Author response to review comments from Associate Editor on manuscript:
"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.

June 25, 2021

We are grateful for these astute and targeted comments, which highlighted a number of important shortcomings in the previous draft. We agree with all of the points raised, and have made changes to the manuscript to address every suggested edit and requested clarification. Following these minor revisions, the manuscript is clearer, and we hope, more persuasive. Once again, thank you for these very helpful comments

Review comment 1
Line 14: It isn't really sediment before it detaches. Consider using "blocks" instead.

Author response: Agreed, good suggestion.

Changes to the manuscript: Text revised as suggested.

Review comment 2
Line 22: I would clarify to say this is talus at your sites. I can imagine a postglacial landscape where the talus generation has stopped (or slowed a lot) and there is weathering of the talus field. I don't think you want to give readers the impression this is universally true

Author response: Agreed, talus certainly does weather in other settings.

Changes to the manuscript: Revised text now reads: "In addition, talus at our sites has not undergone much weathering…"

Review comment 3
Line 82-86: In Inyo Creek the coarse material reflects the fracture spacing. What about the fines? And in the San Gabriels and San Jacinto is it only the coarse fraction or the entire distribution that reflects the bedrock spacing? An edit isn't really needed here but if the answers to the above questions are interest please add.

Author response: The production of fine sediments is certainly of interest, and is addressed in the discussion (last paragraph of section 4.1). Adding this topic to the introduction would be a bit of a digression.

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

Review comment 4
Line 93: There isn't frost cracking at the Inyo site? If the California sites are too warm for frost cracking I would say so above.

Author response: Good point. Conditions favorable to frost cracking do occur at the Inyo Creek site. Frost cracking may be implicated in the observed downvalley fining of the finer mode of the hillslope sediment size distribution there.

Changes to the manuscript: We have added a sentence noting the potential role of frost cracking at Inyo Creek: "Variation in the frequency and intensity of frost cracking with elevation at the Inyo Creek site may also be implicated in the downvalley fining of the fine mode of the hillslope sediment size distribution (Riebe et al., 2015; Sklar et al., 2020)."

Review comment 5
Line 148: This seems too strong: temperature can span much more than 10C, the variation in temperature has a large range as well, you could have 15 sites just spanning metamorphic grade, and precipitation, globally, has a much wider range than in the study sites. I do think you've done a good job in selecting a wide range of sites. But I'm not sure if I would expect these to span "much" (I'm not sure what that even means) of the range in sizes. This would be less distracting if you just said "...expected to produce talus spanning a wide range of sizes".

Author response: Agreed.

Changes to the manuscript: Text revised as suggested.

Review comment 6
Line 162: I feel like it would be helpful to readers if you had an image of the scan line and fractures here (like in figure 2).

Author response: Agreed.

Changes to the manuscript: We have added a second call to Figure 2, near the end of this paragraph, specifically directing readers to panel b, which shows a close-up image of a scan line crossing fractures. The revised text now reads: "This yields a distribution of fracture spacings measured as the distance between successive fractures (Fig. 2b)."

Review comment 7
Line 204: Parallel gives the impression you are comparing straight lines (at least to me). Consider changing this to say "the distributions have the same shape but are offset..."

Author response: Curved lines can be considered "parallel" if the distance between lines (measured perpendicular to the lines) remains approximately constant (for example, here's a link to the relevant Wikipedia page https://en.wikipedia.org/wiki/Parallel_curve). We prefer to use this term, rather than say they have the same shape, because it is more precise. The “same shape” could be interpreted as meaning they are described by the same function. For example, if the “shape” is a cumulative normal distribution, differing standard deviations would produce non-parallel curves even though the shape is the same.

Changes to the manuscript: To clarify what we mean by “parallel” we have added a parenthetical definition “(i.e. offset by a constant distance)”.

Review comment 8
Line 215: Awkward phrasing. Are you saying Messenzehl et al didn't test for correspondence? Clarify this sentence.

Author response: Agreed. This mention of Messenzehl et al., 2018 is not accurate, not needed and not appropriate.

Changes to the manuscript: The clause referring to Messenzehl et al., 2018 has been removed.

Review comment 9
Line 229: Interesting. I've not got to the discussion yet but I'm hoping to hear your opinion of whether this is by chance or there is some mechanistic explanation. - Well, now on page 11 I see there is some reference to this but no speculation if there is a physical process responsible. If only a subset of the fractures end up failing when blocks are released could there be some stress release mechanism that explains this offset? I'm not requesting a change to the text but if you did have an opinion it would be interesting to hear it.

Author response: This is indeed an interesting phenomenon! It is beyond the scope of this paper to address the fracture mechanics that must underlie the differential propagation of fractures of differing sizes. However, a simple (mostly geometric) argument can be made that where longer, more persistent fractures extend more rapidly than smaller fractures, they may be more likely to intersect and detach relatively larger blocks containing smaller fractures.

Changes to the manuscript: We have added a sentence to the discussion paragraph addressing this point. “Incomplete exploitation of fractures could occur where longer, more persistent fractures extend more rapidly than shorter fractures, intersecting to detach relatively large blocks”

Review comment 10
Line 294: I don't really understand what you mean here. The data shows the talus is coarser than suggested by the fracture spacing. Please clarify.

Author response: Agreed. This passage needs clarification. This statement is intended to summarize the fundamental observation that particle detachment from these cliffs is dominated mechanisms that exploit the fractures visible on the cliff face, exploiting most if not all fractures.

Changes to the manuscript: The opening sentence to the discussion now reads "The close correlations between talus size and fracture spacing distributions at our sites (Figs. 3–5) suggest that particles are detached by mechanisms that exploit most if not all of fractures exposed on the cliff faces and do not undergo much size reduction due to physical or chemical weathering in talus deposits."

Review comment 11
Line 297: But this contrasts with the Messenzehl et al results, doesn't it? Does frost cracking (caused by segregation ice) control particle size or not?

Author response: Excellent question! Frost cracking undoubtedly occurs at some of our sites, but other mechanisms may be equally or more important. An assessment of the role of segregation ice growth in detaching particles is beyond the scope of this paper. The mention of segregation ice and sub-critical crack growth is not needed, and not helpful.

Changes to the manuscript: The clause mentioning segregation ice growth has been deleted. The sentence now reads "This finding, while limited to our sites, is robust across a wide range of lithologies and weathering conditions, suggesting that it spans a range of processes that could lead to particle detachment and subsequent weathering in talus deposits."

Review comment 12
Line 313: The surface is coarser. How is it possible the two medians would be the same? Clarify please.

Author comment: Agreed, this passage needs clarification. The method we used (taken from Bunte and Abt, 2001) tests whether the finer sediments should be considered a population distinct from the coarser sediments or whether they represent the fine tail of the distribution of a single population dominated by the coarser sizes. The null hypothesis is that there is only one population. To reject the null hypothesis, the fines must be sufficiently incompatible with the tail of the coarse distribution. In our case, they were not.

Changes to the manuscript: This passage has been revised for clarity, and now reads: "…the size distributions of the bulk samples overlap with the fine tail of the talus distribution enough that they could be combined using established techniques (Bunte and Abt, 2001) into a single continuous distribution. In this case, fine particles are sufficiently numerous on the talus surface that the statistics of the measured surface size distribution would not be affected by loss of a fraction of the fine-tail particles to the interstitial pores."

Review comment 13
Line 320: Say if it reduces the offset.

Author comment: Agreed, this is a vague way of reporting the outcome of this calculation, which showed no significant difference in the offset between the two methods.

Changes to the manuscript: The revised sentence now reads “However, there is no significant difference in the offset when we apply this approach to our data.”

Review comment 14
Line 328: Is the conversely needed here if you already said "however"?

Author comment: Agreed.

Changes to the manuscript: The “conversely” has been removed.

Review comment 15
Line 355-358: This could be clearer. If I understand the argument, the talus slopes have no "regolith" (defining regolith in this context is a little challenging) whereas the California sites from other studies do. I guess you mean it is not all loose blocks in these other studies and there are patches of finer material that hold water and promote weathering. Where do these patches come from? I think this part needs a few more sentences of explanation.

Author comment: Agreed, this passage needs a clearer explanation of the comparison between the cliff-talus setting and the bedrock slopes studied in the other California sites.

Changes to the manuscript: Several sentences have been added to provide a clearer explanation. The revised text now reads “However, variations in particle size across these sites can also be explained by differences in weathering that are driven by variations in the fractional coverage of regolith. In these steep mountain landscapes, the rough surfaces of bedrock hillslopes provide locations favorable to transient storage of coarse particles produced on adjacent slopes. During storage, particles are subject to physical weathering processes, such as frost cracking and thermal stresses, and chemical weathering processes aided by the presence of water and vegetation. In contrast, the relatively smooth and nearly vertical cliff faces at our study sites lack locations favorable to transient particle storage, and the adjacent talus surfaces are well-drained and minimally vegetated. Hence, we conclude that the offset toward finer sizes evident at the Inyo Creek, San Gabriel, and San Jacinto sites is due in part to substantial weathering not experienced on the bare cliff faces and talus slopes at our sites.

Review comment 16
Line 376: This needs an "e.g." since this is only a subset of papers reporting this.

Author response: Agreed.

Changes to the manuscript: “e.g.” has been added as suggested.

Review comment 17
Line 390: I don't think "all of the sediment" is the same as "one layer". Surely the talus slope is more than one layer thick.

Author comment: This sentence needs clarification. The inference is that at the site with the most slowly retreating cliff face, the volume of talus that has accumulated over the past 13,000 years was produced by a cliff-normal, average depth of erosion of the cliff face equal to the median spacing between fractures. The talus pile is presumably many particles thick, except at the toe, but must occupy an area smaller than the area of the cliff face area that eroded to produce the particles. In other words, the talus slope at this site is not one layer thick, but was produced by one-layer's worth of particle detachment from the bedrock cliff.

Changes to the manuscript: We have made several edits to clarify this point. The revised passage now reads "Application of Equation 1 to data from our sites indicates that T_P is as short as 88 years for detachment of one layer of 60-mm diameter latent particles from the cliff at EP-26, the most rapidly eroding cliff face. At the most slowly eroding cliff face, CB-1, Equation 1 suggests that it took 16,500 years to detach one layer of 330-mm diameter particles, indicating that the entire post-glacial accumulation time and more was needed to detach a single layer of latent particles with the characteristic median size (Table 2). The calculated T_P at the remaining talus-cliff pairs in the Sierra Nevada sites is less than 13,000 years, consistent with the assumption in the cliff retreat rate calculations of Moore et al. (2009) that all of the sediment now contained within the talus piles was produced after the glaciers retreated.

Review comment 18
Line 393: Why would the cliff retreat rate (used to calculate T_P be the same as the surface erosion rate? I think equation (2) needs an erosion ratio between cliff retreat and vertical erosion on the talus/soil

Author response: This is a very helpful comment because it shows that in the previous draft we failed to adequately explain the definitions of the two terms involved in the derivation of equation 2.

The definition of T_P is independent of the orientation of the eroding surface, and is based on the simplifying assumptions erosion occurs normal to the bedrock surface and that the median fracture spacing characterizes the depth of erosion that results from detachment of a typical particle. The definition of T_R is also independent of the orientation of the eroding surface, and is based on the simplifying assumption that rock mass is exhumed on a vector normal to the eroding surface, passing through a weathering zone with a thickness defined along a line also oriented normal to the eroding surface. In applying this framework to our eroding cliff sites, cliff retreat rate is the relevant erosion rate for both particle production and bedrock weathering zone thickness. This is the rate at which rock mass is exhumed toward the eroding surface where particles are produced. Much like a symmetrical weathering rind on the surface of a large clast, weathering of the rock mass the near the eroding cliff face can reasonably be assumed to occur over some thickness normal to the free face.

Erosion of the talus slope is not relevant to calculating the time-scale of particle production nor is it relevant to calculating the time-scale of pre-detachment weathering within the bedrock, because particles do not become part of the talus slope until after they are detached from the rock mass.

Changes to the manuscript: We have made numerous changes to sections 4.2 and 4.3 to clarify these points. The changes include: Adding an equation (the new Equation 1) for residence time, with accompanying text to explain; replacing the term regolith with more precise language; additional text to clarify that erosion rate (*E)* applies to the bedrock cliff face and particle production surface and not to the depositional talus surface; additional text to more clearly explain the conceptual framework of pre-detachment weathering occurring during exhumation of rock normal to the surface where particles are produced.