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.

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)

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.

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).

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.

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"

Line 33: "through" to "by"

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.

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)

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.

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

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.

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

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.

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 😊

Line 395: typo in figure callout?

Line 413: eliminate “at a later stage”

Line 421: Elaborate here on specific model assumptions

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

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)

Line 653: Unclear what “per sample site” refers to

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

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.

Line 721: “instead” does not seem to need to be here

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.

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.

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.

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.

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.