

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.

Joseph P. Verdian et al.

leonard@sfsu.edu

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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

Referee Comment (overview) Verdian et al., present a timely new dataset that measures bedrock fracture spacing on cliffs and compares the spacing of fractures to the

C1

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

C2

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.

Author Answer: We are grateful to Referee #2 for these thoughtful, thorough, and constructive comments. They are very helpful for improving the clarity and rigor of the manuscript. We have considered every comment carefully, and made changes to address each suggestion and concern as appropriate. In responding to many of the comments below, we make reference to our responses to comments by Referee #1; we do not repeat all of those responses in full here for the sake of brevity.

Line by line edits:

Abstract

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Referee Comment 1: 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.

Author Answer: Good suggestion.

Changes to the manuscript: We revised the abstract to include more detail on methods.

Referee Comment 2: 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.

Author Answer: We did consider this suggestion. Clast is often used to refer to particles lithified within a sedimentary rock, just as grain is often used to refer to individual crystals within a crystalline rock. We prefer the more generic particle, and make clear that we are referring to sediment particles which are fragments of rock.

Changes to the manuscript: We did not make changes in response to this comment.

Referee Comment 3: Line 20: Is there a metric you can provide to quantitatively describe the correspondence between fracture spacing and b-axis diameter?

Author Answer: If there were more words allowed in the abstract, we would include more of the detailed quantitative results. However, the finding that median b-axis is systematically larger than median fracture spacing is an important result.

Changes to the manuscript: We have revised the abstract to include this result.

Referee Comment 4: 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

C4

what point in rock exhumation to the surface you're measuring latent sediment size (see later comment)

Author Answer: We agree that our original presentation of the notion of a latent sediment size was confusing.

Changes to the manuscript: As detailed in the responses to Referee #1, we have made substantial revisions to address this issue, to the introduction as well as the abstract.

Referee Comment 5: 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

Author Answer: Agreed.

Changes to the manuscript. We have revised the abstract as suggested.

Referee Comment 6: 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)

Author answer: There is field evidence, as noted in our responses to Referee #1.

Changes to the manuscript: We have added a field photo to figure 2 showing an example.

Referee Comment 7: 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).

Author Answer: Agreed.

C5

Changes to the manuscript. We have revised the abstract as suggested.

Introduction

Referee Comment (Set) 8: 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.

Author Answer: These are all thoughtful suggested edits.

Changes to the manuscript: We have revised the introduction substantially, and taken these suggestions into account.

Referee Comment 9: 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 in-

C6

duced 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.

Author Answer: These same issues were raised by Referee #1. Please see our reply to their comment 3.

Changes to the manuscript: We have revised the introduction to clarify our definition of latent size accordingly.

Referee Comment (Set) 10: 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?

Author Answer: These are all reasonable ideas for copy-editing the original manuscript.

C7

Changes to the manuscript: We have taken these suggestions into account in the revision of the introduction.

Referee Comment (Set) 11: 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?

Author Answer: We agree with the goal of these suggestions of describing previous work accurately. We did not mean to imply that previous work was not systematic.

Changes to the manuscript In the revised introduction, we now contrast our study with the limited range of lithologies and weathering environments studied in previous work.

Referee Comment 12: 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.

Author Answer: This comment regarding Sklar et al. (2020) is only true of the fractures measured from air photos. That paper also reports fractures measured in situ with similar resolution to those reported here. (Note that we did not measure fracture opening width or aperture, rather we measured the length of rock between successive fractures, many of which were closed, with submillimeter aperture.)

Changes to the manuscript: We did not make changes in response to this comment.

Referee Comment 13: Line 73: Same comment as above (could only resolve fractures with apertures > 1-3 cm).

Author Answer: The issue of resolution isn’t essential to this introductory review of the results of previous work. It is more relevant in the methods section.

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Changes to the manuscript: We have revised the relevant passage in the methods section to also include note of the resolution of fracture detection in previous work.

Referee Comment 14: Line 73: change DiBiasi to DiBiase

Author Answer: Thank you for catching this misspelling.

Changes to the manuscript. DiBiase is now spelled correctly throughout.

Referee Comment 15: 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.

Author Answer: Agreed.

Changes to the manuscript. Sentence revised as suggested.

Referee Comment 16: 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.

Author answer: Both of these references are relevant to the discussion where we consider the role of erosion rate in regulating residence time and thus the degree of influence of weathering.

Changes to the manuscript: In the revised manuscript we cite both of these previous studies in the discussion section.

Referee Comment 17: 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?

C9

Author Answer: Thank you for this suggested edit.

Changes to the manuscript: We have revised this section of the introduction, as detailed in responses to Referee #1.

Referee Comment 18: Line 81-82: change “initial sizes” and “latent sizes” to “talus clast sizes” and “latent clast sizes” ?

Author Answer: As noted above, we prefer not to use the term “clast” in this way.

Changes to the manuscript: We have not made changes in response to this comment.

Referee Comment 19: 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.

Author Answer: This is more of a methodological issue. These alternative hypotheses are not method dependent.

Changes to the manuscript: We have not made changes in response to this comment.

Referee Comment 20: 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.

Author Answer: Agreed.

Changes to the manuscript: Passage revised as suggested.

Referee Comment 17: Line 87: new paragraph at “Neither of these alternatives”?

Author Answer: Agreed.

Changes to the manuscript: New paragraph begun as suggested.

Methods

Referee Comment (Set)18: Line 98: Before the methods section, I think readers need

C10

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?

Author Answer: Agreed, these are helpful suggestions.

Changes to the manuscript We have added a new sub-section at the beginning of the methods section on study sites that describes the tectonic, lithologic and climatic settings of each site, with relevant citations.

Referee Comment 19 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.

Author answer: Agreed.

Changes to the manuscript. We have added citations to two additional prior studies that used the scan-line technique to measure bedrock fracture spacing

Referee Comment (Set) 20 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

C11

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.

Author Answer: We agree that the scanline method is an imprecise approach to characterizing the dimensions of any given fracture-block exposed on a cliff face. Analysis of scaled photos, as done by Neely and DiBiase (2020), permits more precise measurements, at least for large blocks, although there remains the question of which axes are being measured. And, as we discuss in our response to comment 6 by Referee

C12

#1, non-orthogonal joint sets may create fracture-bound blocks that do not present any of the three primary axes to the viewer. "In the most general case, blocks are formed by three intersecting joint sets with non-perpendicular orientations, and thus are not rectangular prisms. In this case, the inter-fracture distance measured along any given scan line crossing a block could possibly range from near zero (near the tip of an acute-angled point) to greater than the a-axis length (for example if spanning the longest possible linear distance across a rectangular block face). An essential assumption in using the scan-line technique is that this variability can be overcome by a large sample size, resulting in an accurate if imprecise estimate of the central tendency and spread in the underlying population of fracture spacings. However, biased estimates can result at any single site, for the reasons outlined above." Additional analysis using scaled photographs is beyond the scope of this paper. However, we are confident that our large sample sizes are sufficient to test the null hypothesis that the distributions of fracture spacing and talus particle size are correlated across our sites.

Changes to the manuscript In both the methods and discussion sections, we have added text to clarify the differences between in-situ scanline and scaled photo methods and the potential sources of bias in comparisons of scan-line derived fracture spacings and talus b-axis diameters.

Referee Comment 21: Line 124: Slope-parallel transects result in 300 clasts measured per talus pile? Correct? Maybe state this if true?

Author Answer: Our method resulted in measurements of 100 talus particles per talus pile. Evidently, our original text describing the method (sum the three tape lengths and divide by 100 to obtain the spacing between samples) did not make this outcome clear.

Changes to the manuscript: We have revised this passage to state explicitly that, for each cliff-talus pair, 100 talus particles were measured in total, summed across the three transects.

Referee Comment 22: Line 124: again I'm preferential to saying clasts instead of par-

C13

ticles (because particles may include wood or really anything that doesn't necessarily come from the cliffs)

Author Answer: We appreciate the referee's preference for the term clast, but are not concerned that readers will think we're measuring pieces of wood if we continue to use the term particle.

Changes to the manuscript: We have not made any changes in response to this comment.

Referee Comment 23: Line 128: "everything else" – I'd replace this with "larger clasts"

Author Answer: Agreed, "everything else" is imprecise.

Changes to the manuscript: Sentence changed to "larger particles"

Referee Comment 24: 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) ?

Author Answer: The point here is that there is no benefit to measuring particle diameters less than 2mm because there are no fracture spacings to compare to. The comparison of the <2mm fine tail of the distributions thus involves a possibly non-zero fraction of the talus size distribution versus zero in the fracture spacing distribution.

Changes to the manuscript: This sentence has been revised for clarity.

Referee Comment (Set) 25: 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

C14

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?

Author Answer: At the Sierra Nevada sites, and the Grizzly Peak site, there are no streams or other sediment sinks at the base of the talus slopes. These talus slopes have clearly defined downslope end points to the depositional feature. This does not mean that some especially large and energetic particles may have traveled past the end of the slope and come to rest far beyond the end of our measurement transect. Likewise, at the Twin Peaks sites, where there is a road adjacent to the toes of the talus slopes, some large particles may have been lost from the deposit. In these cases, it's possible that we have under-sampled the coarse tail of the distribution. This would not affect the estimate of the median size, and would likely have minimal impact on the distribution spread.

Changes to the manuscript We have included mention of this issue in the expanded site description sub-section.

Referee Comment 26: 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?

C15

Author Answer: The outcome of this sampling was reported in the discussion section of the original manuscript where we discuss possible explanations for the coarse offset of the median b-axis diameters from the median fracture spacing. We were not able to obtain subsurface samples at every site so we did not combine those measurements with the surface point counts. However, we used them as a possible indicator of vertical sorting which might explain the b-axis offset. As described on line 255 of the original manuscript, the surface and subsurface distributions overlap sufficiently and thus do not indicate a truncation of the fine tail of the surface talus size distribution due to vertical sorting processes. Because our analysis of the subsurface samples is limited to addressing this question, we do not use those data to make any comparisons with the fracture spacing distribution.

Changes to the Manuscript: We have rewritten the relevant passages to provide a clearer explanation of this analysis.

Referee Comment 27 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?)

Author Answer: The first three paragraphs of this section all concern the methods employed at the Sierra Nevada sites. The last paragraph describes methods employed at the Bay Area sites.

Changes to the Manuscript: We have edited the text to make it clear that the first three paragraphs all apply specifically to the Sierra Nevada sites.

Referee Comment (Set) 28: 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"

C16

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)

Author Answer: Thank you for this concise summary. We have addressed these three of these four points in our responses to comment numbers 20, 25, and 26. We address the question of whether to include information about how we analyzed the data in the methods, or keep it in the results, in our responses to comments 30 and 31 below.

Changes to the Manuscript: See responses to comments 20, 25, 26, 30 and 31.

Results

Referee Comment 29: Line 155: "c-axis diameters as small as 2 mm" – what about the bulk sediment samples (fines?)

Author Answer: See response to comment 26

Changes to the Manuscript: None.

Referee Comment 30: Line 161: cumulative empirical distribution function is not a jargon-y term? May be helpful to define in the methods section?

Author Answer: We could have referred to it as simply as cumulative distribution, or used the acronyms CDF or EDF but those would be imprecise. We chose to use the complete and correct name to avoid any confusion. Readers unfamiliar with this and other standard statistical quantities can easily look them up (e.g. https://en.wikipedia.org/wiki/Empirical_distribution_function).

Changes to the Manuscript: We did not make any changes in response to this comment.

Referee Comment 31: 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

C17

this comparison because the methods section did not introduce how the quantitative comparison would occur.

Author Answer: A common convention in much scientific literature is to use the methods section to describe how the data are obtained, and to report how the data are analyzed in the results section. We prefer to use this approach, rather than introduce the data analysis techniques in a generic way in the methods, without the data in hand to understand why those techniques might be used. In addition, data analysis choices often flow from the results of preceding analyses, which cannot be explained in a methods section before the reader has seen those preceding results. The challenge of course, is to explain the data analysis clearly and this comment, along with others by Referee #1 indicate that the original manuscript did not.

Changes to the Manuscript: As described in our response to Referee #1, comment 6, we have substantially expanded the explanations of the choices and techniques used in analyzing the data.

Referee Comment 32 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?

Author Answer: Excellent suggestion.

Changes to the Manuscript: We have added a photo from MT39 to Figure 2 which shows fractures evident in the large boulders on this talus slope.

Referee Comment 33 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?

Author Answer: Please see our response to Referee #1, comment 6, which addresses this comment.

C18

Changes to the Manuscript: Please see our response to Referee #1, comment 6, which addresses this comment.

Referee Comment 34 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.

Author Answer: We are fans of telling an engaging story, which often means providing interpretations of some results in order to motivate additional analyses that lead to additional results. A ‘just-the-facts’ approach to reporting results can make more work for the reader, who might have to hold in mind many disparate observations while taking in others, until at a later point in the paper the authors view of their meaning is revealed. Identifying how specific results answer specific questions as the story unfolds can then make it possible to focus in the discussion section on the larger context and implications. To indicate that we have chosen this approach, we titled this section “results and interpretations.” For a compelling argument for this story-telling mindset in writing science papers, we enthusiastically recommend the book “Writing Science” by Joshua Schimel.

Changes to the Manuscript: We did not make any changes in response to this comment

Referee Comment 35: Line 185-186: Would be helpful to clarify how p-values are calculated in methods section

Author Answer: As noted above and in our response to Referee #1, comment 6, we prefer to explain the data analysis where the results are analyzed. We appreciate that the hypothesis tests were not explained thoroughly.

Changes to the Manuscript: We have expanded the explanation of this and other hypothesis tests.

Referee Comment 36: 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.

C19

Author Answer: We agree that readers unfamiliar with the Weibull distribution would not be well served by the text in the original version of the manuscript.

Changes to the Manuscript: We have revised this section to explain more carefully how we compared our data to the theoretical distributions, and how to interpret the Weibull plotting space; we also provide reference for readers who want more background.

Referee Comment 37: 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)”

Author Answer: We prefer to direct the reader to Figure 5 for the point of illustrating the quality of the fits. The previous sentence reports the results of the analysis that used a statistics software package, not the graphical analysis of Figure 5.

Changes to the Manuscript: We did not make any changes in response to this comment.

Referee Comment 38: 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.

Author Answer: We appreciate that this section of the original text was unclear. The quantitative link between the theoretical Weibull distribution and the empirical data lies in the degree of linearity of the data when plotted in this space. The purpose of Figure 5 is to allow readers to evaluate for themselves the how the data compare to pure linear trends. We focus on this qualitative approach to evaluating the data because hypothesis tests of whether the data match the theoretical distribution are not that informative. Ironically, a larger data set is more likely to fail the test (i.e. to result in a

C20

rejection of the null hypothesis that the data are indistinguishable from the theoretical distribution) because the standard of proof becomes so high that even slight deviations cause the hypothesis to be rejected.

Changes to the Manuscript: We have substantially revised the text addressing the comparisons of the data with theoretical distributions, and have added more detail regarding the quantitative tests that lead us to use the Weibull plotting space to evaluate the empirical distributions.

Referee Comment 40: Lines 200 and 202: "In some cases" – is there anything specific about these cases? Should these be discussed more in the discussion section?

Author Answer: Our point here is to help the reader to understand how the patterns of data plotted in the Weibull space link to previous results, such as the differences in the spreads of the distributions documented in Figure 3 and Figure 4, panel d.

Changes to the Manuscript: This passage has been revised for clarity.

Referee Comment 41: 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 . . ."

Author Answer: Agreed.

Changes to the Manuscript: Sentence changed as suggested.

Referee Comment 42: Line 220: how much lower are the mean b:a, c:a ratios?

Author Answer: For b:a ratio, metasediment = 0.57 and granodiorite = 0.64 For c:a ratio, metasediment = 0.26 and granodiorite = 0.35

Changes to the Manuscript: These values have been added to the text in parentheses.

Referee Comment (Set) 43: Line 221-223: this is an interpretation and could be moved to a discussion section?

C21

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

Author Answer: As in our response to comment 34, we prefer to link narrow results with their specific interpretations when the discussion focuses on the larger context and bigger picture implications.

Changes to the Manuscript: We did not make any changes in response to this comment.

Discussion:

Referee Comment 44: 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?)

Author Answer: We agree that subheadings would improve the clarity of the discussion section.

Changes to the Manuscript: We have added sub-headings to discussion

Referee Comment 45: Line 237: "nearly the full network of fractures" .. Rephrase: "from the network of fractures with apertures >2 mm"?

Author Answer: The detection limit of 2mm refers to the minimum length along the scan line of solid rock between successive fractures that we counted as a spacing between fractures. This does not refer to the width of the fracture opening, or aperture.

Changes to the Manuscript: In the methods section we have clarified this detail.

C22

Referee Comment 46: Lines 237-238: "This finding" and "it" are hard to unpack in this sentence. I'm having a hard time interpreting this sentence.

Author Answer: Agreed.

Changes to the Manuscript: The sentence has been revised for clarity

Referee Comment 47: 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.

Author Answer: Agreed.

Changes to the Manuscript: This section has been expanded to consider alternative explanations for the offset between median talus b-axis and median fracture spacing, including the comparison between scan-line and scaled photo measurement techniques.

Referee Comment 48: Lines 251-252: Not sure where these results are presented??

Author Answer: As noted in our response to comment 26, this is where the outcome of the subsurface particle size analysis is reported.

C23

Changes to the Manuscript: No changes were made in response to this comment.

Referee Comment 49: Line 257: are there field photographs of this? These would be useful to show if even qualitative?

Author Answer: Agreed.

Changes to the Manuscript: As noted elsewhere, we have added a field photo to Figure 2

Referee Comment 50: 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.

Author Answer: This study was not designed to test for the influence of climate on fracture spacing or talus particle size. We would expect climate to play a role if weathering were a significant factor, but by selecting bare bedrock cliff faces, known to be actively eroding, we sought to minimize the potential for weathering to be a significant factor. The fact that the climate variables do not correlate with the regression residuals from Figure 4 confirms that assumption, but does not shed any light on the question of how weathering would influence initial particle sizes if it were a significant factor. While the plots suggested in this comment would be fascinating to consider in a study with a more appropriate experimental design, here they would only illustrate this unsurprising negative result, and distract from the main purpose of this study.

Changes to the Manuscript: We did not make any changes in response to this com-

C24

ment.

Referee Comment 51: 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 $Tr = 0$, and soil-mantled systems $Tr \neq 0$?

Author Answer: Agreed.

Changes to the Manuscript: Background on the Moore et al. (2009) study has been included in the expanded site description section in the methods. This discussion paragraph has been revised to more clearly distinguish between the two end member cases.

Referee Comment 52: Lines 275-276: Add reference to Attal et al., 2015 and Roda-Boluda et al., 2018

Author Answer: Agreed.

Changes to the Manuscript: These two references are now cited in this section.

Referee Comment (Set) 53: 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/esurf-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 $Tr \neq 0$? 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

C25

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 soilmantled 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, $Tr \neq 0$ 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?

Author Answer: These are excellent questions and points about the issue of scale in applying this conceptual framework. We would certainly expect spatial variability within a catchment, even along a single hillslope, in the relative influence of latent sediment size and weathering on the size of sediments produced at any given location. Each of the possible controlling factors may vary on a different length scale, creating complex patterns of variation. Alternatively, relatively simple gradients might arise due to gradual covariation of factors such as erosion rate and local climate in a high relief catchment underlain by uniform rock. We propose this framework as a conceptual tool to be applied primarily at the scale of a locally homogenous sediment production regime, however large or small that might be. Hopefully this framework will be useful for understanding and predicting spatial variability in initial sediment size, once the controls within a given sediment production regime are well understood.

Changes to the Manuscript: We have expanded this discussion to include the issues of scale and spatial variability.

Referee Comment (Set) 54: Also, it's not clear to me how long the sediment in the talus

C26

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 occurring? 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 $\hat{=}$ 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??

Author Answer: Our conceptual framework is only intended to apply to the question of what controls the initial sediment size, and not how sediment size evolves afterward. However, the long-term fate of talus is a very interesting question! Initial size and degree of pre-detachment weathering will certainly influence how rapidly size evolves within a talus deposit, however this topic is beyond the scope of this paper.

Changes to the Manuscript: We did not make any changes in response to this comment.

Referee Comment 55: 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/doi/10.1038/ngeo1380> , and Neely et al., 2019 <https://doi.org/10.1016/j.epsl.2019.06.011>

Author Answer: Agreed.

Changes to the Manuscript: The suggested citations have been added.

Referee Comment 56: 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”

Author Answer: See response to comment set 53 above

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Changes to the Manuscript: Misspelling of DiBiase has been fixed throughout the revised manuscript.

Referee Comment 57: 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).

Author Answer: Agreed.

Changes to the Manuscript: This passage has been revised as noted in the response to comment 53 above.

Referee Comment 59: 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?

Author Answer: Agreed.

Changes to the Manuscript: This passage has been revised to place our conceptual framework in the context of the results of these other studies, cited here.

Referee Comment 60: 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?

Author Answer: This is the regional value used by Moore et al. (2009) in their calculations of cliff retreat rate.

Changes to the Manuscript: In the expanded study site subsection at the beginning of the methods section, we include this age value in the review of the Moore et al. (2009) results, along with a relevant citation.

Referee Comment 61: Figure 1: I think it would be useful to expand the study map

C28

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?)

Author Answer: We appreciate the suggestion to elaborate on the geologic context, however the study was not designed to explain why fracture spacing varies, only how initial sediment size might be related to fracture spacing. The suggested figure would be more appropriate for a different study, for example one designed to test the influence of fault proximity on fracture spacing. Scale bars would be impractical for most of these site photos, given the considerable foreshortening. These photos are intended to provide general context, to give the reader a feel for the environmental settings, not to document the exact measurement locations. We do that in Figure 2 for one site as an example.

Changes to the Manuscript: We did not make changes in response to this comment.

Referee Comment 62: Figure 2: add a scale bar to bottom right image? Could use this figure to illustrate how the scan-line fracture spacings were measured?

Author Answer: Agreed. A sense of scale would improve the bottom right image. Annotating the bottom left figure would help illustrate how the scan-line fracture spacings were measured.

Changes to the Manuscript: For bottom right image, the caption has been revised to include the length of the tape transect and to point out the person in red jacket for scale. We have revised the bottom left image to include annotations identifying the fracture spacings measured.

Referee Comment 63: Figure 4: add p-values referenced in text that compare to the 1:1 line to panels?

Author Answer: Labeling the regression lines with p values could be misleading. That

C29

might imply that the null hypothesis being tested is that the slope of the line is zero. We have revised the caption instead.

Changes to the Manuscript: Caption revised be more precise about the finding of no-significant difference between the regression line slopes and the 1-to-1 line.

Referee Comment 64: Figure 5: mark resolution detection limits on x-axis of panels?

Author Answer: The detection limit is implicit as no data exist that fall below the detection limits. Adding a vertical line at the equivalent of 2 mm would clutter already busy graphs with information of low importance. In addition, it could be confusing because the x-axis is normalized by the mean.

Changes to the Manuscript: We did not make changes in response to this comment.

Referee Comment 65: 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!

Author Answer: We used the same notation as in the original Sneed and Folk (1958). Glad you liked the figure!

Changes to the Manuscript: We did not make changes in response to this comment.

Referee Comment (Set) 66: 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

C30

to hillslope erosion rate, fracture spacing, and climatic/ecologic factors?

Author Answer: These are simple cartoons intended to illustrate the concepts. They are not intended to be scaled diagrams or to depict actual landscapes. Our intention is to focus here on just two of the many factors that can vary to create large differences in the time-scale ratio: fracture spacing in unweathered bedrock and the climate-controlled thickness of the weathering zone. According to the definitions of the two time scales, erosion rate is an important factor, however it cancels out of the ratio. This does not mean erosion rate doesn't matter, only that the influences of particle size and weathering zone thickness both scale relative to erosion rate. Other cartoons could be created to illustrate the conceptual framework that might vary erosion rate and slope, however we prefer to use this version to focus on particle size and weathering zone thickness.

Changes to the Manuscript: We have added letter labels to each panel.

Referee Comment 67: "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."

Author Answer: We are enormously grateful to the Referee for these valuable and voluminous comments! We have done our best to make revisions to satisfy all those comments that could reasonably be addressed. Many of the remaining comments and suggestions are insightful and creative, but are outside the scope of this study.

Interactive comment on Earth Surf. Dynam. Discuss., <https://doi.org/10.5194/esurf-2020-54>, 2020.