In this document, all comments by reviewers are in black text while author responses are in red.

Dear Editors,

This document contains our response to overarching and line-level comments provided by both reviewers. Reviewer suggestions were generally positive and have resulted in enormous improvement of the manuscript. We are very grateful that both reviewers clearly took the time to conduct a careful assessment of the manuscript with regards to both readability and scientific quality.

Overarching concerns identified by the reviewers included (1) a lack of quantitative vegetation data and control sites (2) limited linkages between experimental site conditions and our results, particularly aspect and vegetation (3) poor connection between discussion, results, and introduction (4) inadequate connection to results of previous experiments of this nature in the discussion, and (5) limited discussion of factors relevant to error in Lomax shaper parameter A. Each of these issues was addressed through edits with the exception of (1). Due to methodological limitations of the field campaign that could not be retroactively addressed, we were unable to resolve suggestions to extract quantitative information from existing data (e.g. site photos); this will certainly be considered in future work.

In response to these reviews, we largely re-worked the results and discussion sections of the manuscript to directly interpret and discuss how experimental results were influenced by particle size, slope, aspect, and time since fire. Our description of vegetation has been enhanced with photos and consolidated to results Section 3.1. A series of more detailed figures have replaced the previous figure summarizing experimental results, and a table has been added to Appendix B (Table B1) to allow readers to identify the number of experimental particles dropped, Lomax parameters, and error in estimated Lomax parameters for each experimental subset.

Please note that we did not include the relocation of the site description from the introduction to the methods (Section 1.1 to Section 2.1.1) or descriptions of vegetation from the introduction to results (Section 1.1. to Section 3.1) as “tracked changes” (although these changes were made). These changes were not tracked because tracking the relocation of these sections would prevent reviewers from easily identifying changes made within these sections of text.

Additionally, while this manuscript was under review by Reviewers #1 and #2, further comments were provided by Jonathan Perkins directly to the authors. These comments largely focused on the utility of Lomax parameter B, and several figures were added in response (specifically, Figs. 11d-f & 12d-f).

Reviewer 1 comments follow

The authors have run some clever experiments and have collected a wealth of interesting data over several field campaigns. The math involved is above my paygrade so, hopefully, the other

reviewer will be able to look at it more critically. I have uploaded a pdf of the manuscript with detailed comments. Here are my more general comments.

We appreciate your recognition of the value of the data presented in this manuscript, even with the lack of quantitative vegetation data and a control site. The line level comments were also quite helpful in cleaning up the manuscript and forced us to more critically evaluate suppositions we had presented as fact, particularly in the discussion. Thank you for the concise and helpful comments!

1) The arguments about how transport regime changes over time after the fire seem to hinge primarily on the condition and density of the vegetation; however, there's no actual data presented on this, just qualitative observations. There are established techniques for making these types of measurements at the ground level and not employing them in this study was a missed opportunity considering how important the condition of the ground is with respect to frictional resistance. Perhaps the authors took a bunch of pictures and could use them to provide quantitative information? Moreover, the descriptions that are given regarding the vegetation aren't very systematic; it would be helpful to provide a table describing the condition of the vegetation and the surface during each field campaign, segregated by slope aspect.

The lack of vegetation data is unfortunate given that vegetation recovery apparently controlled our experimental results. In addition to the TLS data collected in Summer 2021, we attempted to constrain changes in vegetative roughness and density through time with point cloud data derived from drone or handheld camera photographs and structure from motion. The use of inconsistent methods and presence of grasses that moved in the breeze between images prevented us from successfully comparing data from different experimental epochs. We were unable to retroactively quantify vegetation conditions from existing data, though this was a great idea by the reviewer.

To acknowledge these issues and improve future iterations of these experiments conducted by others, we consolidated all discussion of vegetation to the results (Section 3.1) and summarized dominant vegetation structures by aspect and experimental epoch to assist in interpretation. Site photos selected to highlight aspect and seasonally variable vegetation structures were also added to Section 3.1. In the discussion, we provided suggestions for alternative manual field techniques we could have used to constrain vegetation structure and identified limitations in our ability to interpret results due to a lack of quantitative data (Section 4.3).

2) Also, considering the importance that vegetation and aspect presumably have in modulating the post-fire dry ravel response and the recovery to 'background' conditions, there was little explanation of why or how. Indeed, the last sentence in the paper emphasizes the importance of vegetation and aspect, but this idea isn't explored in the manuscript. For the revision, I would

recommend diving into this concept a bit more, otherwise, it's not clear why it matters that you're comparing north- and south-facing slopes.

We have now removed any reference to a return to "background" conditions given the lack of a control site and modified the discussion to focus on trends in transport with time on both aspects, relating these observations to vegetation conditions. These changes are tied to increasing vegetation density at the south facing slope through time.

3) The study bills itself as an examination of 'post-fire' variability in dry ravel, which implies that what is being measured is unique to recently burned hillslopes. However, the only way to know whether that's true or not would be if you compared your results to north- and south-facing unburned hillslopes. In other words, including control sites would have been important for determining how much of the effect that you saw was due to the fire. Although it's obviously too late to implement this suggestion, consider including control sites in the future, especially if you're documenting changes over time. I would recommend addressing the absence of control sites, and its implication, somewhere in the Discussion. At a minimum, it might spur future studies to consider this.

This is a fair critique. We did attempt to find an unburned site, but logistical considerations and limited time prevented us from conducting experiments at control sites. While control sites could have confirmed some of our interpretations and provided a useful set of data to isolate postfire site responses, our results were in line with previous studies of this nature, and we now provide additional evidence that postfire changes in vegetation structure likely drove changes in particle transport observed through our three experimental epochs.

Manny

Line level comments copied from annotated manuscript:

L 12: Are they constrained at any level? I don't understand why this distinction is being made.

There was no need for a distinction of spatial scale; removed "on a regional level"

L 21: difficult to parse; please split into 2 sentences

Sentences have been replaced, and their replacements are of reasonable length.

L 46: please specify what it is being converted to

Specified "conversion of gravitational potential energy to *kinetic energy*"

L 65: If you're going to distinguish between Southern California and Central California, you will need to define the geographic extents of each region.

Also, you should explain why distinguishing the two regions matter because it's not clear to me. Both have similar types of vegetation. Do they have different fire regimes?

Excellent point; Reviewer #2 shared a similar concern. This distinction was unnecessary, and we removed it to generally summarize observations of ravel in California (rather than Southern CA and Central CA).

L85: awkward

Replaced “7, 11, and 22 months post-fire” with “in March 2021, July 2021, and March 2022”

L 93: Site descriptions should be in the Methods section.

Site description has been moved to methods; previous Section 1.1 is now Section 2.1.1

L 103: typo

Missing space corrected.

L 108: awkward

This has been rewritten (see following line-level comment).

L 108: how was this determined? What are the units of 'softness'? :-)

Replaced “The mineral surface of the NFS is more poorly consolidated and softer than at the SFS.”

with “Although soil compressive strength was not measured directly during our field visits, we observed that the soil at the SFS easily supported our weight with limited deformation. Where soil was present at the NFS, it would compact underfoot such that we left deep footprints, suggesting lower soil strength at the NFS relative to the SFS.”

L 111: (Fig. 1 caption) better to show as a dot or a box than an X

Replaced X with dot.

L 111: So, there was no control site? It's too late now, obviously, but including a control site would have been important.

That is correct.

L 127: What does this mean? Do you just mean the grass is thicker or more dense?

Phrase removed for clarity (unnecessary phrase).

L 130: I think the point of this paragraph is to make a distinction between the SFS and the NFS in terms of the fire behavior. If that's the case, it doesn't come across very clearly. I would recommend making that point explicit and then being specific about what those differences were.

Also, it might be worth explaining the fire behavior differed between the slopes.

The topic sentence has been improved for this paragraph to clarify its purpose: “In addition to observations of different vegetation structures and soil moisture, different fire behavior at the SFS and NFS resulted in observations of variable burn severity between aspects during our field visits.”

L 130: It's not clear what this means. Also, instead of saying it was 'limited,' it would be better to provide a number.

This text describing the estimated depth of charred material has been removed; besides lacking a numeric value, it was unnecessary and was not referenced later in the manuscript.

L 132: too passive; How about rewriting as "We did not observe ..."

Change made as suggested.

L 146: It's not clear what 'this material' is referring to. Just the litter and dead grass or also the live vegetation?

“this material” has been replaced with “leaf litter and dead grasses” to clarify.

L 150: You're making a quantitative statement without any actual evidence.

Agreed. This supposition describing seasonal changes to the surface area of vegetation has been removed.

L 154: appeared similar to what? not clear

Modified “similar to” to “relatively unchanged” as follows: “Throughout all experimental periods, the surface characteristics of the NFS appeared *relatively unchanged* based on vegetation density and moss cover”

L 163: These are not Wolman counts. In a Wolman count, particles are chosen at random, not at regular intervals.

Thank you for pointing out this distinction; we removed all references to Wolman counts in the text and now provide additional references justifying our approach of a regular sampling distance (10 cm).

L 177: What about the gophers? Were they not burrowing?

They were, and we have modified the text to reflect that. The first sentence of Section 2.1.3. now reads “Particle drop experiments were conducted at a total of 7 sites (Fig. 1a) to simulate ravel movement initiated by burrowing of California ground squirrels and Botta’s pocket gophers”

L 179: I thought they were selected at regular intervals?

We have more carefully described our approach for particle selection in line with your comment regarding our use of “Wolman Count” and removed any reference to random selection.

L 219: awkward

Replaced “contour parallel” with “placed along contour (i.e., at a constant elevation)”

L 220: I'm having trouble visualizing this. An illustration would be helpful.

We have added a figure with an image of a tape measure (experimental starting line) annotated to indicate particle drop positions (Fig. 3d)

L 221: If I understand this correctly, the particles are being placed on the ground and then released. If that's the case, calling these 'drop experiments' is really misleading because dropping them would impart an initial velocity.

We added figures detailing our particle drop approach through photos (Fig. 3a-3c). We did release particles onto the slope with some component of initial velocity by rolling them down our palms before they contacted the mineral surface.

L 285: awkward

See response to L 289

L 286: I have no idea what this means. Please rewrite this.

See response to L 289

L 286: this statement needs a reference

See response to L 289

L 287: I don't understand what this means. Isn't all of the motion stochastic?

See response to L 289

L 289: as opposed to what other kind of motion? not clear

Response to all line level comments L 285-L 289: These comments relate to the description of particle motion being “non-stochastic” at very short distances because it is likely particles have not yet encountered roughness elements to induce random motion. Following a drop but prior to their contact with roughness elements (mineral or vegetative) particle motion is very consistent (not stochastic) and particles are unlikely to stop at very short distances, though in a natural setting they may.

We have completely rewritten this paragraph for clarity.

L 411: Larger particles are also less affected by surface roughness (Gabet and Mendoza)

This is an excellent supporting reference where noted; thank you for your suggestion. It has been incorporated.

L 428: not clear what this means

This text has been deleted as part of the reworked discussion

L 430: I don't understand what this means

This text has been deleted as part of the reworked discussion

L 434: Difficult to understand what this means

This text has been deleted as part of the reworked discussion

L 436: Much of this paragraph is really speculative, especially with respect to arguments being made regarding the density and condition of the vegetation. These are critical surface properties that should have been measured and quantified. Plant biologists have developed standard techniques for making these types of measurements.

This text has been deleted as part of the reworked discussion; discussion of existing quantitative approaches for constraining vegetation structure are now included in the limitations (Section 4.3).

L 440: this is much too vague; please be more specific

This text has been deleted as part of the reworked discussion.

L 442: this is the opposite of what is typically observed in the region; north-facing slopes generally have more vigorous vegetation growth because their soils don't dry out as quickly

This text has been deleted as part of the reworked discussion; however, this is an interesting point. Observations of antecedent soil moisture between aspects at our field site by Donaldson et al. (2023, 2024) are now provided as part of our efforts to better connect aspect and vegetation. Their observations suggest that the evapotranspiration rate was greater on the north facing slope due to the presence of oak trees, but soil moisture was generally also greater at the north facing slope. This is explored in Section 3.1 where we describe observed vegetation structures, and again in Section 4.1.2 where we discuss why aspect dependent differences in particle transport were greater in Summer 2021 than Spring 2022.

L 455: Since this is being stated as a fact, a reference will be needed.

Also, why not use your data to back-calculate relative friction coefficients?

This text has been deleted as part of the reworked discussion; we have now back-calculated friction coefficients following eq. 32 in Furbish et al, 2021 (Rarefied particle motions on hillslopes – Part 2: Analysis).

L 461: Please reword this as nonlocal transport on these hillslopes actually happens over time-scales of seconds.

This text has been deleted as part of the reworked discussion; this is a very important distinction, and while the text is now deleted we appreciate this comment.

L 479: they weren't 'dropped' as far as I can tell

This text has been deleted as part of the reworked discussion; improvements to our methods section (addition of Fig. 3 showing drop approach) should resolve this.

L 481: those weren't Wolman counts

This text has been deleted as part of the reworked discussion; all references to our pebble counts now state “pebble counts” rather than “Wolman pebble counts”

L 483: This isn't really what your experiments indicate. Your experiments indicate that, if medium to large particles were mobilized by natural processes, they would have experienced nonlocal transport. In other words, your experiments don't have anything to say about whether these particles were actually mobilized during the course of your study. For that, you would need to rely on other observations.

This text has been deleted as part of the reworked discussion. We have taken care to distinguish experimental observations versus our interpretations of how natural ravel processes may change postfire.

L 488: This isn't quite right. Gabet and Dunne (2003; Water Resources Research) presents a nonlinear sediment transport equation calibrated with data from post-fire dry ravel measurements (see section 2.7 in that paper). As noted in Gabet and Mendoza, the nonlinearity was likely due to nonlocal transport.

This specific text at L488 has been deleted as part of the reworked discussion; however, this is a very useful comment, and we now cite Gabet and Dunne (2003) in the discussion section when describing exactly what you’ve noted in your comment.

L 494: in addition to saying that there are differences, be specific about how they differ (eg, SFS are drier in the northern hemisphere) and how those differences may drive the different fluxes.

Antecedent soil moisture conditions at our site as evaluated by Donaldson et al. (2023, 2024) are now included in the qualitative vegetation results (Section 3.1). These observations align with your description in your comment.

Reviewer 2 comments follow

This work represents an exciting test of previously explored frameworks for quantifying the complex nature of nonlocal sediment transport as presented in Roth et al 2022 and the pair of Furbish papers. The authors take advantage of a disturbance and set up a natural experiment, and they make good use of hard-won field and experimental data. However, I agree with Manny in that there is a disconnect between the focus of the motivation for the study, the data collected, and the discussion/conclusions drawn by the study. This confusion makes it difficult to interpret and contextualize the results and determine if the hypothesized relationships have been tested.

Thank you for your appreciation of our application of Furbish's framework to these in situ experiments. Your note of disconnect between motivation, results, and discussion drove major revisions of the results and discussion sections that we hope will enhance readability. Both your overarching and line-level comments have vastly improved this manuscript. Thank you for the effort you clearly put into a thorough review, particularly in verifying our code was accessible and operational.

My comments and suggestions, from "big picture" to technical:

- As Manny wrote I find it hard to reconcile the data as presented with the results and then discussion. I found that the introduction, results, and discussion did not have similar focuses and I recommend realigning these sections so that the nature of the data collected and analyzed matches the scope and validity of the discussion content. One recommendation I have for this is a reimagining of the discussion section, which is right now two large paragraphs. Instead, I recommend using the discussion to systematically step through the results, challenges, and implications of each variable tested here – I get (1) particle size, (2) time since disturbance, (3) aspect, and (4) hillslope position/slope (e.g. Figure 5 says to me hillslope position isn't an important control – is that also your interpretation?). Right now I only see size and time reported but not systematically discussed. I also recommend using the second paragraph of the conclusion section to lead the discussion (and then keeping the conclusion focused on implications for the evolution of the distribution shapes).

The results and discussion sections have been rewritten following these suggestions. By replacing Fig. 7 with what are now Figs. 10-13, we attempted to make site- and slope-dependent behaviors more visually obvious. The results Section 3.3 containing these figures and the corresponding discussion Section 4.1 (and subsections) are more clearly divided by the apparent influence of particle size, aspect, and slope through time.

To directly answer your question, hillslope gradient was found to be a clear influence on transport behavior. This was not obvious in the previous draft; Fig. 11 now directly shows trends in Lomax parameters and the reciprocal of mean travel distance with gradient.

- Again to echo Manny it seems as though vegetation plays a key role in these results despite not being presented thoroughly with the data – my understanding is that aspect strongly modulates vegetation, to the point where the text focuses on north- versus south-facing slopes when for the reader it might be clearer to discuss "grassy" versus "wooded" slopes (because insolation differences drive vegetation community?)? In addition to adding any and all observations of vegetation as Manny recommended, I recommend revising the abstract, introduction, and discussion to make the distinction of vegetation rather than simply aspect between the two slope directions.

We now note the presence of different vegetation structures on either aspect in each of the abstract, introduction, and discussion. These changes are most obvious in the results and discussion, which have been completely revised in the context of changing vegetation structures through time. Unfortunately, our attempts at characterizing vegetation from point cloud data were unsuccessful and we were unable to extract quantitative vegetation descriptions from any existing data after realizing our drone photography based SfM efforts were not successful due to the dense grasses at the site (as noted in the response to Reviewer #1).

In the abstract, the phrase "both grassy south-facing slopes and oak woodland north-facing slopes" emphasizes differences in vegetation by aspect, and in the introduction we associate aspect with vegetation in the phrase "aspect-dependent vegetation loss and recovery."

- Although literature on dry ravel is cited in the introduction, I noted a lack of engagement with the results from previous work in the discussion section beyond a broad allusion to previous work thinking about vegetation– has this study changed or reinforced how we think about these processes? Please add these outward-looking details in the discussion as well.

This is an excellent suggestion. We now compare our findings with similar field experiments by Gabet and Mendoza (2012), DiBiase et al., (2017), and Roth et al., (2020) in the discussion. Invoking these previous studies has improved the discussion section immensely.

- Alas I am also no math wiz, but I did my due diligence by (1) skimming Roth et al 2020 and the Furbish papers and (2) downloading and executing the Matlab code. These experiences indicate to me that the Lomax distribution reasonably captures the physical characteristics of the system, but that you're "A" value is only as good as the number and nature of your input data. Sections 2.2 through 2.4 are rather dense (although I really like Figure 3), and it's not clear how the method in this work differs from Roth et al. 2020. I recommend citing out what you can and then explaining, with less technical terms, how your real life messy data were fit into the Lomax mold, especially focusing on your method for truncation and censorship (which I would have liked to have been defined) and your sample size, because these seem at least to me where the most slop in your A value will be.

Thank you very much for accessing and testing the Matlab code. It is extremely useful to know that you were able to do so successfully. We also appreciate that Fig. 3 (now 4) helped you interpret these sections.

You are correct in that the A value is only as good as the volume/nature of input data, but the uncertainty is also dependent on the form of the distribution, as heavier tails defined by limited numbers of observations induce more uncertainty (see our more detailed response to this issue in response to the next comment). The newly added Table B1 highlights this by presenting A values, the standard error in A, and the number of particles for each experimental subset (particle size/slope/aspect/time).

The fitting method in this work does not differ in any way from Roth et al., 2020, and we have clarified this in the methods Section 2.4. (2nd sentence of first paragraph) with the text "Our optimization approach is identical to that of Roth et al., (2020)." The majority of their methods were relegated to their supplementary materials, but we included ~560 words describing the Lomax model (Section 2.2), ~470 words describing our methods of truncation and censorship (Section 2.3), and ~470 words describing our optimization approach (Section 2.4). We felt these sections were necessary for readers unfamiliar with Furbish et al. (2021) and Roth et al. (2020), though we made efforts to simplify descriptions of the Lomax model and our optimization approach relative to these works. We believe these details are necessary and we have not modified these sections except to improve readability through Section 2.4.

In Section 2.4 we attempted to improve the explanation of why distributions were truncated, as Reviewer #1 noted this section was quite confusing. Upon review, we also realized that we never actually described what happened to censored measurements, and we now clarify that they were excluded from the empirical distributions. Thank you for noting this disconnect.

- I was interested in running the code because I wanted to experiment with the sensitivity of A to various data shapes and sizes because the authors alluded to the downsides of having (relatively) small sample sizes. Alas, after I installed Matlab and all the required toolboxes I determined that the Lomax optimization step is computationally intensive and would take too long (gentle suggestion to explore parallelization schemes? If each simulation is independent from other simulations perhaps you can run many at once and then compute?). So I am left without an answer to the question: "How many data points do we need for the calculated A to not be useless?," which I think is important for the authors to answer at least numerically (as I was hoping to do, as I didn't quite find the figure I wanted from the Roth or Furbish papers). Can you show the readers with a figure (perhaps in the supplement) how the sample size affects A and how uncertain we are about A if we're only getting 100 samples? My idea was to generate a synthetic distribution with 1000 grains and then simulate randomly sampling 50, 100, 200, 500 of them and then quantifying the spread of subsequent A values would be.

Accessing and testing the code from a reviewer perspective is extremely valuable, and we appreciate these comments. With regards to long run times – these can be greatly shortened, but we did not make this obvious to users and will do so when we update our code repository. The code is time-consuming entirely due to our selection of bootstrapping to estimate error. If you open Scripts>BootstrapTime.m and reduce the hard-coded values of the variable "iboot' from 10000 to 100, the code should run on the order of 30 minutes rather than 12+ hours; while the error estimates will be less reliable in this case, the main estimates of A will be unchanged.

Your suggestions to explore how sample size affects A would be an interesting study; however, we believe investigation of this matter deserves a dedicated publication and is out of scope for this manuscript. Such an endeavor would require significantly more empirical data than has yet been amassed to constrain the amount and type of variation (i.e., noise) in particle travel distances as they encounter diverse terrain and vegetation. This is exemplified in supplemental Fig. B1 (Summer 2021), where a kink in the travel distances around 0.3 m most likely reflects the persistent effect of microtopography and/or vegetation structure at that location (also see Roth et al., 2020 and Furbish et al., 2021b). The minimum sample size necessary to achieve a "good" A value will therefore depend on the site-dependent characteristics of the particle paths, and is further complicated by the fact that A and B values are actually spatially variable functions of the particle path, which we simplify by fitting a distribution. This means that the broader question of appropriate sample size can only be answered relative to a given length scale of interest over which we want to estimate A (see Roth et al., 2020 for further discussion).

Appendix A of Furbish et al., 2021b (Rarefied particle motions on hillslopes – Part 2: Analysis) explores the question of sample size and provides estimates of variability in A with the form of the distribution and the number of experimental particles. The authors note that Lomax parameters identified where N particles < 1000 should be accepted with skepticism.

- In general I recommend revising each paragraph to have a strong topic sentence to focus the remaining sentences, and having those topic sentences follow in a coherent order for each section. I also noted many complex phrases and noun chains (e.g. "dry ravel travel distance exceedance probabilities") that hindered my ability to follow some concepts, so I recommend simplifying some of these.

We have revised the topic sentences of most paragraphs making up the results and discussion section. While not directly requested in this comment, we also added outline paragraphs at the beginning of the results and discussion sections to provide the reader with a sense of the purpose of the following text.

In a handful of locations, we replaced "dry ravel travel distance exceedance probability" by the equivalent R(x). We agree, this is an arduous phrase that we would prefer to avoid.

Some line-by-line comments:

Lines 19-21: These two ideas ("tracked evolution" and "fitting a distribution") are not connected in a clear way.

We modified this sentence and added clarification. These changes are italicized below:

"We *characterized* post-fire evolution of particle transport regimes by fitting a probabilistic Lomax distribution model to the empirical travel distance exceedance probabilities *for each experimental particle size, surface gradient, and time*. *The resulting Lomax shape and scale parameters were used to identify the transport regime for each subset of simulated ravel, , ranging from "bounded" (light-tailed or local) to "runaway" (heavy-tailed or nonlocal) motion."*

Line 26: Abstract should wrap up with a "why we care" sentence or two.

We modified the abstract such that final sentences present main conclusions and their implications: "Our experimental results indicated that as vegetation recovered over the first two years post-fire, the behavior of small particles (median intermediate axis of 6 mm) became less similar across the experimental sites due to different vegetation structures, whereas medium and large particles (median intermediate axes of 13 mm, 28 mm) exhibited a general transition from more runaway to more bounded transport, and large particles became less sensitive to surface gradient. All particle sizes exhibited a decrease in the length scale of transport with time. Of all particle subsets, larger particles on steeper slopes were more likely to experience nonlocal transport, consistent with previous observations and theory. These findings are further corroborated by hillslope and channel deposits, which suggest that large particles were preferentially evacuated from the hillslope to the channel during or immediately after the fire. Our results indicate that nonlocal transport of in situ particles likely occurs in the experimental study catchment, and the presence of wildfire increases the likelihood of nonlocal transport, particularly on steeper slopes."

Line 59: The "lower gradient on smoother hillslopes" phrase appears to be a red herring, as is the later reference to the Central California landscape – why is this important? Does Central California have these low gradient slopes to test a hypothesis you seem to be alluding to in the topic sentence of this paragraph?

Geographic distinctions between Central and Northern California have been removed from these sections. As you identified, it was a red herring and was never discussed later in the manuscript.

Lines 85-92: This is a great paragraph and sets the paper up well.

Thank you very much for this compliment!

Figure 1: Maybe pop out Row C photos so they are big enough to see the contrasts. Also feels like Panel A should have a topographic cross-section as an inset.

We have slightly increased the size of Row C photos (now Row D) by modifying their aspect ratio. Much better photos of vegetation in each experimental epoch are now also shown in Figs. 5 and 6. We have also added topographic cross-sections as requested.

Line 129: Does the Donaldson citation here mean that your observations are consistent with this other work?

Yes, that is correct; we now more explicitly state that our qualitative observations of antecedent moisture were formally validated by Donaldson et al. (2024) who conducted fieldwork in the same catchment (Arbor Creek). Their observations indicate both rates of evapotranspiration and shallow soil moisture were greater at the north facing slope relative to the south facing slope.

Lines 140-51: This section is very connected to the conclusions but is not addressed in results and discussion – does this need to move sections and be placed in context of results? Lines 147-149 in particular read like results.

This section has been moved to the results section as suggested (Section 3.1).

Lines 330-334: In the Results tell us the number or percentage of $0<A<0.1$

We have removed reference to $0<|A|<0.1$, and instead present a table indicating where uncertainty in the value of A (with uncertainty calculated as the standard error in A, σ_A) creates uncertainty in the sign of A (Appendix B, table B1). We also now visualize the standard error in Lomax parameters from Fig. 10 onwards. In paragraph 4 of Section 3.3 we note that for 13 of our 51 sites σ_A is greater than $|A|$, indicating uncertainty in the sign of A.

Figure 4: Add the epoch/time to the figure titles/labels

Epoch descriptions (e.g. “Summer 2021”) have been added to each panel as a title. Note that Fig. 4 is now Fig. 7.

Figure 5: Use smaller gap size for dashed lines

We completely changed these figures (now Fig. 8 & 9) to show empirical data under the fitted R(x). The gap size was not reduced to allow similar R(x) curves to remain visible in the updated plots, which are a bit “busier”.

Figure 7: Use point symbols at observation locations. Also (and this is a bigger question) can you show the reader a +/- uncertainty on A (for example, some of the relatively small changes in A with days since fire may be within uncertainty? Table A4 implies there’s some spread)

Point symbols have now been placed at observation locations in Fig. 10 (previously Fig. 7). Uncertainty (calculated as the standard error) has been added to figures presenting Lomax parameters, and a table including uncertainty on A has been placed in the Appendix (Table B1). These additions greatly enhance the utility of our results as presented – excellent suggestions.