre-4 ma uplift rate; now uplift is 10 x faster, but do knickpoints isolate the study site on a plateau? Would be good to clarify. Other two sites uplift is only <0.1 mm/yr, so could be consistent with any denudation rate that's also less than 0.1. Without some guidance for the reader in understanding their relevance, these data could simply be listed in a table.

We do mention knickpoints in NA that record the change in uplift rates, in lines 115-120. As for the other information, one point that comes up later is that denudation rates are generally quite high in LC and higher than one would expect given the estimated uplift rates. We added a note acknowledging that in the revised version of the manuscript.

Line 107: Please clarify: were the sampled chips broken with a hammer and chisel? Or loose already?

The sampled chips were taken using a hammer and chisel; we have now added that to the methods section (now lines 134-136 in the revised manuscript).

Line 169: Would be helpful to plot the nuclide penetration curves for the combined model (equation 1) showing how the attenuation shifts with time. How does the effect of gradual exhumation affect the interpretation of the average nuclide concentration from a boulder surface? Presumably, T2 varies across the boulder, as does the sampling locations?

In theory, T2 could vary slightly across the top of the boulder, but we did not take multiple samples per boulder. We amalgamated sample chips from the tops of many boulders of similar protrusion heights (methods) and took an average protrusion height from all of those boulders combined. In the second paragraph of section 3.1.3, we propose that the average nuclide concentration of the boulder surface would increase at two different rates; during phase 1, while the boulder is buried, at a rate that reflects the rate of soil erosion, and then in phase 2, after the exhuming boulder breaches the surface, at a rate that reflects the rate of boulder erosion. That is the basis of Eq. 1 and Fig. 3. We recognize that we did not fully explain how boulder protrusion was measured and

assessed, and in the newest version of the manuscript, have explained this more clearly in the methods section (lines 146-152).

Line 228: A tiny pet peeve: best to be consistent with significant figures; could there be a missing 0 in the last decimal place in the reported the uncertainty? Also, there is inconsistent precision on the uncertainty, varying from 1 to 3 significant figures. Inconsistency can undermine reader confidence. One solution is that promoted by environmental data analysis guru Jim Kirchner (ETH Zurich), whose famous 'tool kits' include the convention of using 2 significant figures for reporting uncertainty, and then matching the decimal place for the precision of the reported measurement. Here's a link to Jim's website: http://seismo.berkeley.edu/~kirchner/toolkits.html

See Tool Kit 5 for uncertainty quantification, as well as Tool Kit 7 for hypothesis testing (to address issues raised in other comments)

We went through and made sure the significant figures are consistent throughout the revised manuscript. Thanks for the link!

Line 230: Alarm bells ring when the reader is told about the exclusion of four points (where boulders have lower concentration than adjacent soils). It's clearly not an impossible outcome, because you observe it, so perhaps there's a problem with the model not the data. Later, the discussion considers scenarios that would explain this outcome, but a better model would include the possibility from the beginning. Anomalous circumstances may also apply to the data that don't violate the model, but would not be detected by this approach to potential outliers, possibly biasing the interpretation.

We agree that the data points that don't fit with the model are valid data that should be addressed. We also agree that ideally we would adjust the model to fit the range of data we collected. However, in this case it is not possible, because the lower concentrations of boulders in LC are probably due to rolling or shielding, and it's not clear in the beginning if a boulder has toppled in the past or was covered by another one, etc. Nonetheless, we decided to mention these possibilities already in the methods section, where we describe the model. It is also true that anomalous circumstances may apply to the data that don't violate the model; for example, it could be that a boulder was covered by another one, but sufficiently long exposed so that the concentrations are higher than that of the soil. In that case, we may expect that the boulder erosion rates we obtain are maximum estimates. We chose not to change the model, but rather edited the revised version of the manuscript to state more explicitly the assumptions of the model and potential circumstances that violate those assumptions (new paragraph in the methods section). In addition, we state more explicitly that our model does not fit for every landscape; the model fits well for NA and SG and does not entirely fit to LC, potentially because LC has near-critical slope angles where the sampled boulders have a higher chance of rolling or toppling downslope. We now extensively discuss the geomorphic processes that we think may be operating in LC, in section 5.1.1 and 5.2.2. In section 5.2.2 we also now explicitly acknowledge that our model is not necessarily suitable for steeper landscapes like LC, and that boulders in LC likely experience a more rapid cycle of being exhumed and transported downslope. We also split section 5.2 by field area to highlight the differences in geomorphological processes operating in each landscape. In the conclusions, at the end of the first paragraph, we again explicitly state that the model is fits well for NA and SG and not as well for LC, and that boulders in LC do not seem to have the same effect on suppressing erosion as in NA and SG.

Line 233: The "no unique solution" issue should be explained more carefully. Is there a *circularity problem? Do you need to know how much boulders contribute to the soil nuclide concentrations to be able to know how slowly boulders erode? Figure 5 seems to have the answer, but when the figure is first mentioned here (line 235), the text treats it as if the reader already understands it. Take a paragraph or two to lay out the problem, consider possible solutions, present the algorithm, and guide the reader to be able to interpret the figure. As written, this is a bit of a train wreck.*

We understand that our description of Fig. 5 (now Fig. 6) is unclear, and have edited section 4.2 to include more explanation of what parameters the denudation rate ranges are based on. We also now point the reader to section 5.2, where we go into much more depth into which denudation rates are most plausible. We also extensively discuss potential caveats in section 5.1. We lay out the equation and its components in the methods section (3.1.3).

Line 276: Another pet peeve: I suggest avoiding use of the word "true". Truth in science is usually beyond our reach. The best we can do is show that alternative interpretations are less consistent with the data than our preferred interpretation. At the end of the day, these denudation rates are interpretations of the measured concentrations, and rely on many assumptions embedded in conceptual model. Calling them 'true' may undermine the confidence of readers in the rigor of the analysis.

We agree that "true" is not an appropriate word to use, and have taken it out in the revised version of the manuscript.

Line 311, and paragraph beginning on line 325: (This time more than a pet peeve, this goes to a key weakness in the interpretation of the data) The word "likely" implies a quantification of probability, as in a hypothesis test where the 'p' value quantifies the likelihood that the observed pattern could have arisen by random variability. Instead, this paragraph describes a series of somewhat subjective choices of what to believe, without any estimate of likelihood. I suggest using a different term such as "preferred" denudation rates or rates "most consistent with model assumptions". The bigger issue is whether there's a better way to evaluate these data, for example with a hypothesis test where a null hypothesis could stand a chance of being rejected.

We agree that the word likely is inappropriate in certain contexts. We have removed the word likely when it pertains to denudation rates or data, and have replaced it with the word "plausible."

Line 471: the word "tested" is too strong for what was done here. A "test" requires criteria for the hypothesis to fail. Here the data are interpreted as if the hypothesis is already known to be correct (the conceptual model) and data inconsistent are simply excluded. As I've suggested above, a more rigorous effort to quantitatively test a null hypothesis would be a stronger use of the data. Without that, I suggest saying that the study has "explored" the hypothesis.

We have replaced the word "tested" with "explored", also in the introduction, where we introduce our hypothesis. In addition, we added a null hypothesis. Our hypothesis is that boulders erode slower than soil and our null hypothesis is that there is no difference between soil and boulder erosion rates.

Papers cited here but not in the manuscript under review:

Benjaram, S.S., Dixon, J.L. and Wilcox, A.C., 2022. Capturing the complexity of soil evolution: Heterogeneities in rock cover and chemical weathering in Montana's Rocky

Mountains. Geomorphology, 404, p.108186.

Callahan, R.P., Riebe, C.S., Pasquet, S., Ferrier, K.L., Grana, D., Sklar, L.S., Taylor, N.J., Flinchum, B.A., Hayes, J.L., Carr, B.J. and Hartsough, P.C., 2020. Subsurface weathering revealed in hillslopeâ€□integrated porosity distributions. Geophysical Research Letters, 47(15), p.e2020GL088322.

Callahan, R.P., Riebe, C.S., Sklar, L.S., Pasquet, S., Ferrier, K.L., Hahm, W.J., Taylor, N.J., Grana, D., Flinchum, B.A., Hayes, J.L. and Holbrook, W.S., 2022. Forest vulnerability to drought controlled by bedrock composition. Nature Geoscience, 15(9), pp.714-719.

Fletcher, R.C. and Brantley, S.L., 2010. Reduction of bedrock blocks as corestones in the weathering profile: Observations and model. American Journal of Science, 310(3), pp.131-164.

Hahm, W.J., Riebe, C.S., Lukens, C.E. and Araki, S., 2014. Bedrock composition regulates mountain ecosystems and landscape evolution. Proceedings of the National Academy of Sciences, 111(9), pp.3338-3343.

Heimsath, A.M., DiBiase, R.A. and Whipple, K.X., 2012. Soil production limits and the transition to bedrock-dominated landscapes. Nature Geoscience, 5(3), pp.210-214.

Marshall, J.A. and Roering, J.J., 2014. Diagenetic variation in the Oregon Coast Range: Implications for rock strength, soil production, hillslope form, and landscape evolution. Journal of Geophysical Research: Earth Surface, 119(6), pp.1395-1417.

Martel, S.J., 2011. Mechanics of curved surfaces, with application to surfaceâ€□parallel cracks. Geophysical Research Letters, 38(20).

Moon, S., Perron, J.T., Martel, S.J., Holbrook, W.S. and St. Clair, J., 2017. A model of threeâ€□dimensional topographic stresses with implications for bedrock fractures, surface processes, and landscape evolution. Journal of Geophysical Research: Earth Surface, 122(4), pp.823-846.

Sklar, L.S., Riebe, C.S., Genetti, J., Leclere, S. and Lukens, C.E., 2020. Downvalley fining of hillslope sediment in an alpine catchment: implications for downstream fining of sediment flux in mountain rivers. Earth Surface Processes and Landforms, 45(8), pp.1828-1845.

Verdian, J.P., Sklar, L.S., Riebe, C.S. and Moore, J.R., 2021. Sediment size on talus slopes correlates with fracture spacing on bedrock cliffs: implications for predicting initial sediment size distributions on hillslopes. Earth Surface Dynamics, 9(4), pp.1073-1090.

Papers cited in author’s comment not already cited in the referee comment reference list:

Heimsath, A.M., Chappell, J., Dietrich, W.E., Nishiizumi, K. and Finkel, R.C.: Soil production on a retreating escarpment in southeastern Australia, Geology, 28(9), 787-790, https://doi.org/10.1130/0091-7613(2000)28<787:SPOARE>2.0.CO;2, 2000.

Raab, G., Egli, M., Norton, K., Dahms, D., Brandová, D., Christl, M. and Scarciglia, F.: Climate and relief□induced controls on the temporal variability of denudation rates in a granitic upland, Earth Surf. Proc. Land., 44(13), 2570-2586,

https://doi.org/10.1002/esp.4681, 2019.

Balco, G., Purvance, M.D. and Rood, D.H.: Exposure dating of precariously balanced rocks, Quaternary Geochronology, 6(3-4), pp.295-303, 2011.

Buss, H.L., Brantley, S.L., Scatena, F.N., Bazilievskaya, E.A., Blum, A., Schulz, M., Jiménez, R., White, A.F., Rother, G. and Cole, D.: Probing the deep critical zone beneath the Luquillo Experimental Forest, Puerto Rico, Earth Surf. Proc. Land., 38(10), 1170-1186, https://doi.org/10.1002/esp.3409, 2013.

Carretier, S., Tolorza, V., Regard, V., Aguilar, G., Bermúdez, M.A., Martinod, J., Guyot, J-L, Hérail, G., and Riquelme, R.: Review of erosion dynamics along the major N-S climatic gradient in Chile and perspectives, Geomorphology, 300, 45-68, https://doi.org/10.1016/j.geomorph.2017.10.016, 2018.

Please also note the supplement to this comment:
https://egusphere.copernicus.org/preprints/2022/egusphere-2022-619/egusphere-2022-619-AC1-supplement.pdf