EGUsphere, referee comment RC1
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Comment on egusphere-2022-619

Anonymous Referee #1

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-RC1, 2022

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.

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.

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.

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).

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.

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.

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

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).

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)

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).

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.

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.

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?

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.

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!).

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.

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