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Interactive comment on "Sediment size on talus slopes correlates with fracture spacing on bedrock cliffs: Implications for predicting initial sediment size distributions on hillslopes" *by* Joseph P. Verdian et al.

Anonymous Referee #2

Received and published: 4 August 2020

Verdian et al., present a timely new dataset that measures bedrock fracture spacing on cliffs and compares the spacing of fractures to the grain size of sediment contributed to downstream hillslopes. The topic is very pertinent. Growing research shows that sediment grain size can impact relationships between topography and sediment transport at a range of spatial scales, and the need is growing for empirical datasets that constrain the initial grain size of sediment produced from fresh rock. This study primarily tackles the 2nd point above by collecting field data from a number of cliff locations previously studied by Moore et al., 2009. I found the connection between bedrock fracture

spacing and talus-sediment grain size convincing and promising, but have suggestions to hopefully strengthen the manuscript's clarity, impact, and context.

After some moderate revisions that address these comments, I would be happy to work with these authors towards publishing this manuscript in esurf.

Review aspects:

Does the paper address relevant scientific questions within the scope of ESurf? Yes. Does the paper present novel concepts, ideas, tools, or data? Yes. New data. Are substantial conclusions reached? Yes. Are the scientific methods and assumptions valid and clearly outlined? Could use clarification Are the results sufficient to support the interpretations and conclusions? Yes Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Generally, yes. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes. Does the title clearly reflect the contents of the paper? Yes. Does the abstract provide a concise and complete summary? Generally, yes. Is the overall presentation well-structured and clear? Yes with suggestions Is the language fluent and precise? Yes but could improve Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? No Are the number and quality of references appropriate? Yes with some additions Is the amount and quality of supplementary material appropriate? Yes

Principle criteria

Scientific significance: good – ideas and concepts have existed and have been explored in other publications, but this study contributes useful new field datasets – particularly fracture spacing measurements paired with talus grain shape and size measurements. New conceptual model introduced in discussion section requires some clarification, connection to hillslope/catchment-scale variability in weathering zone thickness, and specific factors that can be measured in the field.

Scientific quality: good – individual data collection methods are valid, but comparison between datasets and connection to pre-existing work should be clarified. Clarify quantitative methods used to distinguish results and make comparison between datasets. Datasets are very useful, but additional context is likely required to communicate results and interpretation to readers. I would like to see some more detail in the methods section.

Presentation quality: good/fair – Clear statement of testable hypothesis. I recommend adding to the background/study site section to frame tectonic, climatic, and lithologic context. Some organizational suggestions to distinguish results from interpretations (results section vs discussion section). Encouraged to add subheadings to discussion section and results section to improve organization of results and interpretations. Some additional context is needed to communicate the importance of the problem and the utility of the findings.

Line by line edits:

Abstract

Line 19: Before introducing results (median fracture spacing and particle size. . .), add a sentence describing methods used to measure fracture spacing (scan lines) and sediment grain size (a-b-c axis measurements). Potentially this could be incorporated into the previous sentence.

Line 19: Here and throughout the manuscript, consider switching "particle" with grain or clast? I would interpret particle to include wood or other debris that does not originate on cliffs. Just my preference though.

Line 20: Is there a metric you can provide to quantitatively describe the correspondence between fracture spacing and b-axis diameter?

Line 22: "weathering has not modified latent sediment either before" seems confusing

to me. You are measuring bedrock fracture spacing on the cliff faces (which are affected by near-surface weathering to some degree), so you haven't quantified how the latent sediment size changes before reaching the cliff face. It is important to define (possibly in the abstract but definitely in the introduction) at what point in rock exhumation to the surface you're measuring latent sediment size (see later comment)

Line 22: I would remove point about modification of sediment before detachment and focus on point about modification of sediment during detachment: i.e. grain size does not fine significantly during rockfall or clast spallation from the cliff face

Line 24: replace "it" with clasts? - "clasts contain some fractures inherited from bedrock" – is there any field evidence of this and is it possible to present this evidence (even if photographs of deposits)

Line 27-30: Possibly you can shorten these sentences and allow space to expand on your methods earlier in the abstract. Also, it's important to acknowledge landscapes between the two end-members mentioned, as many steep catchments fall into this domain space (some cliffs, but still some soil-mantled hillslopes).

Introduction

Line 33: I might reword "the life of sediment" and remove this opening phrase: (The size distribution of sediment influences chemical, physical, and . . . ect)

Line 36: Replace "The first stage begins" with (Initial sediment grain size is set when clasts are detached from. . . ect..)

Line 38: rephrase?: "The resulting initial size distribution is the starting point for the evolution of the size distribution of sediment on hillslopes, and therefore sets the upper limits on sediment size distribution as sediment is routed through the catchment network" ? – slightly more specific than the life-cycle of sediment

Line 41: good statement of overall problem that this paper is addressing

Line 46: Can you cite this point? Or add a qualifier to the beginning of this sentence: "We hypothesize that. . ." or "The "latent" size distribution of clasts is thought to be set by . . . (add a citation to the Sklar et al., 2017 paper maybe if no others come to mind?)

Line 48: I feel like this paragraph should likely be split into at least 2 if not 3 or 4 paragraphs. Here might be a place to do it.

Some place in this paragraph you need to specify where in the rock exhumation timeline you are considering fractured rock to reflect a latent clast size.

It probably makes sense to define the time to measure "latent" sediment size at the point when "clasts" are at the cliff face (this is where you are measuring fracture spacing), but it's important to note that near-surface processes that fracture bedrock can modify fracture spacing before rock reaches the cliff face surface where your fracture spacing measurements are occurring. The true "latent" clast size reflecting the first interaction between the topographic surface and fractures likely occurs at some depth beneath the cliff face, where stress induced by non-flat topography becomes non-negligible (i.e. Miller and Dunne, 1996 – https://doi.org/10.1029/96JB02531) or temperature fluctuations can fracture rock due to frost cracking (Hales and Roering, 2007 – https://doi.org/10.1029/2006JF000616) or diurnal heating and cooling (harder to get specific depths below surface where this process is important?. . . Collins and Stock, 2016 – rockfall triggered by thermal cycling, or work by Epps: Epps and Keanini, 2017?).

I think it is good to pick cliff surfaces as the surface where "latent" clast sizes are quantified (because this is most possible to measure), but it's important to clarify that modification of clast sizes related to near-surface processes may have potentially started at deeper depths, and the amount of subsurface latent clast size modification before reaching a cliff face might depend on climate, hillslope relief, or tectonic stress orientations.

Line 48: If you describe the point above at the end of the 3rd sentence in the current

paragraph, I think it makes sense to split the next sentence into a new paragraph (describing processes that fracture rock unrelated to the near surface – tectonic fracturing, unloading, and cooling)

Line 54: I would split the sentence starting with "As weathering commences" into a new paragraph that talks about near-surface processes that fracture bedrock. (climate, topographic stress perturbations, deep weathering)

Line 60: New paragraph starting with "Although these hypotheses.." End this new paragraph re-iterating that you are defining the "latent" sediment size as the spacing of fractures on exposed cliff faces?

Line 61-62: I believe this point has been explored by Messenzehl et al., 2018 and Neely and DiBiase, 2020 (this is in preprint.. so I'm not sure if you can cite this contribution yet?, but I see this is cited elsewhere due to relevance). I would reword to say: "the relative importance of latent sizes and weathering in initial size distributions has been explored systematically across a limited suite of climatic and lithologic settings" ?

Line 66-67: maybe re-iterate at the end of this sentence that prior studies focused on a limited range of bedrock lithology and climate variables?

Line 70: It might be important to note the resolution of the fracture spacing surveys in this study (could only resolve fractures with apertures > xxx cm). Your fracture spacing measurements occur at much higher resolution.

Line 73: Same comment as above (could only resolve fractures with apertures > 1-3 cm).

Line 73: change DiBiasi to DiBiase

Line 74: Results from Neely and DiBiase show that latent clast sizes dominate on steep slopes where bedrock cliffs are exposed, but not on gentle soil-mantled slopes.

Line 74: May add reference and background to Attal et al., 2015 (https://doi.org/10.5194/esurf-3-201-2015) and Roda-Boluda et al., 2018 (https://doi.org/10.1002/esp.4281), which look at erosion rate and hillslope morphology controls on sediment grain size. These studies do not quantify bedrock fracture spacing, but their results imply a link between weathering on soil mantled slopes and sediment grain size.

Line 76: I like contrasting the Messenzehl findings with the prior findings from California landscapes. Maybe move the following sentence (lines 78-79) up to the end of this paragraph to state how your study fits in with these prior investigations?

Line 81-82: change "initial sizes" and "latent sizes" to "talus clast sizes" and "latent clast sizes" ?

Line 85: Also, would mention that clast detachment could occur along pre-existing fractures that are below your detection limit. The small-aperture fractures may also be pre-existing.

Line 87: Another reason talus grain size may not match fracture spacing is if talus sediment is sorted after detachment. This is mentioned later in the manuscript, but should be mentioned here as well.

Line 87: new paragraph at "Neither of these alternatives"?

Methods

Line 98: Before the methods section, I think readers need a "study site" section that describes the various tectonic, climatic, and lithologic settings of each site. (Analyzing cliff/talus systems across these different variables is a main strength of the paper!). This section should at a minimum:

- Detail the location and tectonic setting/history of the outcrops (how does this connect to the inherited bedrock fracture network? (maybe add a map figure to show this too).. are some outcrops closer to active or inactive (– dead) faults?

- Details the climate and climate history of outcrops – relation to frost-cracking? diurnal heating and cooling? Some of this information is in table 1, but the climate history may also be important.

- Introduces the prior work used from Moore et al., 2009 (how did Moore et al., 2009 estimate cliff retreat rates used in later parts of the manuscript?)

Line 103: Include a source after the statement that says three sites where differences in lithology correspond to differences in average fracture spacing?

Lines 108 – 120: Scan lines have been used to measure bedrock fracture spacing in a number of applications, and it would probably be good to cite some of the studies that developed/used these methodologies.

Lines 108-120: One of my main critiques of this study is that the comparison is not straightforward between sediment grain size along an A-B-C axis and fracture spacing on a scanline. This needs to be stated clearly, because this impacts how to interpret results presented from the study.

Scanlines the way they are described (to my knowledge in this manuscript) do not usually run through the longest or shortest axis of a fracture-bound block on a cliff. A horizontal scanline will likely be skew across the fracture-bound block unless all fractures are perfectly vertical. The spacing between the fractures on the horizontal scanline is usually not the A-B or C axis of the fracture-bound block. See Figure 2 in the manuscript, lower right.

While the scanline fracture spacing is still a useful measure of fracture spacing, I'm not surprised that the B-axis of the clasts is larger than the scanline-fracture spacing in the results, because the scanline-fracture spacing likely does not sample the B-axis of the clast (scanline crosses corners of clasts and does not go along the widest or shortest axis).

For example, the Neely and DiBiase study in preprint measured fracture spacing as the short-axis of fracture-bound blocks on orthophotos of cliff faces. This assumes that the short-axis of fracture-bound blocks on the cliffs represents the B-axis. Though this isn't true for "latent" particles where the visible short-axis of the fracture-bound block is actually the C-axis, measurement of the short axis of the fracture-bound block might be more straightforward to map to the B-axis of a detached clast (see figure attached).

The difference between these methods needs to be clarified for the reader here, and this should be mentioned when interpreting the results (unless I'm missing something about how fracture spacing was measured)?

(See attached figure)

If scaled photographs exist for part or all of the scanlines (i.e. figure 2 lower left), another option would be to measure the short and long axis of clasts along the scanline on the photographs (with a photo resolution limit of 2mm) (i.e. attached figure right panel). This could be compared to the scanline fracture spacing and the A-B-C axis of the clasts in the talus piles. This could be a useful comparison for future studies connecting fracture spacing to geomorphic processes or sediment grain size.

Line 124: Slope-parallel transects result in 300 clasts measured per talus pile? Correct? Maybe state this if true?

Line 124: again I'm preferential to saying clasts instead of particles (because particles may include wood or really anything that doesn't necessarily come from the cliffs)

Line 128: "everything else" – I'd replace this with "larger clasts"

Line 129: to be clear, "particles" are "talus cone clasts"? Also I'm confused, if the fracture spacing resolution limit was 2 mm, how do you compare talus clasts <2 mm to the "latent" clast size, which cuts off at a detection limit of 2 mm (presumably) ?

Lines 134-139: I agree with this section, but how do you know that the talus cone captures all of the sediment grain size distribution spalled from the cliff? . . . the largest clasts roll the furthest, do some of these traverse the whole talus pile? If the talus pile

is mined by an active stream at its base, the largest clasts may be somewhere else downstream and the talus pile undersamples the coarsest clasts.

It's been shown in a couple studies/settings that the coarsest sediment grain size distribution is typically found at the base of colluvial/headwater channels in steep landscapes: Hack 1965 https://pubs.usgs.gov/pp/0484/report.pdf), Brummer and Montgomery, 2003, https://doi.org/10.1029/2003WR001981 , and Neely and DiBiase preprint figure 9 https://www.essoar.org/pdfjs/10.1002/essoar.10502617.2

It might be important to note this limitation when comparing bedrock fracture spacing to the grain size of sediment on talus cones. But maybe these talus piles act as better 'clast traps' than angle of repose talus cones...

Also, it looks like the bottom two sites in figure 1 have roads at the bottom of them? I'm not sure if large clasts can travel far enough from the cliffs to reach these roads, but these clasts would likely be cleared by road crews?

Line 140-145: These methods are mentioned, but I don't see the results presented anywhere. Or the methods detailing how these results are integrated into the full distributions are missing? Does this particle size distribution replace the fine tail of the talus grain size distribution that is below the resolution limit of the fractures (2 mm?). Do you quantify fracture spacings finer than 2 mm? even though this is the detection limit for the fractures?

Lines 147 – 151: This section seems out of place. Maybe move this to be near the other paragraphs that describe how you measure sediment grain size (1st paragraph in this section?)

Methods section suggestions overall:

- Need to clarify difference between scanline fracture spacing and a-b-c axis measurement of clast/fracture-bound block

- A number of statistical techniques are later mentioned in the results section. It would help me to introduce these techniques in the methods section and explain how these techniques are used to quantitatively distinguish between the different distributions and test the hypothesis of the paper?

- Detail how the “fines” are accounted for. Where are the bulk sample sediment results presented? How are these included into the distribution? Is the same thing performed with the fracture spacings?

- Note that talus cones may not capture the largest grains if the base is actively mined by a steep and competent stream (or a road maybe?.. not sure)

Results

Line 155: “c-axis diameters as small as 2 mm” – what about the bulk sediment samples (fines?)

Line 161: cumulative empirical distribution function is not a jargon-y term? May be helpful to define in the methods section?

Lines 159-168: A lot of this comparison is qualitative. I see more quantitative comparison in the next paragraph, but I feel unprepared to understand this comparison because the methods section did not introduce how the quantitative comparison would occur.

Line 167: I would like to see the discussion section return to the anomalous result from MT-39 – do the clasts here have many fractures still retained ? – if the fracture spacing is smaller than the c-axis even?

Line 173: I’m not quite sure how these “p” values were calculated or what they mean. I see some comparison to a 1:1 correlation and I think I know how you could do this, but in the methods section, could this be clarified and related to how you are testing your hypothesis?

Line 178 – 181: this is an interpretation and could be moved into a discussion section?

– I am a fan of splitting the results and discussion sections.

Line 185-186: Would be helpful to clarify how p-values are calculated in methods section

Lines 193-194: Would be helpful to clarify in the methods section how the Weibull distribution is populated/used to compare with your data? I find this hard to interpret without some background on the methods.

Line 194-195: Change “the degree to which the data follow a Weibull distribution at each site is illustrated in Figure 5” to a figure call to figure 5 at the end of the previous sentence “(Fig. 5)”

Lines 190-210: Be clear to specify that “A,B,C-axis” distributions come from talus sediment and not from fracture-bound blocks on cliffs. It’s not clear to me how the Weibull distributions are quantitatively linked to the results presented? Most of the comparison seems qualitative at this point? Is there a way to quantify this comparison like with the p-values? If so this should be reported and introduced in the methods section.

Lines 200 and 202: “In some cases” – is there anything specific about these cases? Should these be discussed more in the discussion section?

Lines 217: reword? “Within each rock type, there is little site-to-site variability in mean particle shape. When sites are grouped by rock type, we find . . ..”

Line 220: how much lower are the mean b:a, c:a ratios?

Line 221-223: this is an interpretation and could be moved to a discussion section?

Lines 229 – 234: this point also reads like a discussion point (contextualizing with prior work) – reorganize?

Discussion:

Overall – I had a hard time following parts of the discussion. Potentially, points should be organized into subheadings?

- Significance/reliability of comparison between scan line fracture spacing and sediment grain size in talus piles?

- Comparison with observations from other landscapes where bedrock fracture spacing was quantified?

- Reasons for differences between fracture spacing and sediment grain size? (sorting, kinetic sieving, incomplete breaking along fracture planes, measurement resolution?)

- Climate/weathering controls on initial sediment grain size?

- Conceptual model (with some revisions/clarification?)

Line 237: "nearly the full network of fractures" .. Rephrase: "from the network of fractures with apertures >2 mm"?

Lines 237-238: "This finding" and "it" are hard to unpack in this sentence. I'm having a hard time interpreting this sentence.

Lines 241 – 247: This might be a good place to return to the discussion about the difference between scan-line fracture spacing and the long and short axis of a fracture-bound block. Note that Neely and DiBiase (check spelling) uses the short axis of fracture-bound blocks, which may account for the difference between these study results.

Line 249: See above comment, also Neely and DiBiase –preprint - and Sklar et al., 2020 also have coarser fracture detection limits? (1-3cm in Neely and DiBiase (typically 2 cm) and not sure about Sklar et al., 2020 because a variety of techniques are used – aerial imagery likely has a coarser detection limit though?). The fracture spacing will increase with coarser fracture detection limits – but maybe fracture scaling relationships could be used to compare between these studies – probably a non-trivial endeavor (see Hooker et al., 2014 and similar work that quantifies the distribution of fracture

apertures in rock masses https://doi.org/10.1130/B30945.1) Either way, I would make sure to acknowledge the difference in technique used to measure fracture spacing and the difference in minimum resolvable fracture aperture when comparing these results to results from these other two studies.

Lines 251-252: Not sure where these results are presented??

Line 257: are there field photographs of this? These would be useful to show if even qualitative?

Lines 261-265: this reads more like a results section. I think it would be useful to incorporate the climate data comparison with fracture spacing/ talus grain size in some sort of figure? Plot mean annual temp & precip. Vs median fracture spacing/talus B-axis ? Same thing with lithology vs median fracture spacing/talus B-axis? This would show any correlation or lack of correlation between these variables and fining the sediment after it spalls off of the cliffs? Again, I think the study sites ranging across various climates and lithologies is a strength of this paper and should be used/analyzed! – though some more analysis might need to be done to correctly synthesize these additional parameters/forcings.

Lines 269-272: Background on the analysis by Moore et al., 2009 should be introduced in the beginning of the manuscript. Also, I had a hard time following the organization of this paragraph. Maybe split paragraph into discussion of cliff-dominated systems Tr $\sim$0, and soil-mantled systems Tr » Tp?

Lines 275-276: Add reference to Attal et al., 2015 and Roda-Boluda et al., 2018

Line 276: a bit unclear what is meant by "sites", yes if the site is only a talus and cliff system, the weathering zone is very thin or only along fractures and Tr = 0, but there is a problem if you scale this up to a full catchment (which is relevant for landscape evolution), full catchments typically include a mix of bare-bedrock and soil-mantled hillslopes (see DiBiase et al., 2012 https://doi.org/10.1002/esp.3205 ; Milodowski et al., 2015 www.earth-surf-dynam-discuss.net/3/371/2015/doi:10.5194/esurfd-3-371-2015 ; Neely et al., 2019 https://doi.org/10.1016/j.epsl.2019.06.011)

Line 276: I'm struggling to understand the spatial scale over which Tr ∼0. At the catchment scale, catchments are rarely 100% bare bedrock (there's still some soil). At what point does the catchment system switch from Tr = 0 to Tr » Tp?

Lines 279 – 311: I think it would be useful to define the spatial scale over which these timescales (Tr and Tp) are being assessed.

Consider breaking this up into paragraphs that address this issue at the cliff/hillslope scale, and then the catchment scale (which mixes sediment from cliffs and soil-mantled hillslopes)?

At the scale of a single cliff or hillslope, I think this ratio/analysis may be helpful, but at the scale of a steep catchment with cliffs, usually hillslopes are a mix of soil-mantled slopes, talus slopes, and bare-bedrock cliffs. It becomes challenging to define 'H' in these settings or ensure that 'E' is the same for soil-mantled slopes and cliffs. (See Neely et al., 2019 https://doi.org/10.1016/j.epsl.2019.06.011 and Neely and DiBiase preprint https://doi.org/10.1002/essoar.10502617.2 and Sklar et al., 2020 https://doi.org/10.1002/esp.4849)

I do think this is a useful discussion point to link the study to past work, but maybe this should be reframed?

- i.e. at the scale of exposed cliffs and downslope talus, Tr « Tp and sediment grain size reflects latent clast sizes. The emergence of cliffs in steep landscapes then implies a larger supply of sediment with grain sizes that mirror bedrock fracture spacing?

Also, it's not clear to me how long the sediment in the talus cones remains in the talus cones? Tr could be very long on the talus cones, but lack of biota, water retention, and soil could lower the efficacy of weathering on these cones. So the sediment is detached, sits in the weathering zone as mobile regolith, but there is little fining occur-

ring?

For example, post-glacial rock piles and boulder fields may not develop soil for tens to hundreds of thousands of years despite being in temperate climates on low slopes (see Denn et al., 2017 https://par.nsf.gov/servlets/purl/10098651)

In the boulder field setting, Tr » Tp, but the latent particle size still dominates? – maybe this is a special case, but relict boulder fields are common in post-glacial landscapes such as the ones maybe studied by Moore et al., 2009??

Line 305: transition to patchy soil cover in these landscapes may be better described by DiBiase et al., 2012 https://doi.org/10.1002/esp.3205 , Heimsath et al., 2012 http://www.nature.com/doifinder/10.1038/ngeo1380 , and Neely et al., 2019 https://doi.org/10.1016/j.epsl.2019.06.011

Line 305: not sure where these patchy-soil cover landscapes fit in the context of figure 7, because some of the hillslopes within these landscapes are Fig. 7a, and some are Fig. 7b. Also change "DiBiasi" to "DiBiase"

Line 306-307: I'm not sure about this point with the difference in fracture detection resolution and fracture spacing measurement technique (scanline vs short-axis between fracture bound block).

Lines 308-311: I think this point has been demonstrated in other analyses: Attal et al., 2015; Roda-Boluda et al., 2018; Neely and DiBiase preprint. The data from this study does not really show much soil weathering (unless some data is replot to show sediment fining in the talus piles due to climate variables??), so maybe this point should be rephrased, cited, or removed?

Line 483: why 13000? Is there are source/data suggesting this is when deglaciation occurred near these cliffs? Should this number be the same for each cliff?

Figure 1: I think it would be useful to expand the study map figure into its own figure (new figure 1) and show the geologic context (bedrock lithology and distribution of

active and inactive faults). Can you add a scale bar to each panel? I'm concerned about the road in the bottom two panels, and I'm not sure where in panel CB-1 and MT-39 the measurements occurred (highlight the transects a bit?)

Figure 2: add a scale bar to bottom right image? Could use this figure to illustrate how the scan-line fracture spacings were measured?

Figure 4: add p-values referenced in text that compare to the 1:1 line to panels?

Figure 5: mark resolution detection limits on x-axis of panels?

Figure 6: I find the C:A notation a bit confusing and prefer C/A, B/A, A-B/A-C notation (just my preference though). Nice figure!

Figure 7: I'm a bit confused about the scale/dimensions. Make sure to have (A) (B) and (C) markers on panels?

I'm not sure about the assumption that the erosion rate and slope are the same despite different fracture spacing or weathering zone thickness (in caption and in discussion section).

For example, soil transport rate for a given slope may change in response to different clast size (larger blocks require steeper slopes to move at same erosion rate – see Neely et al., 2019 https://doi.org/10.1016/j.epsl.2019.06.011 , DiBiase et al., 2018 https://doi.org/10.1016/j.epsl.2018.10.005) If climate changes drive difference in weathering zone thickness, that may also change the hillslope erosion rate – see Owen et al., 2011 https://doi.org/10.1002/esp.2083)

I'm not sure the assumptions in this conceptual model are realistic because weathering zone thickness is often coupled to hillslope erosion rate, fracture spacing, and climatic/ecologic factors?

— These are a lot of edits! But I think most of these items can be addressed or clarified? The dataset is a valuable foundation, and I think with some more context and

specific language surrounding the methods, this will be a very useful study that can be pulled into a lot of future research! Hopefully this isn't discouraging or too intimidating.

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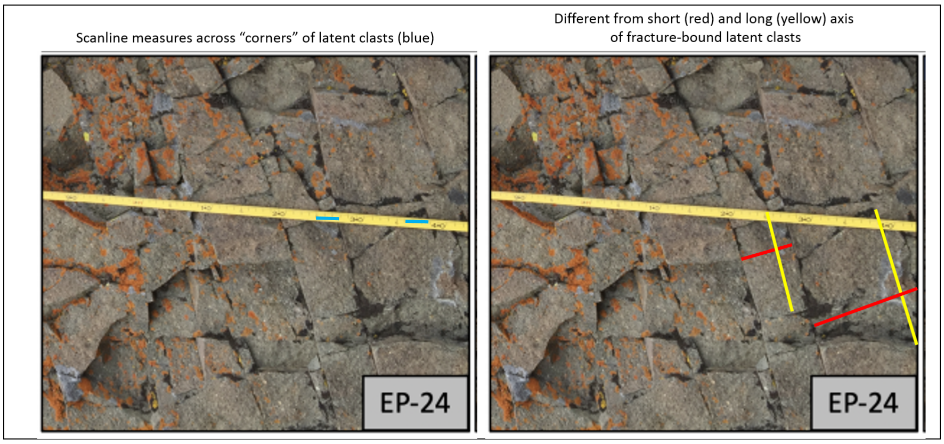
Scanline measures across "corners" of latent clasts (blue)
Different from short (red) and long (yellow) axis
of fracture-bound latent clasts
EP-24
EP-24

Fig. 1.