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Comment on egusphere-2022-619

Anonymous Referee #2

Referee 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-RC2, 2022

Review comments on

"The story of a summit nucleus: Hillslope boulders and their effect on erosional patterns and landscape morphology in the Chilean Coastal Cordillera"

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.

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.

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?

[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.

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. 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.

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.

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.

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.

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.

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

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?

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)

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.

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.

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.

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.

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.

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.