Round 2 reviewer 1 comments & response

Dear referee and editors,

We thank the reviewer for providing in-depth and constructive suggestions, which we think improved this manuscript. We have implemented most of the comments, including adding a supplemental figure and editing figures 1 and 9 to show the importance of slope for denudation rates and the mobility of boulders. However, we have not removed the part of the manuscript about stream and fault orientations. Our data suggests that bedrock fracture patterns affect stream incision to some degree, which is shown by the close relation between local fractures, regional faults, and stream orientations that evidence a connection between the analyses through faulting, fracturing and grain size. Hence, we disagree with the reviewer suggestion to exclude them from the manuscript. Below, we reply to each suggestion in blue text.

I would consider my recommendations to be moderate revisions, because the manuscript is much improved from the previous version but I still have one or two issues with the presentation of the data that requires additional consideration. The model assumptions and field sampling caveats have been sufficiently cleared up in my mind so that my review this round focuses on the framing of the manuscript and main takeaways from the work.

The issue here is that in addition to the climate variation between the three sites that the authors frame their work around, there is also differences in slope angle (which strongly modulate their field data) and a lack of information about how bedrock fractures vary across the sites (which the authors also speculate are important modulators of erosion). As presented, the hypothesis to be tested – differential denudation of soil and boulders – does not appear related to the climate gradient but instead to slope. If the authors were interested in determining how the climate gradient impacted grain size, they would need a climate gradient that spans a homogeneously fractured bedrock, but the authors emphasize the spatial variability of fracturing too. Or, a variably fractured bedrock would be exposed to the same climate to test the role of fractures on differential denudation, but that data only exists for one site.

We agree that slope appears to be more important than climate for controlling erosion rates, at least with respect to La Campana. We also agree that it is difficult to explore the relationships between grain size and climate when fracture density is variable, and we do not intend to argue that climate sets grain size in our field sites. In NA, 5 samples show that higher fracture density is correlated to higher denudation rates, but this data is limited and we understand that it cannot be extrapolated beyond part of NA. The three field sites were chosen (within the research program we are part of) because of their similarity in tectonic setting, rock type, expected uplift rates, lack of evidence for glaciation, and minimal human influence. In the course of our field work, we stumbled across exposed bedrock surfaces, often at topographic highs, that are devoid of fractures, whereas in other parts one can observe meters-thick regolith. These observations prompted us to think about the role of fractures in controlling grain sizes and possibly differential denudation and we designed the study of which we present the results here. We agree with the reviewer that it would have been great if either climate or fracture density would have been constant across all sites to single out only one variable. But that's not the case here, and quite likely nowhere, especially when considering that climate varies in time. In our view, these challenges do not prohibit asking questions of how both, climate and fracture density, affect denudation, and we also think that our observations and data provide some relevant results for finding answers to these questions.

I find it worrisome in the author response to reviewer 2 that the authors do not attempt to reconcile why the model (both numerical and conceptual) does not "fit" all three sites – I think it is entirely possible to build at least a conceptual model from the data. My read of the work as presented now is that, when the landscape is gently-sloping, big boulders act more like bedrock, denuding slowly, and humid conditions lead to larger disparities between boulders and soil. In intermediate climate conditions, where slopes are high, boulder mobility approaches that of loose soil. So slope is perhaps a primary control on differential denudation, and climate is secondary. With limited data exploring this parameter space (due to the confounding variables addressed above), the authors can really only speculate, but the measurements are valuable. This in turn may assist the authors in incorporating slope or other factors into their model so that it "fits" their data. This is where a revision of Figure 9 to describe inter-site variability would be useful (see figure comment)

That's a great and concise summary of our work, to which we largely agree. It is not immediately clear that where slopes are steep, big boulders approach the mobility of loose soil. Of course, if slopes are so steep that everything that is loose falls down immediately and no regolith can accumulate at the surface, also big blocks or boulders fall down. However, that's the case in none of the field sites. In contrast, we find boulders protruding above the surface in all sites, even La Campana, which means that they have a lower mobility than the soil that is eroded around them. Nevertheless, in the revised manuscript, we added statements that slope is an important factor in controlling differential denudation, in the abstract, at the last paragraphs of the discussion, and in the conclusions.

We disagree that we "do not attempt to reconcile why the model (both numerical and conceptual) does not "fit" all three sites". Most of the first chapter of our discussion (5.1 Deciphering the denudation rates of boulders and soil) is devoted to understanding why some of the sample pairs appear to fail. We consider several confounding factors or processes that could be responsible for observations that don't fit the model. In addition, we also attempt to draw conclusions from these problems for the cases of where the model appeared to work. Reviewer 2's suggestion was to include these problems already in the beginning ("Later, the discussion considers scenarios that would explain this outcome, but a better model would include the possibility from the beginning"). While we agree in general, we don't know how this could be done. For example, if we all agree that steeper slopes result in greater mobility of boulders, how can we know in advance if a boulder has been moved? Our approach to minimize such issues was to collect amalgamated samples (explained in section 3.1.1), but we don't know how many boulders may have moved and we cannot preclude that the results we obtained are affected by this process. In our discussion, we thus highlight how some sample-pairs from hilltops and hillslopes differ.

Finally, we changed Figure 9 and added boulders rolling down the hill and accumulating at the base (panel C). Although very simplified and sketchy, we think this helps and makes the figure better. Thanks for the suggestion! We also added text to the discussion that explains these processes.

In contrast I still find the discussion of stream orientation to detract from the main focus of the data (differential erosion on the hillslopes) and truly do recommend eliminating it from this paper. I emphasize that the authors do not present any data on erosion rates associated with streams in certain alignments, channel morphology as a function of fault alignment, grain sizes in streams, erosion mechanisms operating within those streams, etc. While I agree that rivers eroding along faults is *thematically* related to the paper (erosion and fractures), the overwhelming focus of the paper (and field data) and thus most fleshed-out conceptual model building is on hillslope processes. Please see my two in-line comments related to stream discussion for additional insight.

We thank the reviewer for the recommendation. However, over the course of presenting our results in meetings with other researchers, we always received positive feedback regarding this part. In fact, it helped in discussions to connect the hillslope scale processes to the landscape scale. As the reviewer acknowledges, the thematic link is there, and in our opinion, it allows us to link back to fractures. To make the link stronger between our field data and the fault orientation analysis, we have added fracture orientation measurements from NA that align relatively well with fault and stream orientations (now figure 5B). We had removed this data from one of the first versions of the manuscript because we felt that we didn't have enough measurements (41) and because we only had measurements for NA. However, we now feel that it might strengthen the link between field data and the larger-scale analysis.

From the last reviews, we understood that both reviewers were concerned that we frame our study in the context of fracture density without providing any data on fractures. We thus deemphasized fractures in the presentation and discussion of our results, and find the outcome better than before. However, we want to emphasize that fractures are nevertheless a key control of grain sizes on hillslopes and in rivers. Just because we cannot measure fracture density where soil is covering the bedrock, does not mean we should ban it from the discussion. Our analysis of stream orientation is in our view and apparently also in the view of others we had discussions with, a useful addition that attempts to bridge the gap in observations.

In-line comments:

Lines 16-18: With revision, I don't agree that sediment size dictates *location* of topographic highs and lows (on what scale?). Revise to either "nature of breakdown of bedrock" or sediment size dictates erosion mechanisms that can act on a portion of the landscape, etc, (I believe you argue for both in this work).

We understand the sentiment, and have clarified the sentence to state more clearly that sediment size influences the location of topographic highs because of the way it influences erosion. We have adjusted the wording to this: "sediment size can regulate hillslope denudation rates and thereby influence the location of topographic highs and valleys."

Line 20 and throughout abstract: I would recommend "erode" be revised to "denude" throughout, since erosion might imply chemical dissolution, but your cosmo data mainly focuses on denudation.

We agree and have changed "erode" to "denude' throughout the manuscript.

Line 28-29: Consider revising this statement to be a punchier version of what I think is an interesting takeaway from your work based on this revision, something like: "in contrast to lower-sloping sites in which boulders and bedrock are equally (im)mobile, at the steepest site, steep slopes appear to facilitate higher denudation rates for boulders whereas bedrock erosion rates remain low"

Great idea, we have revised the sentence to something very similar to what the reviewer suggests: "In our lower-sloping field sties, boulders and bedrock are similarly immobile based on similar 10Be concentrations. However, in the site with a Mediterranean climate, steeper slopes allow for higher denudation rates for both soil and boulders (~40-140 m Myr-1), while the bedrock denudation rate remains low (~22 m Myr-1)."

Line 33: "through" to "by"

Done

Line 39-40: The observation that streams follow the orientation of the faults in the area does not lead to the inference that bedrock fracture patterns set maximum grain sizes in the field site. No data are presented to assist in this inference and thus I do not think these two sentences should be in the abstract without substantially more proof of the relationship between these two ideas.

We understand that the connection between those two statements is weak. We changed the phrasing of the sentence to: "We thus infer that tectonically-induced fractures and faults influence stream incision, and likely also influence hillslope denudation in our field sites through setting the maximum grain size." This way the two ideas are shown as separate and not dependent on each other, but thematically related.

Lines 55-60: The Neely and DiBiase work in the San Gabriel and San Jacinto Mountains are counter-examples to the idea that slower erosion = smaller grain size, but I think their work would argue for less soil cover = larger grain size, so maybe re-phrase this sentence to focus less on erosion rates and more that soil cover promotes the chemical weathering that breaks down rock, regardless of erosion rate (plus you can have thick soils and fast erosion in the Southern Alps a la Larson et al) (Larsen et al., 2014; DiBiase et al., 2018; Neely et al., 2019; Neely and DiBiase, 2020)

Ok, fair enough. Although we believe this statement is true in many cases, at the same time, the statement contradicts the concept that higher fracture density leads to smaller grains which leads to faster erosion, which is what we argue for in this paper, and what was also a conclusion of Neely and DiBiase's work in the San Gabriel and San Jacinto Mountains. So, to limit reader confusion, we have removed the phrase "being the consequence of slow erosion."

Lines 93-96: Once again I struggle with relating the scales and processes of fractured bedrock on hillslopes and landscape-scale faulting controlling river locations. First, fractured bedrock can erode via plucking (not grain mobility but size-related) (Lamb et al., 2015) and weaker rock could have lower tensile strength for abrasion (Sklar and Dietrich, 2001). I don't think the Molnar et al. reference is helping this argument considering the scale of your work vs theirs (e.g. if you had erosion contrasts versus fracture density between your sites), and the Roy et al. study is a model where grain size is set as a rule. Your final sentences implies you will examine whether erosion rates are higher in fault-aligned rivers versus unaligned rivers. However, you do not present any data that show that faulted rivers host smaller grains than non-faulted rivers and/or erosion rates associated with faulted vs non-faulted rivers. I would recommend using the Roy study as fodder for discussion of how faulting might play a role in setting the grain sizes in your landscape but dropping the river analysis altogether.

Thanks for this point - the arguments that fractured bedrock can enhance plucking and abrasion are good supporting evidence for our argument that faults and fractures enhance fluvial incision. While our stream orientation analysis is focused on the landscape scale, the work is actually done on a smaller scale, where fractures that are related to the larger faults reduce the size of particles that can be plucked by the stream, and the same fractures also weaken the bedrock and allow for enhanced abrasion. Therefore, we have added the references to the manuscript.

In terms of the last sentence, we did not intend to imply that we have gathered erosion rates; rather it simply says that we expect that rivers preferentially incise in zones of more intensely fractured rocks that align with the orientation of faults. This is indeed what we investigate in our river analysis: whether rivers are incising along directions that are similar to the directions of mapped faults in the region. In the next paragraph, in the first sentence we write that we "also discuss the extent to which bedrock fracturing affects…stream incision". We do not claim that we have evidence that faulted streams have smaller grain size and higher erosion rates. It would be really great to expand the study to collect such data, though!

Lines 101-115: I think this is a very strong, clear paragraph. Use this as a template for re-writing your abstract.

Thank you! We have edited the abstract to make it more similar to this paragraph, for example by including the null hypothesis.

Lines 124: Neither citation references convexities of the hillslopes, and Terweh et al. only speculates that higher rainfall leads to more landslides at LC. You should supply field photos or hillslope cross-sections (perhaps in the supplement) to demonstrate convexities and evidence of diffusive transport mechanisms (tree throw? Bioturbation?), unless it is just "lack of landslides," in which case maybe just rephrase that some sites are more landslide-dominated than others.

Good point – we revised the sentence to say that NA and SG have more gently sloping hillslopes and a lack of observed landslides.

Lines 137-138: Perhaps include a supplementary figure with longitudinal profiles showing how your sites are all upstream from knickpoints.

Good idea; we have added a supplementary figure with longitudinal profiles and maps with knickpoints and samples plotted from all three field sites. We refer to this figure in the field area description section of the revised manuscript (Fig. S1). LC and SG do not have any knickpoints near the samples and all knickpoints in NA are below the sample locations.

Line 160 and throughout methods: I would recommend collating all of your sampling caveats and assumptions separate from your actual methods rather than having them spread throughout a method paragraph in order to help your reader keep track of them.

This makes sense, we have arranged the sentences in the methods to list the caveats after the methods themselves have been outlined.

Lines 235-242: Sorry to be "that reviewer" but if both reviewers mention how density differences in boulders versus soil might impact your interpretations (which, given slow erosion, they probably do), you might need to at least have as a supplemental figure a sensitivity test demonstrating a range of reasonable densities (soil could be anywhere from 1.0-2.0 g cm-3) to demonstrate the robustness of your interpretations, if anything to prove us wrong

We agree that lowering the soil density in our model would affect denudation rates and considered adding a supplemental figure where we vary the soil density. However, we believe that our approach of using the same density value for soil and bedrock makes more sense in this case. We have also added a sentence on the assumption that soil density can be treated the same as bedrock density in the model at the beginning of section 5.1, where we elaborate on our model assumptions: "Although the density of soil and saprolite layers would be in reality lower, we assume a steady thickness of these layers through time, which means that the lowering of the bedrock-saprolite boundary occurs at the same rate as that of the soil surface. The actual thickness of the soil and saprolite layers is relatively unimportant (Granger and Riebe, 2014), and thus one can consider the thickness to be zero. While this approach may appear unrealistic, it is important to note that the attenuation of cosmogenic nuclide production with depth depends on length times density, and a lower density soil layer can simply be viewed as inflated bedrock."

Line 395: typo in figure callout?

Actually, the figure is called Fig. 2B3, it is panel B3 in Figure 2. We grouped the panels by field site. We clarified this in the figure caption.

Line 413: eliminate "at a later stage"

Done

Line 421: Elaborate here on specific model assumptions

Good idea, the model assumptions are listed at the end of section 3.1.3, but now we describe them again here at the beginning of section 5.1. We have also added a model assumption that soil density can be treated the same as bedrock density in the model, and elaborate on this at the beginning of section 5.1.

Line 486: eliminate parenthetical and rephrase/simplify so it fits in the flow of the topic sentence.

We eliminated the phrase in parentheses and moved it to the end of the previous section.

Lines 649-650: Clarify "more difficult to transport," as big boulders can transport large distances when they topple off a slope (as seen in talus piles with the biggest blocks at the base; check DiBiase et al., 2017)

True – we added "on low or moderate slopes"

Line 653: Unclear what "per sample site" refers to

We removed "per sample site"

Lines 656-660: This sentence as written is difficult to parse; please rephrase.

Thank you for the comment, we agree that the phrasing was a bit confusing. We actually broke up the sentence and moved the part of it that lists the number of fracture spacing and boulder width measurements to the methods section and the part that describes the results of our measurements to the results section (the end of section 4.3). We also changed the sentence(s) to this: "Fracture spacing in NA is generally larger than boulder width (Fig. 5C), although there is overlap. If we assume that boulder width is initially delineated by fracture spacing at depth, our results indicate that boulders have reduced in size in the weathering zone prior to and during exhumation. If we further assume that hillslope sediment lies on a spectrum with unweathered blocks delineated by fractures on one end, and sediment that has been significantly reduced in size in the weathering zone on the other end (e.g. Verdian et al., 2021), boulders in NA seem to fall somewhere in the middle."

Lines 692-693: "Small streams are transient features compared to larger ones" is a really difficult argument to make with any of the data presented, and I think is symptomatic of how unrelated the stream incision angle is to the rest of the paper.

We agree that we do not support that sentence with evidence and therefore we have removed it.

Line 721: "instead" does not seem to need to be here

Agreed- we removed it.

Line 730-734: It is my personal preference to not have a "future work" section at the end of the paper but to rather incorporate the "what-ifs" into other paragraphs in the discussion.

We removed the future work section.

Figure 1: Based on my read of this version the differences in slope between the sites seems important, so I would recommend adding a slope map beneath the maps of the sites with a common color bar.

Agreed – we have revised Figure 1 to include slope maps of all three sites.

Figure 6: I would recommend making the x and y axes the same so that there is a 1:1 line so readers can quickly understand the differential erosion.

Good idea. We have revised figure 6 so that the x and y axes are the same for each plot.

Figure 9: As presented right now this figure is not really specific to the work here (it's too generic to be helpful). What would be helpful would be a three-panel figure that demonstrates your conceptual models for each site (which I think you walked through well in the discussion). With such a figure you can visually represent the differential erosion that you model (currently only visualized abstractly in figure 6). You can also demonstrate how the dynamics at LC are different from the other sites due to slope.

We understand that Figure 9 is quite general and that it applies mostly to NA and SG. However, we think that the processes illustrated in Figure 9 also apply to LC. To fit LC more closely, we added more boulders that roll down the hill and accumulate at the base in panel C. Although very simplified and sketchy, we think this helps and makes the figure better. We also added text to the discussion that explains these processes.

DiBiase, R.A., Lamb, M.P., Ganti, V., and Booth, A.M., 2017, Slope, grain size, and roughness controls on dry sediment transport and storage on steep hillslopes: Journal of Geophysical Research: Earth Surface, v. 122, p. 941–960, doi:10.1002/2016JF003970.

DiBiase, R.A., Rossi, M.W., and Neely, A.B., 2018, Fracture density and grain size controls on the relief structure of bedrock landscapes: Geology, v. 46, p. 399–402, doi:10.1130/G40006.1.

Lamb, M.P., Finnegan, N.J., Scheingross, J.S., and Sklar, L.S., 2015, New insights into the mechanics of fluvial bedrock erosion through flume experiments and theory: Geomorphology, v. 244, p. 33–55, doi:10.1016/j.geomorph.2015.03.003.

Larsen, I.J., Almond, P.C., Eger, A., Stone, J.O., Montgomery, D.R., and Malcolm, B., 2014, Rapid Soil Production and Weathering in the Southern Alps, New Zealand: Science, v. 343, p. 637–641, doi:10.1126/science.1244908.

Neely, A.B., and DiBiase, R.A., 2020, Drainage Area, Bedrock Fracture Spacing, and Weathering Controls on Landscape-Scale Patterns in Surface Sediment Grain Size: Journal of Geophysical Research: Earth Surface, v. 125, doi:10.1029/2020JF005560.

Neely, A.B., DiBiase, R.A., Corbett, L.B., Bierman, P.R., and Caffee, M.W., 2019, Bedrock fracture density controls on hillslope erodibility in steep, rocky landscapes with patchy soil cover, southern California, USA | Elsevier Enhanced Reader:, doi:10.1016/j.epsl.2019.06.011.

Sklar, L.S., and Dietrich, W.E., 2001, Sediment and rock strength controls on river incision into bedrock: Geology, v. 29, p. 1087–1090, doi:10.1130/0091-7613(2001)029<1087:SARSCO>2.0.CO