

Review Responses for egusphere-2025-4671 by Chong et al. titled Seasonal influence on post-fire debris flow likelihood after the 2020 Lake Fire

Public justification (visible to the public if the article is accepted and published). Three reviewers asked for major revision, so we proceed accordingly.

Dear reviewers and editors,

Thank you to all four of the reviewers and editors for your constructive comments and suggestions to improve this manuscript. The resubmission now contains (1) review response, (2) tracked changes Word document, and (3) clean Word document. **The line numbers in this review response correspond to those in the tracked-changes Word document.**

In this response document, the detailed response (in blue) and the reviewers' comments (in black) are organized by reviewer number (Click the links to jump to each reviewer comments: [RC1](#), [RC2](#), [RC3](#), [RC4](#)).

We sincerely thank the editor and reviewers for their time and effort in evaluating our work.

Best,

Jeng-Hann Chong on behalf of the authors

General changes

We have made changes to the main text and supplement to improve the clarity of the methods, results, and structure of the paper. We also updated the affiliations for some of the authors to reflect when the project first started and corrected the name for one of the co-authors:

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Review response to 11-Nov-2025 (RC1)

Reviewer Comments

Black – reviewer; Blue – response

Thank you for this opportunity to review this manuscript. This study details a remote sensing and field-based approach to understanding how post-wildfire debris flow susceptibility changes through time and is subject to seasonal variations following fire in five headwater basins burned in the Lake Fire (2020) located in the Transverse Ranges of southern California, USA. The authors combine information from field visits four years following fire where the authors were able obtain only outlet grain size measurements immediately following the fire (Year 0) and then also obtain grain size and soil hydraulic properties in two visits four years following the fire. In intervening time periods between Year 0 and Year 4, the authors were able to estimate dNDVI using higher-resolution spatiotemporal data from PlanetScope data to assist with estimating vegetation recovery at seasonal timescales and possible surface property changes related to sediment deposits. These data and some assumptions based on similar previous datasets collected in the region (particularly for Year 0 where field data was limited to only grain size) were then used to calibrate a process-based debris-flow likelihood model. From the authors' modeling approaches their main claims are that grain size and seasonally-variable recovery of hydraulic conductivity were greater controls on debris flow susceptibility than vegetation recovery.

I have many concerns with this study stemming from issues related to basic information on the data presented, likely flawed assumptions, and whether model results (which run contrary to established conceptual understanding of post-fire hazards) can be trusted given a lack of information on presence or absence of debris flows to validate model outputs given the overall low number of ground-based observations (only three observation campaigns over a four-year period). Therefore, I do not believe that this manuscript is ready for publication and opt for a recommendation to reject it in its present form. Below I provide my reasoning based on the information presented in the manuscript and in the event the authors revise for a future resubmission, I also provide additional detailed line comments.

Thank you for your review. We address the comments in blue. Note the response line ONLY matches the tracked changes Word document.

Specific comments:

Potential flaw #1: Unclear if data presented are reliable and possible flawed assumptions

The authors were able to visit the field site a total of three times, in Year 0 immediately following fire and Year 4 in the wet and dry seasons. For each visit, no information is provided about how and what number of sediment samples were taken (i.e. multiple samples, a composite of outlet sediments, landform position?). Also, because soil infiltration properties are extremely variable, it is also important to describe the study design of mini-disk measurements – what is the sample

size and spatial coverage of measurements (authors seem to indicate it may be higher up in the watershed given accessibility concerns in Year 0?). For hydraulic conductivity (I'll herein refer to as Ks – as the authors should also in line with previous literature they cite) – what value is eventually used as an input to the model (e.g., median, mean) and how variable are these values? A mountain of evidence provides information on just how variable this can be (see McGuire et al., 2018 and Ebel, 2022 for some examples). From the standpoint of Ks this becomes important given the finding of its importance in model outputs for these basins from sensitivity testing. Scientific reporting should always include this set of basic information (e.g. summary statistics).

Thank you for the comments. We understand that most studies perform multiple measurements within a basin and average them. Our sample area at the outlet was limited, since we aimed to take Ks measurements without disturbing the topsoil or altering the topography. Thus, we report only the initial Ks measurement from locations at the basin outlet, even though some basins do have a second Ks measurement (within a few meters of the first). We also did not include these second measurements as they were located farther from the basin outlets and may also contain sediment sourced from other basins. Only after completing the Ks measurements, we collect samples for grain size analysis. We have also added more description of this method's potential limitations to the discussion, in response to this and other reviewers' comments.

Lines 155-210 (Section 3.1): “We measured unsaturated soil hydraulic conductivity for the second and third field surveys using a Mini Disk Infiltrometer (METER Group) adjacent to our soil samples within the basin outlet without disturbing the surface material (i.e., flattening) (METER, 2021; Watson et al., 1986). We only report the initial hydraulic conductivity values for each basin, as there is a limited undisturbed area with a suitable flat area. During the first field survey, due to the lack of instruments and access, we assumed a hydraulic conductivity of 10 mm/hr as a conservative estimate based on data from the 2016 Fish Fire in the adjacent San Gabriel Mountains with no vegetation cover (McGuire et al., 2021a). We also performed a Water Droplet Penetration Test (WDPT) at the basin outlets during this survey. The 2016 Fish Fire occurred in an area with similar granitic bedrock, average slope (46°-51°), northwest-facing aspect, and vegetation types (Table S7) as compared to the 2020 Lake Fire (McGuire et al., 2021a; Paysen et al., 1980; Staley et al., 2018). We also collected soil core samples (down to 20 cm) during the second and third surveys and visually inspected for stratification and performed.”

Lines 647-650 (Section 5.1): “Despite relying on a single measurement, we observed an increase in hydraulic conductivity from the wet to dry season, consistent with Martinez et al. (2025), who also estimated an increase (~5-fold) in mean field hydraulic conductivity at the Contreras Fire in Arizona.”

Lines 748-754 (Section 5.1): “Debris flow deposits could rework the sediments in subsequent years (e.g., Hoch et al., 2021; Wall et al., 2022). However, we did not identify strong indications of rework in the basin outlets from our field sampling. We did not observe any significant stratification from our soil cores during the second and third surveys. We recognize that these

samples may not fully represent the full range of grain sizes needed to characterize basin-scale grain size. Future work should examine variability within the basin interior and at the outlets over the recovery period to better understand how grain size changes within basins.”

Watson, K. W. and Luxmoore, R. J.: Estimating macroporosity in a forest watershed by use of a tension infiltrometer, Soil Sci. Soc. Am. J., 50, 578–582, <https://doi.org/10.2136/sssaj1986.03615995005000030007x>, 1986

METER Group, Inc. USA., Mini Disk Infiltrrometer Operator's Manual, 2021

Additionally, there are some assumptions that are potentially flawed: Let’s begin with Year 0, where grain size values were measured at the outlet of the watershed under a shaky assumption that grain size measurements taken at an outlet should be representative of the full distribution of grain sizes from colluvial sources located upstream and reference a study (Santi et al., 2008) that I do not believe makes this claim. Firstly, there is a lot of dry ravel as indicated in Google Earth historical imagery of the site and Figure 2 – Panel A that are common to the Transverse Ranges (see Lamb et al., 2011). Given that field sampling occurred following very limited rainfall and associated runoff (Figure S2) it is very unlikely that a grab sample at the outlet is representative. Runoff events such as a debris flow could act to mix these sources of sediment sources but this did not appear to be the case here (and a judgment of the representativeness of such sampling is also hampered by a lack of information on sample collections indicated above). There is less concern for this particular issue in Year 4 samples following runoff, however, even in the event of a debris flow, there can be large variations in grain size that can occur depending on where sampling takes place – such as very coarse clast-supported deposits such as boulder trains or levees versus more fine-grained poorly-sorted matrix-supported portions of a debris deposit.

Thank you for the detailed comments. We reply to individual points from this comment.

We acknowledge these issues and agree that the lack of debris flows may result in less mixing, especially at the basin outlets, and that collecting upslope measurements would be the best representation of the basin and debris flow source. As mentioned in the comment above, we were unable to obtain a larger set of samples owing to the difficult terrain.

Also, debris flow deposits can also be reworked by subsequent runoff so this evidence is perishable. Therefore, measurements should account for this depositional variability which is presently not clear in the methods. Other studies have gone beyond this assumption to ensure that d50 measurements are representative through intensive sampling in the watershed (see Tang et al., 2019) but this does not appear to be the case in the present manuscript.

Thank you for the comment. We understand that debris flow deposits can be reworked. We did not identify any indication of reworked debris flow deposits at the basin outlet from our field sampling (20 cm soil cores) of the 2nd and 3rd surveys. A majority of the basin outlet looked similar from the first and subsequent surveys. We also believe that future work will benefit from a more quantitative analysis and estimates of debris flow deposits/changes using aerial or drone

photogrammetry, to produce a post-event DEM to estimate any major changes. Unfortunately, we were twice unsuccessful in getting an updated DEM from the Student Seed Proposal Program by NCALM. We added the soil core in the methods (see response above and changes in Section 5.1)

Lastly, the vegetation cover approach also remains concerning to me. Were ground measurements using approaches such as point-intercept surveys ever used to validate satellite-based dNDVI approaches? Given that the original parametrization of the dimensionless-discharge model used ground cover data taken in the field, it might be important to be sure that satellite data can adequately represent this parameter through ground-truthing.

Thank you for your comment and question. We did perform a validation using the 20-meter point-intercept survey along the riverbanks that is accessible outside the burn scar and estimated ~81% vegetation cover. The mean NDVI along the same transect on the same day is 0.5191 (max NDVI within image is 0.74); this gives ~70% vegetation cover. The dNDVI between the field measurement and 7 months before (Sept 2023) has a mean of 0.034 (indicating not many changes).

We note that the point-intercept transect method is not necessarily a direct comparison to the pixels of satellite imagery. Thus, we used the percentage of vegetation and non-vegetation for comparison to derive vegetation cover of an area from each method. We will address this point in addition to the discussion and supplement.

As mentioned in the supplement (Section S2, Figure S1), we assessed the reliability of the satellite-derived measurements using ground surveys within a 3×3 m field plot for validation in areas outside of the basins. However, it is difficult to calibrate our dNDVI values within the basin due to inaccessibility. We were also unable to access the site immediately after rain events to investigate the different dNDVI values in debris flow deposits and non-deposits. We do intend to perform site-specific controls to better characterize the box plot method for vegetation cover, as satellite imagery captures the canopy cover better than the understory vegetation. We have updated these in the main text:

Lines 306-310 (Section 3.3): “We also performed a 20-meter point-intercept survey (Herrick et al., 2009) during the second field survey along the riverbank outside the burn scar to compare with our dNDVI values for vegetation cover. We noted the surface cover (canopy/vegetation, litter, or soil) every 20 cm, where litter includes exposed loose plant material, and calculated the vegetation cover as the fraction of points intercepting canopy/vegetation.”

Lines 810-813 (Section 5.2): “The estimate from our 20-meter point-intercept survey vegetation cover (~81%) is in reasonable agreement with the mean NDVI (0.5191) along the same transect (max NDVI within image is 0.74), which yields ~70% vegetation cover. The dNDVI between the field measurement (April 2024) and 7 months earlier (Sept 2023) was low, with a mean of 0.034.”

Lines 833-836 (Section 5.3): “We note that the point-intercept survey focuses on the understory, but satellite imagery captures the vegetation cover or canopy from tree cover, exposed understory, and bare ground, which could explain the variations in vegetation cover between these two methods.”

Potential flaw #2: Lack of true validation of debris flow susceptibility modeling

This major comment stems from the fact that the authors seemed to be unable to make unambiguous measurements of runoff response in their basins. Therefore, even with relatively robust field measurements (which is already called into question above), it is difficult to know if the model is adequately representing basin-scale runoff response without independent data on presence or absence of debris flows (or some similar proxy beyond what seems to be ambiguous satellite indices and sparsely-timed field observations). Part of the issue may arise from the fact that rainfall intensity-duration thresholds were never met at all during the study period so it may be that this monitoring experiment was ill-suited to begin with (this is a challenging aspect of post-fire monitoring work where we have little control over what happens with post-fire weather!). The authors do highlight this possibility (of overall low peak rainfall I15) at least, but it does not help the case here.

Thank you for the comment and for raising the challenge posed by post-fire weather conditions. We agree that the lack of observed debris flows to validate against makes it difficult to evaluate our model results directly. However, the focus of this study was to understand how different model parameters influence PFDF likelihood for the 2020 Lake Fire, using samples and field observations from this fire to inform the model and provide reference values for each parameter. We hope our revised text better conveys this objective.

Lines 115-128 (Introduction): “In this study, we investigate how seasonal variation influences post-fire debris flows (PFDF) likelihood four years after the 2020 Lake Fire, drawing on information from five high burn severity basins. Specifically, we use data collected from three field surveys, during the first year, during a dry and wet month in the fourth year, along with high-resolution, high-repeat satellite imagery and field measurements to estimate how seasonal changes could affect PFDF likelihood immediately after the fire and four years after the fire. Our study aims to (1) evaluate long-term changes in post-fire debris flow (PFDF) likelihood in high-burn-severity areas over the four years following a fire, (2) assess the relative importance of vegetation cover, hydraulic conductivity, and grain size in controlling the rainfall intensity-duration (ID) threshold over the long term, and (3) assess the significance of seasonal variation relative to long-term post-fire recovery. Overall, we show that the rainfall ID threshold remains highly sensitive to seasonal changes in soil hydraulic conductivity even several years after the fire. This study improves our understanding of how seasonality, climatic patterns, and post-fire recovery interact to drive PFDF dynamics.”

Additionally, at face value, the authors’ work seems to show debris flow risk sticking around for up to four years, in fact even increasing due to possible seasonal-scale variability in Ks (where

drier soils generally, except for Basin A, resulted in large increases in K_s). These findings seem to be contrary to large databases of post-fire debris that find a precipitous drop in runoff-generated debris flow occurrence 3+ years out from wildfire (see Graber et al., 2023), particularly as vegetation recovery takes hold. Therefore, a suggestion of possible increases in runoff-generated debris-flow risk with increasing time since fire (as this study seems to indicate, e.g. Figure 5) is contrary to many studies and a general conceptual understanding of time evolution of runoff-related post-fire hazards (for example guidance on the USGS M1 model used for emergency assessments does not advise using rainfall ID thresholds beyond Year 2 following fire). Less ambiguous and more well-supported evidence to validate such modeling approaches would be needed here to make such claims, which is especially salient in post-fire hazard science where this work is used to inform decision-making related to public health and safety concerns.

Thank you for the comment and explanation. We agree with Graber et al. (2023) that post-fire debris flow occurrence will drop years after the fire due to factors such as vegetation, grain size changes, and hydraulic conductivity. However, we also want to point out that there was significant debris flow in Oak Glen 5 years after the El Dorado Fire in 2020, and this event highlights that even though likelihood decreases, the potential for debris flow still exists. Also, a new study from Ridgway et al. (2026) showed post-fire debris flow 3 years after the fire. We address these comments in the main text with the following changes.

Lines 63-68 (Introduction): Additionally, recent studies have shown that seasonal variations in the first two years post-fire can alter the rainfall ID threshold (Gorr et al., 2023; Martinez et al., 2025) and that post-fire debris can still occur 3 years after the fire (Ridgway et al., 2026). However, it is still unclear how seasonal variations after the first two years can influence the PFDF likelihood.”

Lines 817-822 (Section 5.2): “We propose that the increase in debris flow likelihood in the wet season in the 4th year could reflect short-term variability such as intense rainfall shortly before our field collection, which resulted in significantly low hydraulic conductivity values rather than a long-term yearly increase of debris flow likelihood. Further studies are needed to understand how long-term seasonal PFDF likelihood varies across regions with different climates (e.g., He et al., 2025; Zhou et al., 2026; Ridgway et al., 2026).”

He, K., Hu, X., Wu, Z., Zhong, Y., Zhou, Y., Gong, X., and Luo, G.: Preliminary analysis of the wildfire on March 15, 2024, and the following post-fire debris flows in Yajiang County, Sichuan, China, Landslides, 21, 3179–3189, <https://doi.org/10.1007/s10346-024-02364-z>, 2024

Ridgway, P., Bläsi, L. T., Halso, M. C., Vetsch, D. F., and Lane, B.: Beyond the initial impact: Channel evolution following wildfire and debris flows in a mountain stream of the Upper Colorado River Basin, Geomorphology, 2026, 110485, <https://doi.org/10.1016/j.geomorph.2026.110485>

Zhou, Y., Hu, X., Gong, X., He, K., Jiang, M., Yang, J., and Peng, H.: Variability of post-fire debris-flow initiation mechanisms across different lithological zones in the Hengduan Mountains, China, *Landslides*, 23, 129–141, <https://doi.org/10.1007/s10346-025-02616-6>, 2026.

Here are some specific line comments:

Line 50: Tillery and Rengers, 2020 reference – I don’t think this study supports the idea of post-fire debris flow risk persisting 10 years or more following fire, this study looked at adjacent burned vs unburned debris flow initiation mechanisms. I would opt for Graber et al., 2023 instead for a rich discussion of this in addition to DeGraff et al., 2015.

Thank you for the suggestion; we have changed this accordingly.

Lines 218-219: “Similarly, we assumed no runoff generated debris flow occurred during this period due to the lack of high-intensity rainfall from our simulations” is an unfortunate choice in phrasing that implies you are only relying off simulated rainfall for making this assumption. I would think it would be better to say based on the identified rainfall-ID threshold of __mm/hr, we found that it was unlikely debris flows were initiated because rainfall values never exceed this threshold (or something similar – needs to be supported by observations).

Thank you for the suggestion; we have changed this accordingly.

Lines 464-469 (Section 4.1.1): “Given the maximum observed rainfall intensity throughout the year (max I15 = 13 mm/hr), we found that it was unlikely that runoff-generated debris flow occurred during this period because the rainfall intensity threshold (I15 = 30 mm/hr) was not exceeded (Figure 5). We also observed dry ravel during our first field survey, including slopes outside our basin boundaries.”

Line 266: Related to the idea that basin topography could be an important control on rainfall ID threshold variability across basins – this could be easily assessed by looking at basic watershed morphology (listing basin mean slopes or channel slope variation for example). In fact, I would report these somewhere as standard practice in many post-fire geomorphic studies.

Thank you for the suggestion. We added more details in the supplement. In addition to that, we included the DEM and Channel locations (ASCII files) in our repository data, accessible using QGIS. The Dryad repository will be available upon acceptance of the manuscript.

Chong, J-H., Ghost, A., Page, B., Jesmok, G., Berg, D., Lopez, M., Upadhyay, D., Stone, D., Hauswirth, S., Lindsey, E., and Scuderi, L.: Seasonal influence on Post-Fire Debris Flow likelihood on the 2020 Lake Fire, *Dryad [Dataset]*, <https://doi.org/10.5061/dryad.vdncjsz6t>, 2026.

Supplement Table S6.

Basins	Mean basin slope [degrees]	Mean basin aspect [degrees]	Mean channel slope [degrees]	Mean channel aspect [degrees]
A	34.5	276	32.8	306

B	34.7	306	33.76	315
C	34.5	227	28.18	264
D	37.2	196	32.0	214
E	33.1	220	30.78	237

Table S6: Days with rainfall intensity exceeding I15 = 15 mm/hr. The table also shows the dates of the respective peak I15.

Line 277: Related to the comment about opening macropore spaces in drying soils - but what about the literature that supports dry soils promoting enhanced soil water repellency? (see Ebel & Moody, 2013)

Thank you for the comment. We performed the Water Drop Penetration Time only in the first field survey and did not observe the droplet entering the soil after 10 minutes, indicating strong water repellency at that time. We agree that repellency can remain strong through the drier season, but we do not have WDPT measurements from our later surveys to confirm whether this held when the hydraulic conductivity was higher. Regardless, we do not think dry soil water repellency has a great influence, given the high hydraulic conductivity measured. We added the finding from WDPT in the main text.

Lines 205-206 (Section 3.1): We also performed a Water Droplet Penetration Test (WDPT) at the basin outlets during this survey.

Lines 467-469 (Section 4.1.1): During the first field survey, we did not observe any changes after 10 minutes during the Water Droplet Penetration Test, indicating a strong repellency.”

Line 318-320: I’m glad the authors acknowledge this limitation here and something to consider more, especially for the Year 0 sampling where there did not seem to be evidence of debris-flow deposits being sampled.

Lines 321-330: The guesses here about observed shifts in grain size are interesting but I think a more careful consideration of exactly where deposits were taken could help with this. For example, did the authors see evidence of stratification of where deposits were taken (e.g., fines sitting over coarser material supporting theory #2) or inferences type of flows depositing the sediments based on sedimentary characteristics (e.g., debris flow deposits are generally more poorly sorted versus alluvium which is more well-sorted and normally-graded). If such information was recorded in the field, it could help with constraining this and may also help to elucidate concerns brought up earlier related to representativeness of grain size estimates for susceptibility modeling. If not, this is a learning opportunity for the authors to more carefully plan sampling designs and collect this information in the future (as they indicate in the future work statement at the close of the paragraph).

Thank you for the comments. As mentioned above, we collected a soil core in the 2024 surveys and did not observe any clear indication of stratification or debris flow deposits at the basin outlets. See above response for changes in the main text.

Line 360-361: Variability in rainfall patterns influencing debris flow triggers: what variability type is being referred to here? Spatial or temporal variability? Not really sure how useful this sentence is.

Thank you for the comment. We meant temporal variability. We clarified this in the main text.

Lines 802-804 (Section 5.2): We suspect the temporal variability in rainfall patterns could also influence the triggering of debris flows (Thomas et al., 2021).

Lines 368-371: I agree here, I think the inferences made from satellite data without ground validation are too generous from satellite data alone (as outlined in major comments).

Line 381: Typo to fix: “larger to not be affected” should read “large enough to not be affected.”

Thank you for the suggestion; we have fixed it accordingly.

Lines 423-424: Related to the primary succession of grasses: Does your modeling approach of relying on satellite dNDVI account for the growth of these grasses?

Thank you for the clarification. Yes, it does. Our field observation confirmed that grasses in large areas adjacent to the river are captured by the dNDVI. We clarified this in the discussion:

Lines 808-813 (Section 5.2): “We also note that satellite-derived vegetation cover used in our model input can include shrubs or grasses in areas without tree cover. Areas with high tree cover presence can obscure the understory. Therefore, the vegetation cover here does not necessarily represent understory density and surface roughness, and canopy interception can vary depending on the vegetation type (Zhao et al., 2019).”

Also for some of the figures: I would recommend changing the scale bar to be more legible (i.e. add a white background) as they can be a bit hard to read.

Thank you for the suggestion; we updated the font size of the scale and axes in Figures 1 and 5.

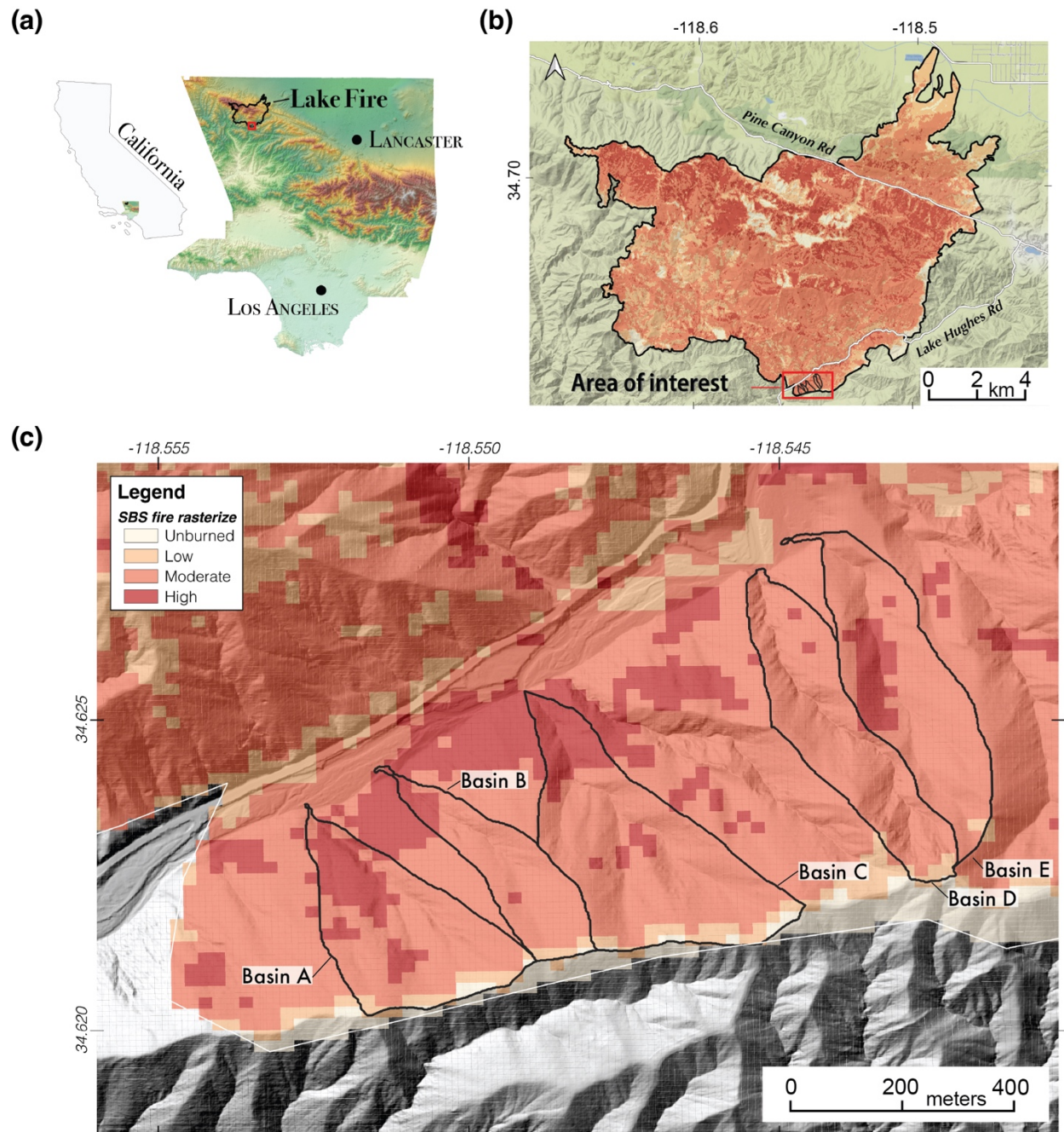


Figure 1: (a) General study area location map, (b) The Lake Fire perimeter. Colors represent the different soil burn severity (SBS) from the US Forest Service (US BAER) in (c). The five sub-basins used in this study (Black outlined areas). Basin outlets are located at the northern tip of each basin.

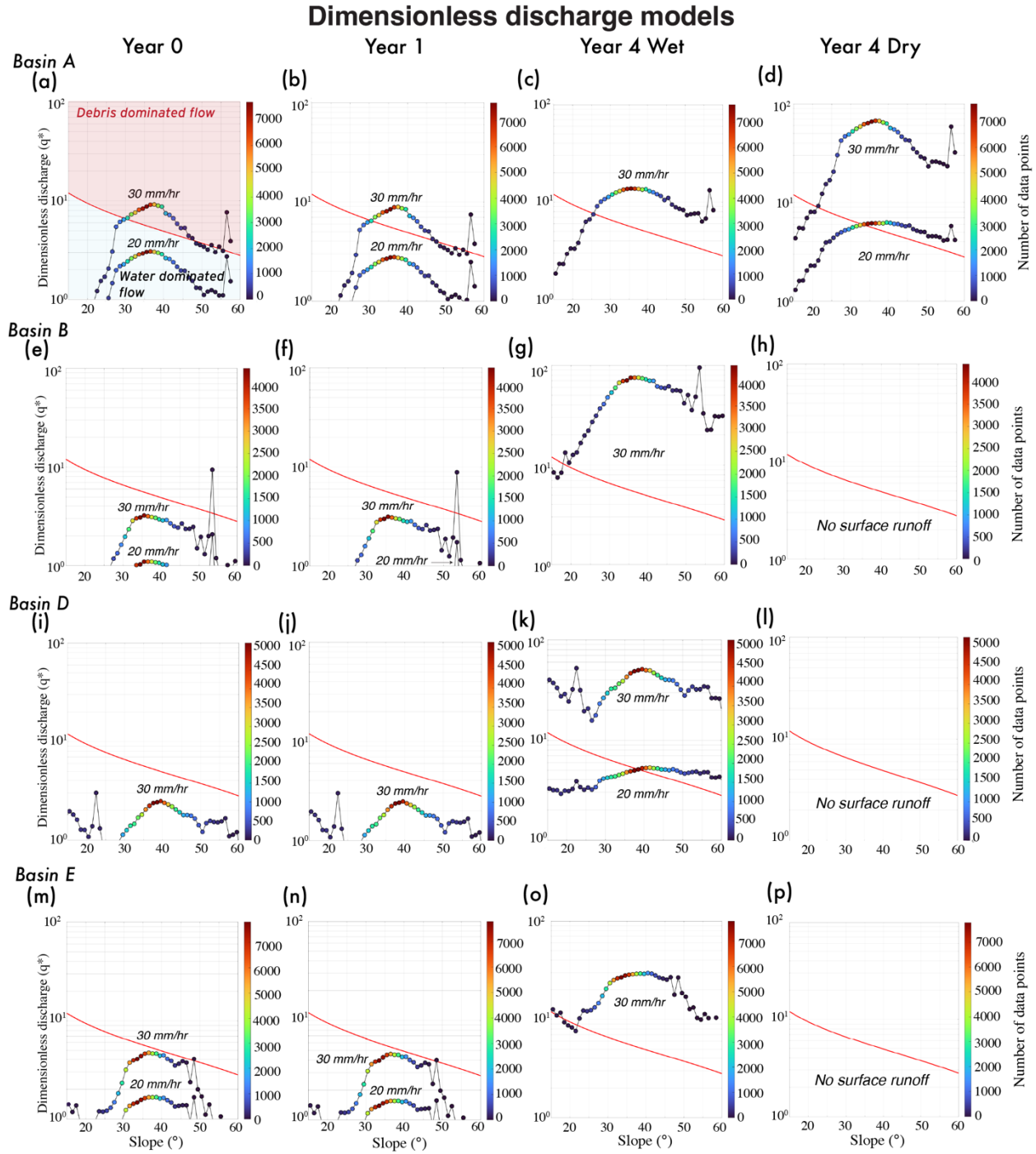


Figure 5: Dimensionless discharge models for the 4 basins (A, B, D, E) showing the number of slopes (x-axis) within each basin exceeding the dimensionless discharge threshold (red line) that is capable of producing debris-dominated flows at different 15-minute rain intensities (i.e., $I_{15} = 20$ mm/hr or 30 mm/hr). Year 0 represents immediately after fire. Water-dominated flows are indicated below the threshold (red line), and greater rain intensity values (I_{15}) indicate a higher rain intensity. Year 1 represents the first year since the fire, where we assumed the same soil

properties as in Year 0 but with 100% vegetation cover. Year 4 consists of the two field surveys during a wet and a dry season.

References cited in this review beyond those cited in study:

Ebel, B. A., & Moody, J. A. (2013). Rethinking infiltration in wildfire-affected soils. *Hydrological Processes*, 27(10), 1510-1514.

Ebel, B. A. (2022). The statistical power of post-fire soil-hydraulic property studies: Are we collecting sufficient infiltration measurements after wildland fires?. *Journal of Hydrology*, 612, 128019.

Graber, A. P., Thomas, M. A., & Kean, J. W. (2023). How long do runoff-generated debris-flow hazards persist after wildfire?. *Geophysical Research Letters*, 50(19), e2023GL105101.

Lamb, M. P., Scheingross, J. S., Amidon, W. H., Swanson, E., & Limaye, A. (2011). A model for fire-induced sediment yield by dry ravel in steep landscapes. *Journal of Geophysical Research: Earth Surface*, 116(F3).

McGuire, L. A., Rengers, F. K., Kean, J. W., Staley, D. M., & Mirus, B. B. (2018). Incorporating spatially heterogeneous infiltration capacity into hydrologic models with applications for simulating post-wildfire debris flow initiation. *Hydrological Processes*, 32(9), 1173-1187.

Review response to 22-Dec-2025 (RC2)

Reviewer Comments

Black – reviewer; Blue – response

The study "Seasonal influence on post-fire debris flow likelihood after 2020 Lake Fire" is relevant and interesting, and I support its publication after major revisions, primarily aimed to improve how the information is presented to the reader. I agree that incorporating seasonality in predicting PFDF initiation is important. However, I don't see this idea developed as it should along this manuscript. In addition, the method section needs to be more descriptive and the results need to be better developed and clearly separated from the discussion. Several results currently appear only in the discussion, and some methodological steps are not clearly explained. The description provided of the model is particularly poor, which prevents me to give a proper assessment of the robustness of the applied methodology.

Below I provide some detailed comments:

3.2 Precipitation records – since the manuscript focuses on how seasonality will improve PFDF prediction, I recommend a better characterization of the precipitation regime for each season. This can include cumulative precipitation, average – max rainfall intensity. In addition, I will clearly define what you mean for wet and dry season

Thank you for the comments. We use the general climatology of the southern California Coast Range; we consider the winter months (November-March) and summer months (April-October) to be wet and dry, respectively. We clarified this in the main text and included precipitation record, intensity, and cumulative in Figure S2 and Table S5.

Lines 151-152 (Section 3.1): “We classify the wet season (winter months) between November and March, and the dry season (summer months) between April and October.”

Lines 224-230: “Rainfall intensity is highest in the first year at I15 ~13 mm/hr. There were a total of 12 precipitation events exceeding I15 = 15 mm/hr throughout the study period. 4 events exceeded I15 = 20 mm/hr, but none exceeded I15 = 25 mm/hr (Table S6; Figure S2). Specifically, these 12 events are occurred as follows: 2 events in November 2021- March 2022 (mean I15 = 19.01 mm/hr and max I15 = 22.35 mm/hr), 7 events during the November 2022- March 2023 wet season (mean I15 = 19 mm/hr, max I15 = 24.38 mm/hr), 1 event in the dry season of August 2023 (I15 = 18.28), and 2 events in February 2024 (mean I15 = 17.65 mm/hr, max I15 = 19.03 mm/hr).”

3.3 Differenced Normalized Difference Vegetation Index (NDVI). At line 145, dNDVI is presented as the difference between post and pre. While Table 1 clarifies that pre-images are from the previous year of interest, this may be confusing for the reader. I suggest rephrasing this section to better describe the timing of pre- and post- imagery.

Thank you for the comments. We added this point in the main text and supplement. We now classify pre- and post-fire as before and after the fire. Whereas “reference” and “subsequent” images are the date pairs.

Lines 242-244 (Section 3.3): “We estimated the vegetation changes using high-resolution satellite imagery from pre-fire to the fourth year. We acquired PlanetScope (3 m) images and calculated the Normalized Difference Vegetation Index (NDVI) (Equation 1) within the basins and estimated the differences between two dates of interest, a reference and a subsequent image, to estimate the vegetation changes and possible soil mobilization (i.e., debris flows) (Equation 2). Here we classify “pre” and “post” imagery as the before and after the Lake Fire. “Reference” imagery represents the earlier-date image (which may be before or after the fire) and “subsequent” represents the later-date image.”

3.4 Numerical modeling inputs. The model used should be described better.

Thank you for the comment. We included a more detailed description in the methods and supplement (Section S4).

Lines 312-329 (Section 3.4): “We used a modeling approach to allow us to investigate the effects of the measured parameters on PFDF initiation, regardless of whether debris flows actually occurred within the study area. We used the KWAVE numerical model (Rengers et al., 2016b; Rengers et al., 2019), which uses the kinematic wave approximation for overland flow routing, the Green-Ampt equation for infiltration, and the Rutter et al. (1975) for canopy interception, to simulate runoff discharge. We then used a slope-dependent dimensionless-discharge numerical model to estimate the rainfall intensity-duration (ID) needed to trigger runoff-generated debris flows based on simulated infiltration, interception, and runoff across a catchment’s topography (Tang et al., 2019b). In this study, we varied the input parameters, including the unsaturated hydraulic conductivity, vegetation cover, grain size, and rainfall intensity, while maintaining other parameters similar across basins and years (Supplement Section 4). A complete list of input parameters used in our models can be found in Table 2 and in the supplement (Section 4 and Table S2).”

Supplement Section 4: “We use a numerical model, KWAVE from the Community Surface Dynamics Modeling System (CSDMS) (<https://csdms.colorado.edu/wiki/Model:KWAVE>) as described in Rengers et al., (2016) and Rengers et al., (2019), to simulate runoff and debris flow initiation. KWAVE uses the kinematic wave approximation for overland flow and Green-Ampt equation to represent infiltration and canopy interception (Rutter et al., 1975).

We modified only the input parameters that correspond to our field and satellite measurements as described in Section 3.4. All remaining parameters have been kept at the default values as measured from the Fish Fire (e.g., Hoch et al., 2021): the hydraulic roughness parameter ($n_0 = 0.1$), canopy interception parameters ($S_i = 0.00044$, $g_i = 525$, $K_i = 0.000012$), and Manning’s

roughness = 0.1, exponent in roughness model (epsilon = -0.33) and the critical depth in roughness model ($h_0 = 0.003$).

We also used a suction head (h_f) = 0.01 m and initial volumetric soil moisture content, $\theta_i = 0.1$ and soil moisture at saturation ($\theta_s = 0.41$) derived from Carsel & Parrish (1988) when assuming loamy sand, which is used for our hydraulic conductivity measurements (K_s). However, we assigned a $\theta_s = 0.30$ to represent an extreme lower-bound case for this study. Similarly, we set a constant Manning's roughness to better understand the specific changes from the parameters described in the main text; however, we propose that our assumption here is the lower bound and that Manning's roughness could increase in subsequent years."

Line 179 - You assumed a fractional vegetation cover of 0 immediately after the fire (Year 0). Why not use dNDVI to estimate the vegetation cover for this year as well?

Thanks for the comment. This value is indeed from the dNDVI where there is no vegetation (**Figure 2**), which we then used "0". We clarified this in the caption of Table 2.

"Table 2: Input parameters for our dimensionless discharge model over time. dNDVI used for vegetation cover; see the methods section. 45 months since the fire (Year 4) is considered the wet season, and 50 months since the fire (Year 4) or 5 months after the wet season months later is considered the dry season.

^a denotes values used from McGuire et al. (2021). We there are no vegetation cover immediately after the fire (Year 0). Basin C does not have subsequent measurements due to accessibility issues."

Line 249 – it is not clear how these results were obtained. The method section focuses heavily on dNDVI but provides little detail on precipitation intensity analysis. Thus, these results are without sufficient methodological context. Is PFDF initiation based only on intensity? What about rainfall duration? Is seasonality affecting these thresholds?

Thank you for the comment. We added more information about the modeling methods (see response above). We did not vary the rainfall duration, as rainfall intensity is a stronger predictor for debris flows (Staley et al., 2017; McGuire et al., 2021a). Thus, we maintained the same rainfall pattern for all the models to directly test each variable's effect on the modeled PFDF likelihood.

Line 266 – I15 of 80 mm hr⁻¹ needed to trigger PFDF in the dry season. This value is presented for the first time in the discussion, it should be moved in the result section.

Thank you for the suggestion. The sentence has been moved to the results:

Lines 598-600 (Section 4.1.4): "Simulation results further indicate that at least $I_{15} = 80$ mm/hr is needed to trigger PFDFs in the dry season for basins B, D, and E, which correspond to the I_{15} associated with a 100- to 200-year precipitation event (NOAA, last accessed July 2025)."

Line 266 – highly variable rainfall ID threshold among adjacent basins. I can't find where this variability is shown or quantified.

Thank you for the comment; we noticed that this point might not be clearly expressed, and we have removed this point.

Line 296-304 - A sensitivity analysis is discussed, but not introduced in the methods or presented in the results section. This makes it difficult to understand how it was performed and which variables are analyzed. All analyses should be described in the methods section and presented in the results sections before introducing them in the discussion.

Thank you for the comments. We added more details about the sensitivity test in the methods and discussion.

Lines 353-360 (Section 3.4): “Additionally, we performed a sensitivity test to better understand how each parameter influences the PFDF likelihood of each basin using the dimensionless discharge model (Tang et al., 2019a) (discussed in Section 5.2). More specifically, we vary the individual parameters in our model input (i.e., hydraulic conductivity, vegetation cover, and grain size) either one at a time or changing more than one parameter simultaneously at different rain intensities (i.e., $I_{15} = 10, 20, 30$, and 40 mm/hr). All simulations started with the baseline parameters based on the field surveys and assumptions immediately after the fire: a hydraulic conductivity of 10 mm/hr, no vegetation cover (0%), and median grain sizes for each basin from the first-year sample.”

Line 315 – This statement should be moved in the result section.

We have moved this to the results Section 4.1.

Lines 455-458 (Section 4.1): “We found an overall decrease in median grain size from the first survey was larger than in the subsequent surveys. We also did not observe significant changes in the median grain size between the second and third survey (Table 2).”

Line 335- 337 – This statement should be moved in the result section.

We have moved this to the results Section 4.1.

Lines 605-608: “The results of the sensitivity tests for each basin at different rain intensities to isolate the effects of hydraulic conductivity, grain size, and vegetation cover on post-fire debris flow triggers are discussed in Section 3.2 (Figure 6; Table 4).”

Figures

Figure 3 – I suggest removing the date boxes and instead including the dates directly in each panel title. If you choose to keep the boxes, please clearly explain which dates correspond to pre-

and post-fire conditions. Since “pre” and “post” are commonly used to indicate pre- and post-fire periods, alternative wording may reduce confusion. Also, the date box is missing in Figure 3d.

Thank you for the suggestion; we made the changes as described above. We decided to keep the box but changed the panel titles to indicate post-fire.

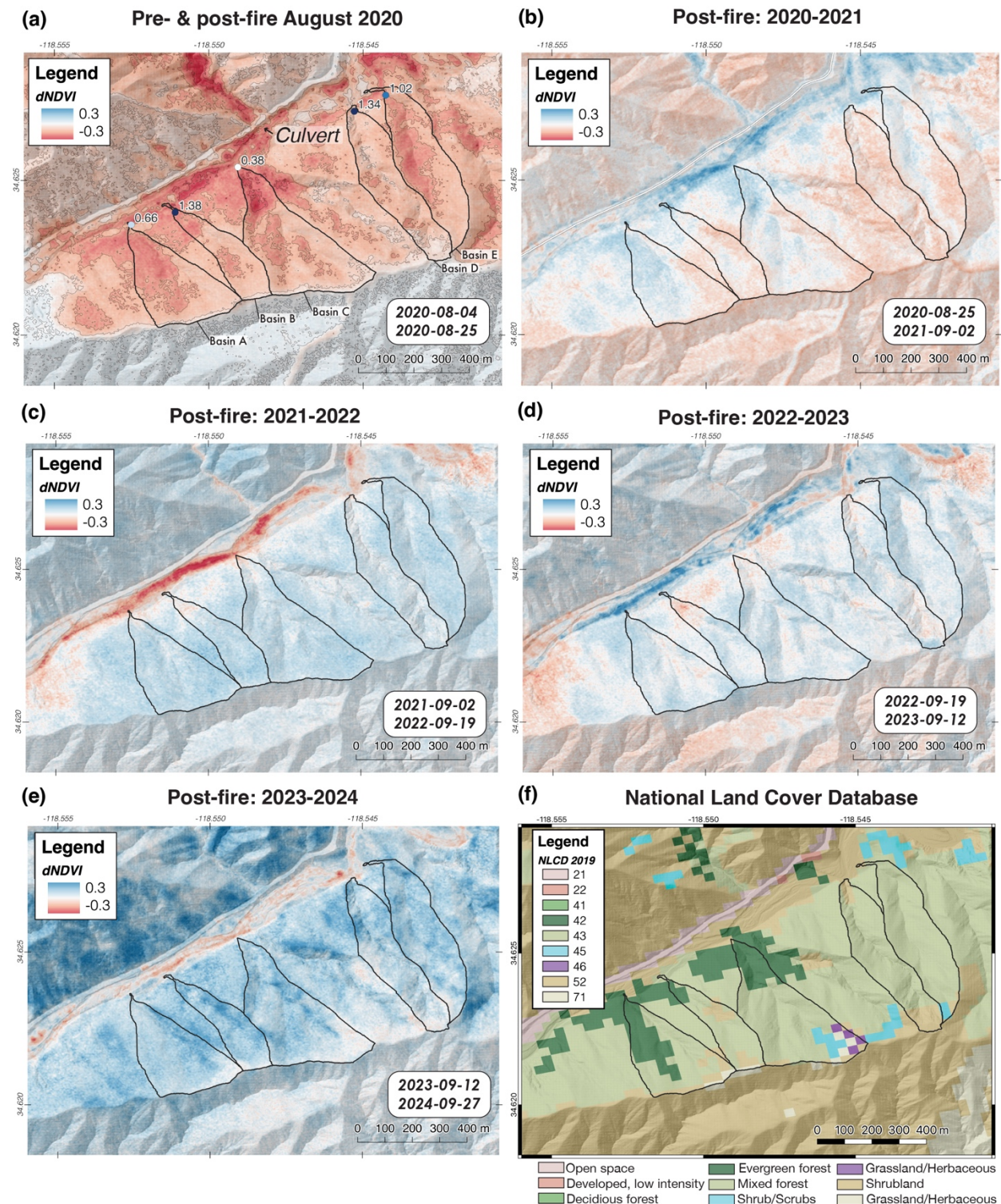


Figure 3: dNDVI of the study sites (a) changes immediately after the fire, (b-e) the subsequent 4 years, and (f) vegetation classification from NLCD2019 (Yang et al., 2018). Cooler colors indicate an increase in the green index (i.e., vegetation), and warmer colors indicate a loss of the green index (i.e., soil exposure, drying vegetation). The grain sizes of each of the first field surveys are indicated by the circles labeled with the median grain size in mm (d50).

Figure 4 – I don't understand what the small numbers near watershed outlets represent. Are these median grain sizes? Table 3, referenced in the caption, does not appear to include grain-size information.

We updated the caption in Figure 4.

“Figure 4: dNDVI of the two field surveys (April 2024 and September 2024) post-September 2023. The bottom panel shows a close-up view of the basin outlets. Values within (a) and (b) represent the median grain sizes (mm) shown here are listed in Table 2.”

As for the grain-size information, we included it in Table 2. Table 3 now only includes the results from the modeling.

Figure 5 – The caption is confusing and should be rewritten for clarity.

We updated the caption and increased the font size in the axes for Figure 5 (see Reviewer #1 response).

Figure 6 – the variables shown are difficult to interpret. Please describe them more clearly in both figure and caption.

Thank you for the suggestion; we updated the caption:

“Figure 6: Sensitivity analysis showing how vegetation cover, grain size, and hydraulic permeability affect debris flow initiation across different slopes and 15-minute rainfall intensities (I15). Warmer colors indicate a higher percentage of slopes (% out of 90 degrees) that exceed the debris flow initiation threshold. Percentages do not reach 100% because the maximum slope in each basin is less than 90°. The baseline model uses parameters from the first survey, 3 months after the fire. The rows indicate the different parameters changed from the baseline. For example, Ks25 indicates a hydraulic conductivity of 25 mm/hr (baseline: 10 mm/hr), FullVeg indicates 100% vegetation cover (baseline: 0%), and Finer Grain Size (FinerGs) indicates a reduction of grain size by 50% relative to the baseline. See Table 4 for the description of parameters for each model.”

Figure 7 – is this figure necessary? It is introduced only in the discussion and appears to be connected with future research rather than this study.

We intend to use the final figure to summarize this research and motivate future research into how short-term trends and long-term trends could influence the PFDF likelihood. We also updated the figure caption accordingly.

“**Figure 7:** Summary time-series of dNDVI changes (a-d) and conceptual time-series model of (e) NDVI changes over time, along with the dNDVI panel above, and (f) proposed non-monotonic hydraulic conductivity changes over time.”

Tables

Table 1 – Year 0 appears to be missing.

Thank you for pointing this out. We updated the table header to indicate “Years since fire” rather than the year the images were taken, since this shows the changes between NDVI pairs (dNDVI).

Years since fire	Predate	Postdate
Pre-Fire	2020-08-06	2020-08-25
1	2020-08-25	2021-09-02
2	2021-09-02	2022-09-19
3	2022-09-19	2023-09-12
4	2023-09-12	2024-09-27
4-Wet	2023-09-12	2024-04-21
4-Dry	2024-04-21	2024-09-27

Table 2 – Please clarify whether D50 represent an average? Consider to add “Wet” and “Dry” after the month and year in column “Months since fire”.

Thank you for the comment; it does represent the mean, and we included the wet and dry months in the table.

Basins	Months since fire (MM/YY)	Hydraulic Conductivity (mm/hr)	Vegetation Cover (fraction)	Pi (Throughfall coefficient)	Average d ₅₀ (mm)	Average d ₈₄ (mm)
A	3 (Dec 2020)	10 ^a	0	1*	0.66	1.77
	12 (Sep 2021)	10 ^a	0.57	0.43	0.66	1.77
	45 (Apr 2024) Wet	34	0.99	0.01	0.1	0.24
	50 (Sep 2024) Dry	22	0.0047	0.99	0.11	0.34
B	3 (Dec 2020)	10 ^a	0	1*	1.38	2.45
	12 (Sep 2021)	10 ^a	0.29	0.71	1.38	2.45
	45 (Apr 2024) Wet	24	0.99	0.01	0.08	0.26
	50 (Sep 2024) Dry	405	0.018	0.99	0.09	0.39
C	3	10 ^a	0	1*	0.38	0.9

	(Dec 2020)					
	12 (Sep 2021)	10 ^a	0.47	0.53	0.38	0.9
	45 (Apr 2024) Wet	-	0.98	0.02	-	-
	50 (Sep 2024) Dry	-	0.97	0.03	-	-
	3 (Dec 2020)	-	0.054	0.99	-	-
D	3 (Dec 2020)	10 ^a	0	1*	1.34	2.45
	12 (Sep 2021)	10 ^a	0.03	0.97	1.34	2.45
	45 (Apr 2024) Wet	22	0.97	0.03	0.1	0.27
	50 (Sep 2024) Dry	576	0.13	0.87	0.05	0.2
E	3 (Dec 2020)	10 ^a	0	1*	1.02	1.77
	12 (Sep 2021)	10 ^a	0.26	0.74	1.02	1.77
	45 (Apr 2024) Wet	31	0.99	0.01	0.085	0.25
	50 (Sep 2024) Dry	157	0.049	0.99	0.086	0.32

Table 2 - Please correct in the caption 50 with 5.

Thank you for pointing this out. We updated the caption as shown earlier in the response.

Table 4 – Similar to Figure 6, the variables listed need to be explained.

Thank you for pointing this out. We added the explanation.

“**Table 4:** Description of the parameters changed for the sensitivity test. The baseline grain sizes are from the first survey in 2020. Simulation results from the three field surveys are shown in Figure 6, S5, and S6. Baseline: reference parameters from the first field survey; FullVeg: only increased the vegetation cover by 100% (1); Ks25: increasing the hydraulic conductivity to 25 mm/hr; FinerGs: Grain size reduced by half (50%).”

Supplementary material

Table S2 is very similar to Table 2, why you don't merge them? Same for Table S3 and Table 3

Thank you for the suggestion; we removed Tables S2 and S3 to avoid confusion with the same information. Previously, Table S2 and S3 also included the properties in the years without a field

survey. Now Table S2 shows the parameters for the modeling and Table S3 is the sensitivity test parameters based on the first field survey.

Symbol	Definition	Unit	Value
n0	Hydraulic roughness coefficient	s m ^{-1/3}	0.1
hc	Critical depth in roughness model	m	0.003
ε	Exponent in roughness model	–	–0.33
hf	Wetting front suction head	m	0.01
θi	Initial soil moisture	–	0.1
θs	Soil moisture at saturation	–	0.30
gi	Decay parameter	m ⁻¹	525
Si	Canopy storage capacity	mm	0.44
Ki	Drainage rate coefficient	m s ⁻¹	1.2 × 10 ⁻⁵

Table S2: Summary table of the parameters that are fixed across all the basins and years. See Section 4 text for detailed descriptions.

Bas ins	Inte nsity	Base line	Model 1Veg	Model 2Ks25	Model3K s25Veg	Mode l4Gs	Model5 KsGs	Model6 GsVeg	Model7K sGsVeg
A	15	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
A	20	0.00	0.00	0.00	0.00	0.19	0.00	0.17	0.00
A	30	0.17	0.14	0.01	0.00	0.71	0.68	0.70	0.16
A	40	0.71	0.69	0.68	0.14	0.72	0.70	0.71	0.71
B	15	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
B	20	0.00	0.00	0.00	0.00	0.01	0.00	0.01	0.00
B	30	0.01	0.01	0.00	0.00	0.63	0.01	0.63	0.00
B	40	0.01	0.01	0.01	0.01	0.68	0.26	0.67	0.23
D	15	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
D	20	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
D	30	0.00	0.00	0.00	0.00	0.63	0.00	0.19	0.00
D	40	0.00	0.00	0.00	0.00	0.63	0.61	0.63	0.21
E	15	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
E	20	0.00	0.00	0.00	0.00	0.01	0.00	0.01	0.00
E	30	0.01	0.00	0.00	0.00	0.24	0.13	0.21	0.01
E	40	0.19	0.17	0.01	0.00	0.69	0.26	0.68	0.22

Table S3: Sensitivity test using grain sizes from first field sampling. Values in tables show the percentage change with slopes exceeding the threshold to cause runoff-generated debris flows. The matrix plot is shown in **Figure 6**.

Additional comments:

We also increased the font size in the supplementary figure and caption (Figure S2).

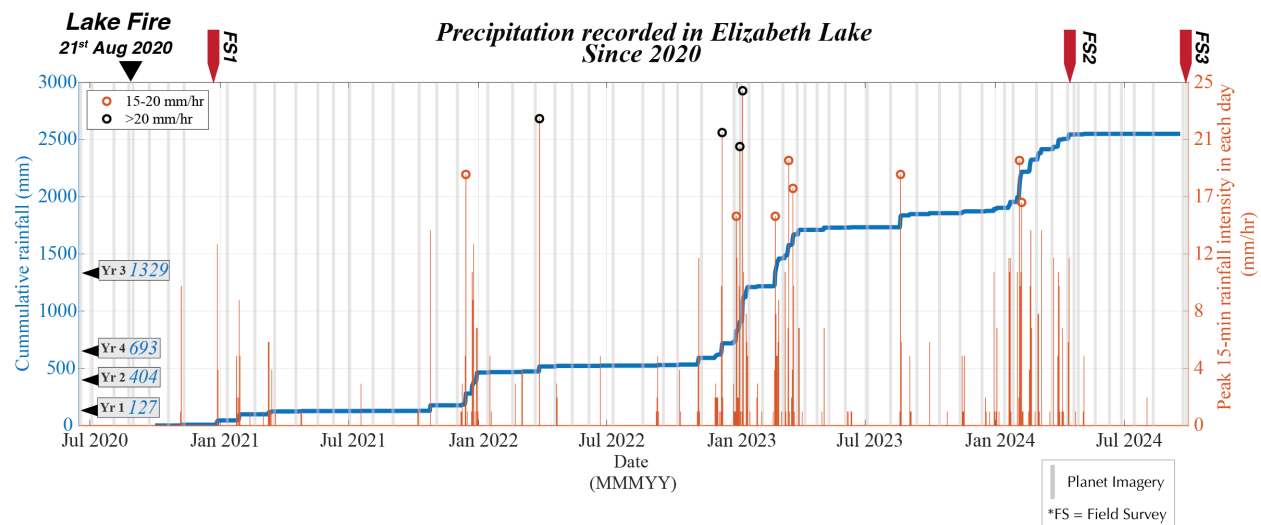


Figure S2: Record of cumulative precipitation in blue graph (blue axis) and the daily peak rain intensity (orange axis). Orange circles represent rain intensity between I_{15} of 15-20 mm/hr, and dark circles are I_{15} greater than 20 mm/hr. FS represents field survey. Grey lines represent the dates with PlanetScope images. See **Table S5** for the dates exceeding the $I_{15} = 15$ mm/hr. The overall rainfall recorded for the first, second, third, and fourth years was 127.5 mm, 404.1 mm, 1329 mm, and 693.7 mm, respectively

Review response to 13-Jan-2026 (RC3)

Reviewer Comments

Black – reviewer; Blue – response

Overall, the manuscript is well written and easy to comprehend.

The authors apply a hydrologic model to calculate discharge and then convert it into a dimensionless unit discharge. The dimensionless discharge is then compared to a 15-min dimensionless-discharge rainfall ID threshold. Input parameters used in the hydrologic model included the use of mini disk infiltrometer data to estimate hydraulic properties of soil, remote-sensed metrics such as NDVI and dNDVI to estimate changes in vegetative cover, and sediment grain size to estimate roughness and control critical Shields number/shear stress used in developing model-derived ID thresholds. Two main findings reported by the authors include 1) unsaturated hydraulic conductivity increased by an order of magnitude between the wet and dry season four years post fire, suggesting unsaturated hydraulic conductivity is a significant factor in post fire debris flow (PFDF) occurrence, and 2) vegetative cover and grain size provided less influence compared to hydraulic conductivity in controlling PFDF occurrence.

Although the results of this study do show some similarities with other studies, the methods applied lack scientific rigor with the use of only a few data collected in the field to fully test influences of hydraulic conductivity and changes in vegetative cover. For example, the finding that seasonal variability in unsaturated hydraulic conductivity is a significant factor appears to be based on mini disk infiltrometer measurements taken at the outlet of four basins, with three of the four basins showing an increase in hydraulic conductivity from the wet to dry period. Mini disk infiltrometer measurements are known to exhibit large variability between measurements at the same location and typically vary spatially across the landscape. Therefore, relying on just a few values of hydraulic conductivity measured at the outlet of basins is not advised and additional data would be required.

Moreover, the authors rely on dNDVI to approximate changes in vegetative cover. NDVI is known to be sensitive to seasonal variations in vegetative cover, largely as a function of vegetative health and the natural cycle of plant growth. For example, plants actively growing in the spring will have NDVI values approaching 1 compared to the same plants that go dormant and dry out during the late summer. Thus, NDVI as a sole predictor of vegetative cover is not recommended. Instead, use of a combination of indices including NDVI, LAI, and vegetative water content has been demonstrated to provide better predictors of vegetative cover, particularly when used to inform hydrologic models (Lahmer et al, 2025; Thomas et al, 2021; Nourani et al., 2017). Regardless of the remote-sense metrics used, it is important to validate measured values against in-field observations, including vegetative type, density, height, and ground cover.

Based on the limitations and findings contained within this study, it is difficult to except it for publication unless substantially changed.

Please see comments below for additional context

Lines 90-102: The authors describe making field measurements at the outlet of five basins that include taking samples for grain size analyses and performing mini disk infiltrometer measurements. In reading the text, it appears the sediment samples were taken from the channel and represent sediment that was transported from source areas above. It also appears the mini disk measurements were performed in the channel adjacent to where the soil samples were collected. If this is the case, then the mini disk measurements were made on deposits of transported material (e.g. alluvium/colluvium within the channel) and may not be representative of in-situ soils across the basin that would control infiltration and runoff. A more acceptable approach would have been to conduct far more and spatially dispersed measurements across the basin.

Thank you for the comment. We responded to a similar comment from Reviewer #1 about our field measurements and made the following changes.

Lines 155-210 (Section 3.1): “We measured unsaturated soil hydraulic conductivity for the second and third field surveys using a Mini Disk Infiltrometer (METER Group) adjacent to our soil samples within the basin outlet without disturbing the surface material (i.e., flattening) (METER, 2021; Watson et al., 1986). We only report the initial hydraulic conductivity values for each basin, as there is a limited undisturbed area with a suitable flat area. During the first field survey, due to the lack of instruments and access, we assumed a hydraulic conductivity of 10 mm/hr as a conservative estimate based on data from the 2016 Fish Fire in the adjacent San Gabriel Mountains with no vegetation cover (McGuire et al., 2021a). We also performed a Water Droplet Penetration Test (WDPT) at the basin outlets during this survey. The 2016 Fish Fire occurred in an area with similar granitic bedrock, average slope (46°-51°), northwest-facing aspect, and vegetation types (Table S7) as compared to the 2020 Lake Fire (McGuire et al., 2021a; Paysen et al., 1980; Staley et al., 2018). We also collected soil core samples (down to 20 cm) during the second and third surveys and visually inspected for stratification and performed.”

Watson, K. W. and Luxmoore, R. J.: Estimating macroporosity in a forest watershed by use of a tension infiltrometer, Soil Sci. Soc. Am. J., 50, 578–582, <https://doi.org/10.2136/sssaj1986.03615995005000030007x>, 1986

METER Group, Inc. USA., Mini Disk Infiltrometer Operator's Manual, 2021

Additional information on the method applied to perform the grain-size analyses is needed. For example, were the samples run in the lab using an established method such as ASTM D422? If so, please state the standard method that was applied. The grain size listed in Table 2 and Figures 3 and 4, suggest grain size with a D50 as low as 0.08mm. It implies that hydrometer tests were performed to quantify the grain size distribution. Grain sizes equivalent to silt and clay are not frequently encountered in channels where fluvial process occur. Such fine grains would

dramatically influence the dimensionless discharge and the critical dimensionless discharge used to set the ID rainfall threshold. Please elaborate.

Thank you for the suggestion; we added this information in the supplementary material (Section 1).

Supplement Section 1: “We used ASTM E11 test sieves with mesh size 2 mm, 1 mm, 0.5 mm, 0.25 mm, 0.125 mm, 0.063 mm. Sieves were stacked and shaken on a RO-TAP sieve shaker.”

Please state or reference the standards that were applied in performing the mini disk infiltrometer tests. If you followed the manufacturer’s standards, please state so and include a reference. Also, please state the suction head applied and if you used a thin layer of silica bedding sand to ensure good contact with the mini disk.

We performed measurements with a suction head of 1 cm without silica bedding, as we had good contact between the mini disk and sediment. We added the tension infiltrometer reference and the METER’s User Manual. See the response above for changes in the methods section.

As discussed, it does not appear more than one measurement was performed at each of the basin outlets for each sampling period. If this is incorrect, please indicate the number of tests performed and show the results of each test in the supplement.

This is correct. We have responded to a similar comment above.

Year 0 mini disk measurements were not conducted. Instead, Year 0 results were assumed by extrapolating results obtained on the Fish Fire. Additional information justifying the use of values obtained on the Fish Fire should be done. For example, bedrock type, soil type, slope aspect, slope gradient, pre-fire vegetation, and post-fire soil burn severity should all be shown to be relatively similar to apply the Fish Fire results to the Lake Fire study area.

We did not have the Mini Disk Infiltrometer for the first field survey; thus, we assumed a value based on the similarity with the 2016 Fish Fire, given the shared similarity in granitic bedrock, average slope for all basins is 30 degrees with all aspect NW facing Lake Fire (Fish Fire has slope ranging between 46 to 51 degrees), and vegetation before the fire. During the first survey, we also observed strongly repellent soil from the Water Droplet Penetration Test, which led us to use a more conservative Ks value of 10 mm/hr. We also responded to a similar comment from Reviewer #1’s and see the response above for changes in the methods section. We also added more description of the land cover dataset in the supplement as follows.

Supplementary Section 1: Using National Land Cover Dataset, both 2020 Lake Fire and 2016 Fish Fire have similar categories of Shrubs/Scrub. The entire Fish Fire is dominated by California mesic and dry-mesic chaparral, mixed evergreen woodland and mixed conifer and woodland, and montane grasslands (Staley et al., 2018). This is similar to the Lake Fire sites within the Los Angeles National Forest area where Chaparral also dominates (Paysen et al., 1980).

Staley, D. M., Tillery, A. C., Kean, J. W., McGuire, L. A., Pauling, H. E., Rengers, F. K., & Smith, J. B. (2018). Estimating post-fire debris-flow hazards prior to wildfire using a statistical analysis of historical distributions of fire severity from remote sensing data. *International Journal of Wildland Fire*, 27(9), 595–608. <https://doi.org/10.1071/WF17122>

Paysen, T. E., Derby, J. A., Black, H., Jr., Bleich, V. C., & Mincks, J. W. (1980). *A vegetation classification system applied to southern California*. USDA Forest Service, Pacific Southwest Forest and Range Experiment Station, General Technical Report PSW-45. Berkeley, CA, USA. 39 pp. https://www.fs.usda.gov/psw/publications/documents/psw_gtr045/psw_gtr045.pdf

Lines 110-113: It is unclear what “15-minute duration threshold” is being referred to here. Is it the fire-wide USGS I15 threshold of 24mm/hr used to estimate debris flow likelihood? Or is it basin specific thresholds derived by the USGS assuming a probability of occurrence of 50%. Or is it basin specific derived using the critical dimensionless discharge model? If the “threshold” being referred to is associated with the USGS model, then it would only apply for years 0-1, since the USGS model does not predict thresholds after year 1. Also, Table S7 and Figure S2 don’t refer to “thresholds” in the sense of triggering thresholds. Table S7 simply shows the dates that rainfall exceeded 15mm/hr in 15 minutes and Figure S2 shows peak I15 rainfall events that exceed 20mm/hr.

Thank you for the comment. The 15-minute duration threshold corresponds to the threshold calculated in the dimensionless discharge model (red lines in Figure 5). The threshold is based on the lower limit from Tang et al., (2019a). We clarified this in the methods:

Lines 217-222: “To evaluate whether each precipitation event can trigger debris-dominated flows or water-dominated flows, we report the peak 15-minute rainfall intensity (I15) for each year. The threshold used to distinguish between these flow types is based on the lower limit of the dimensionless discharge threshold in Tang et al. (2019a). We also apply the same threshold when reporting our modeling results as a conservative estimate to trigger debris flows.”

Thank you for pointing out the table and figure. Indeed, these show the peak precipitation intensity recorded rather than the triggering threshold from the modeling approach. Since none of the observed rain intensities exceeded the modeling threshold, we only list dates with an $I_{15} = 15$ mm/hr and higher that could potentially create debris-dominated flows (i.e., Basin A Year 4 Dry).

Note that Table S7 is now Table S6, and we have updated the header.

Dates	Peak 15-min duration rain intensity (I_{15}) (mm/hr)
14 th December 2021	18.28
28 th March 2022	22.35
11 th December 2022	21.36
31 st December 2022	15.24
5 th January 2023	20.32

9 th January 2023	24.38
24 th February 2023	15.24
15 th March 2023	19.30
21 st March 2023	17.27
20 th August 2023	18.28
4 th February 2024	19.03
7 th February 2024	16.26

Table S6: Days with rainfall intensity exceeding $I_{15} = 15$ mm/hr. The table also shows the dates with the respective peak I_{15} .

Tang, H., McGuire, L. A., Rengers, F. K., Kean, J. W., Staley, D. M., and Smith, J. B.: Developing and testing physically based triggering thresholds for runoff-generated debris flows, Geophys. Res. Lett., 46, 8830-8839, <https://doi.org/10.1029/2019GL083623>, 2019a

Section 3.4, starting on Line 158: Additional information is needed to explain the hydrologic model used. It is understood that the authors applied the Green-Ampt formula for infiltration and Kinematic Wave formula for routing using the program KWAVE (Renger et al., 2016), yet there is a lack of detail on how the models were parameterized. For example, additional information is needed to explain how saturated hydraulic conductivity, sorptivity, soil porosity, initial soil moisture, infiltration depth, and Mannings # were obtained and used in the hydrologic model. The list of parameters summarized in Table 2 is insufficient, and the broad generalization of assumed values applied and discussed on lines 190-192 are poorly justified. See methods section in Tang et al., 2019, and McGurie and Youberg, 2020, for examples how to explain the model and parameterize it.

Thank you for the suggestion. We have added more details about the models we used (see our response to Reviewer #2). We recognize that the referenced studies performed a series of calibrations and measurements to parameterize their hydrologic models. However, the focus of our study was to understand how parameters informed by the samples and measurements we obtained would affect debris flow likelihood for this fire, rather than to replicate a fully calibrated hydrologic model. We have clarified in the main text (**Section 3.4**) and supplement (**Section 4 & added Table S2**) which variables were held constant, and which were varied. We also responded to similar comment regarding the objective of this paper to Reviewer #1.

Lines 313-329 (Section 3.4): “We used a modeling approach to allow us to investigate the effects of the measured parameters on PFDF initiation, regardless of whether debris flows actually occurred within the study area. We used the KWAVE numerical model (Rengers et al., 2016b; Rengers et al., 2019), which uses the kinematic wave approximation for overland flow routing, the Green-Ampt equation for infiltration, and the Rutter et al. (1975) equation for canopy interception, to simulate runoff discharge. We then used a slope-dependent dimensionless-discharge numerical model to estimate the rainfall intensity-duration (ID) needed

to trigger runoff-generated debris flows based on simulated infiltration, interception, and runoff across a catchment's topography (Tang et al., 2019b). In this study, we varied the input parameters, including the unsaturated hydraulic conductivity, vegetation cover, grain size, and rainfall intensity, while maintaining other parameters similar across basins and years (Supplement Section 4). A complete list of input parameters used in our models can be found in Table 2 and in the supplement (Section 4 and Table S2)."

Supplement Section 4: We use a numerical model, KWAVE from the Community Surface Dynamics Modeling System (CSDMS) (<https://csdms.colorado.edu/wiki/Model:KWAVE>) as described in Rengers et al., (2016) and Rengers et al., (2019), to simulate runoff and debris flow initiation. KWAVE uses the kinematic wave approximation for overland flow and Green-Ampt equation to represent infiltration and canopy interception (Rutter et al., 1975).

We modified only the input parameters that correspond to our field and satellite measurements as described in Section 3.4. All remaining parameters have been kept at the default values as measured from the Fish Fire (e.g., Hoch et al., 2021): the hydraulic roughness parameter ($n_0 = 0.1$), canopy interception parameters ($S_i = 0.00044$, $g_i = 525$, $K_i = 0.000012$), and Manning's roughness = 0.1, exponent in roughness model ($\epsilon = -0.33$) and the critical depth in roughness model ($h_0 = 0.003$).

We also used a suction head ($h_f = 0.01$ m) and initial volumetric soil moisture content, $\theta_i = 0.1$, and soil moisture at saturation ($\theta_s = 0.41$) derived from Carsel & Parrish (1988) when assuming loamy sand, which is used for our hydraulic conductivity measurements (K_s). However, we assigned a $\theta_s = 0.30$ to represent an extreme lower-bound case for this study. Similarly, we set a constant Manning's roughness to better understand the specific changes from the parameters described in the main text; however, we propose that our assumption here is the lower bound and that Manning's roughness could increase in subsequent years."

Lines 190-192: The authors suggest the use of dNDVI to assess vegetative cover and post-storm response, including sediment transport and deposition associated with debris flows. It is understood that dNDVI can show the location of vegetative-free areas, but to infer flow response, such as flood flow or debris flow, in areas using dNDVI would require field verification.

We could not match the correct lines. However, we agree with the reviewer. We discussed this in the discussion.

Lines 871-872 (Section 5.3): "Additional method validation through field and/or other techniques is needed to ensure dNDVI can accurately detect, delimit, and quantify debris flow features."

Results 4.0, starting on Line 194: It is difficult to understand why the modeled dimensionless discharge have bell-shaped curves with peak discharge occurring at slope angles of about 35 degrees. Typical plots of dimensionless discharge start low at high slope angles and increase at

lower slope angles. These results are suspect and difficult to rationalize. Further explanation is needed.

One possibility is that the slope angle exceeded the soil's angle of repose; steep slopes retain only a small amount of loose sediment, as any loose material would have already eroded before any high-intensity rainfall. A second possibility is that the lack of sediment on the steeper slopes produced unrealistically high discharge values, as seen in the high variability of dimensionless discharge at the high steeper slopes (x-axis). We have added this explanation to the end of the results section.

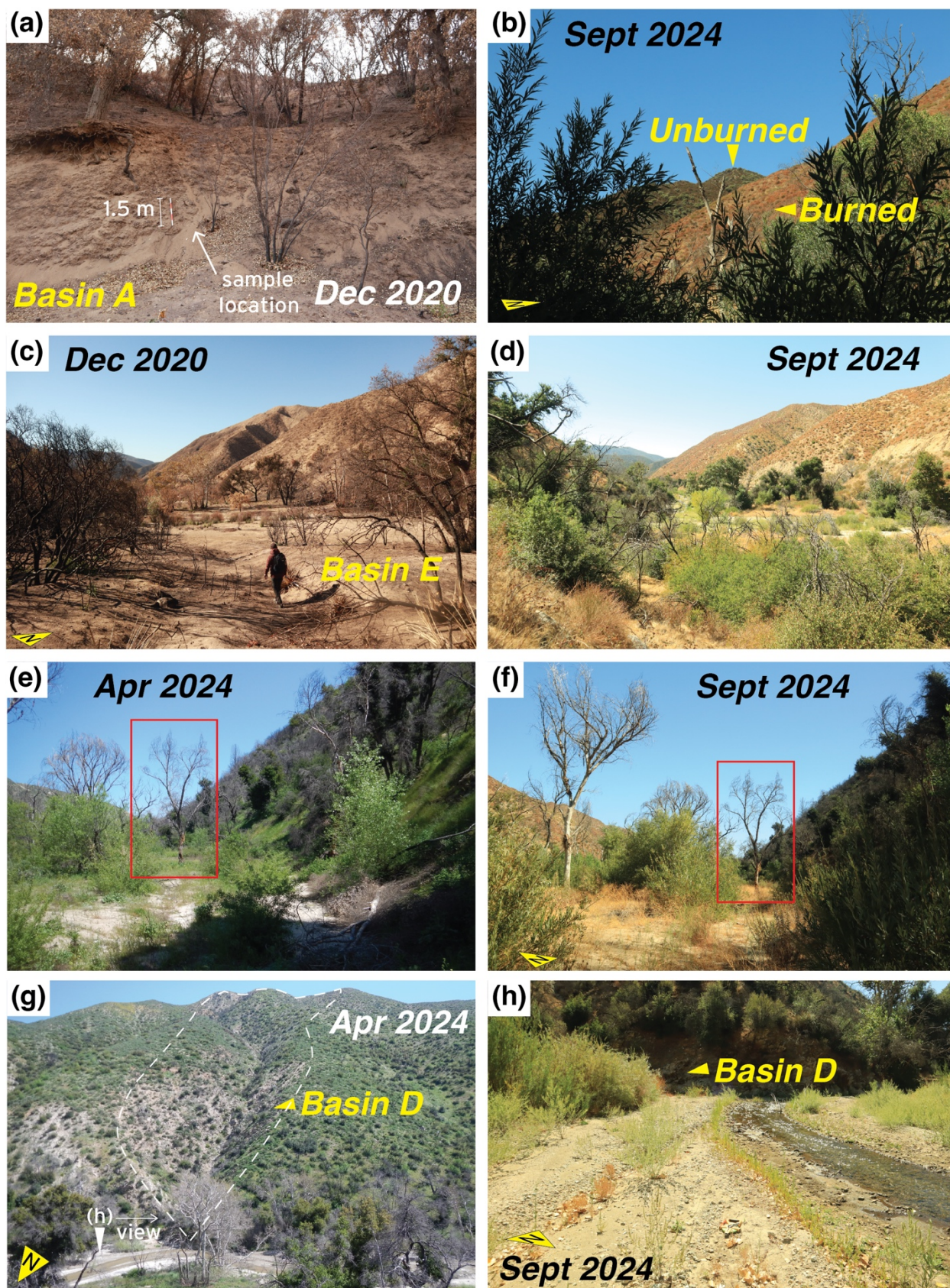
Lines 602-608 (Section 4.1.4): “We suspect that the bell-shaped curves in our modeled dimensionless discharge (Figure 5) result from a lack of loose sediments at steeper slopes (e.g., >45 degrees), where the angle of repose is exceeded (e.g., Statham, 1976). The limited slope data available within the basin may also contribute to the high variability at steeper slopes (e.g., >50 degrees in Basin A). The results of the sensitivity tests for each basin at different rain intensities to isolate the effects of hydraulic conductivity, grain size, and vegetation cover on post-fire debris flow triggers are discussed in Section 3.2 (Figure 6; Table 4).”

Statham, I.: A scree slope rockfall model, Earth Surface Processes, 1(1), 43-62, doi:10.1002/esp.3290010106, 1976

Figure 2: The riparian zone shown in the Dec 2020 photos following fire suggest low burn severity due the lack of consumption and observed intact organic mat on the ground surface in padel (a). The coarse resolution of the BARC map used by the BAER team likely didn't capture the low burn severity in the mainstem channel and within the lower elevations of the basins. Because field observations and data were collected only at the basin outlets, near their intersection with the main channel, it is possible the data is biased towards areas of low fire severity and not indicative of conditions located upslope within the basins.

Thank you for the comment and for pointing out the potential of high-resolution corroborating field observations. Our field measurement/sample sites are immediately at the basin outlets; they are closer to the basin than to the main river channel. Thus, our samples are unlikely to be sampling/measuring the low burn severity closer to the main river channel.

We updated Figure 2 with the location of the sample and added more field photos in the supplement (**Figure S7**). We also made updates to the figure captions. We also want to note that the samples collected at Basin A (Figure 2a) are from the first year only. For the subsequent surveys, we collected at a flatter spot ~1 meter below where we collected (Figure S7).



“**Figure 2:** Field comparison of the three field surveys (December 2020, April 2024, and September 2024). (a) Direct view of the Basin A outlet and the location of soil collection in December 2020. (b) Looking NW from Basin E in September 2024. (c) Looking NW towards the vegetation differences between burned and unburned in September 2024. (d) Basin A in December 2020. (e) Looking NE from Basin A to Basin B in April 2024. (f) Same as (e), but in September 2024, the box identifies the same tree as a reference. (g) Looking south at Basin D in April 2024 from the highway. (h) Looking SW at Basin D with the river of lower height in September 2024.”

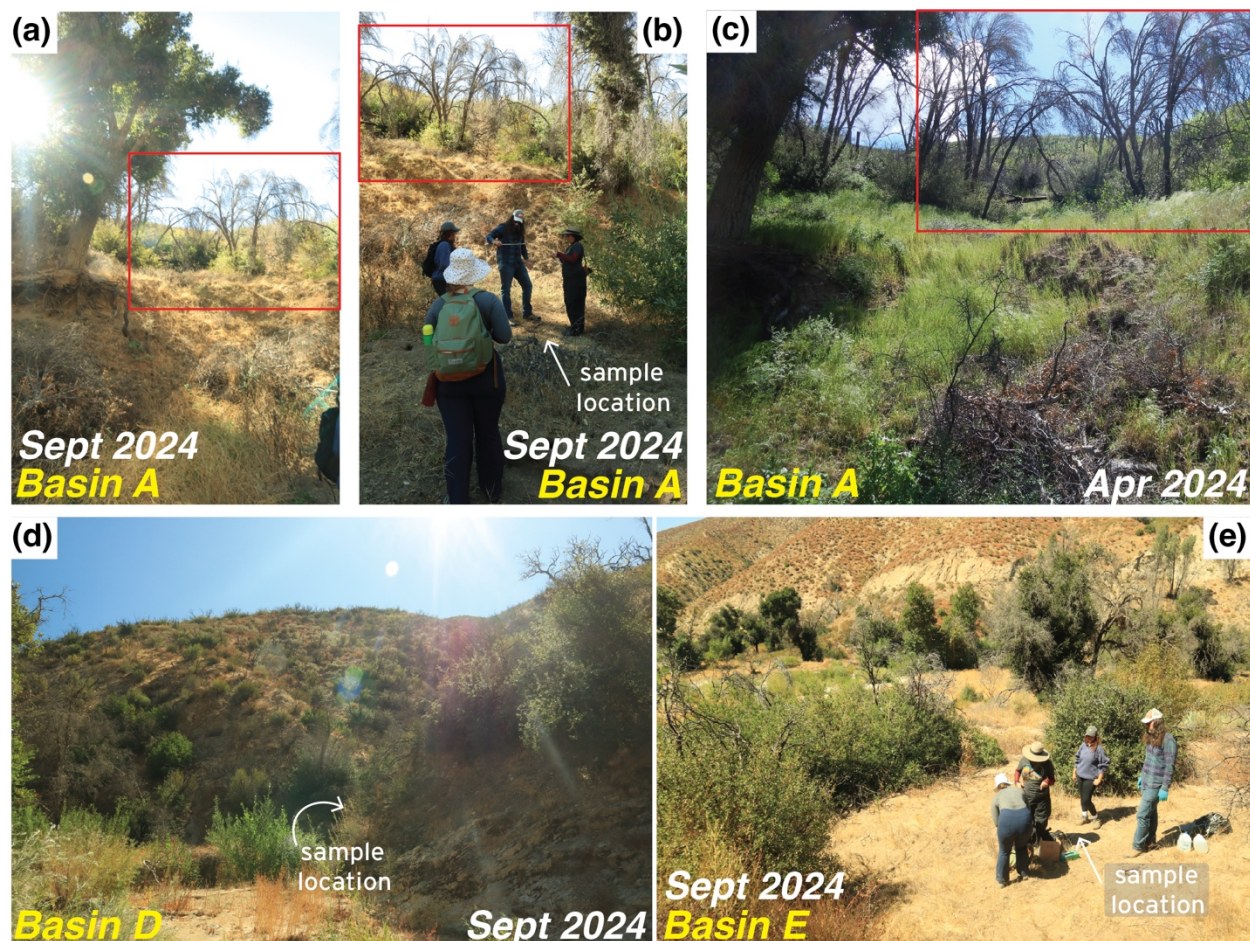


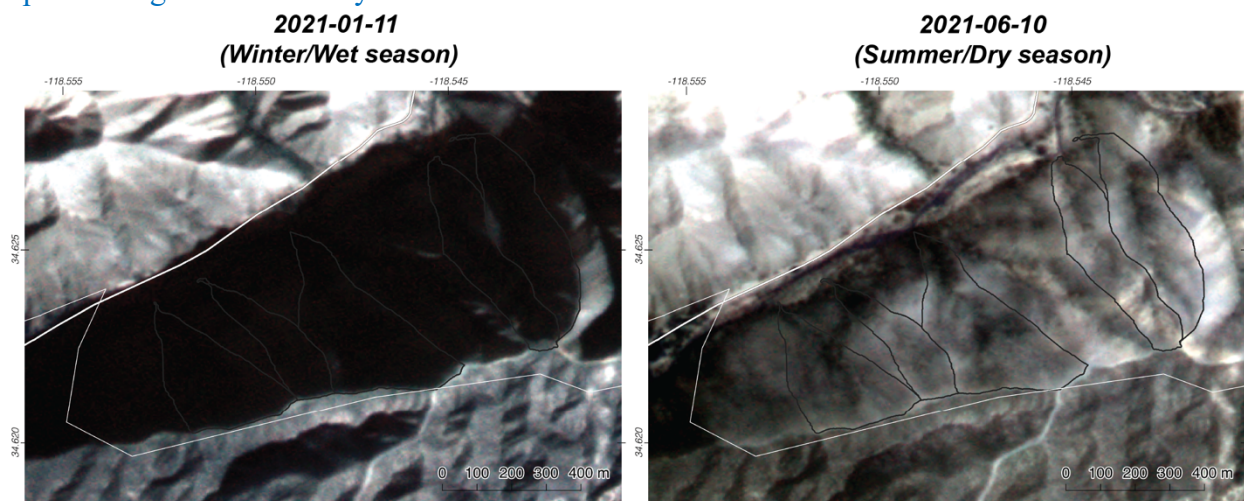
Figure S7: Additional field photos from the 2024 field surveys. At Basin A, the second and third survey sampling locations were approximately 1 meter below the first survey's sampling location, where a flat surface allowed for mini disk infiltrometer measurements. See Figure 2a for the corresponding 2020 (first) survey location, which did not include hydraulic conductivity measurements.

Figure 3: In looking at the shades of blue and red in areas outside the burn scar, it appears that seasonal changes in vegetative cover may not have been accounted for and may mislead the interpretation of vegetative recover inside the burn scar using dNDVI. For example, areas outside the burn perimeter in panel a show no change with the color being light grey to white. This is expected. However, panels b, c, d, and e all show some level of change occurring within the unburned areas due to seasonal variations that are likely due to soil/vegetation moisture content or vegetation health. To correct this, the average observed dNDVI values in unburned areas should be subtracted from the values inside the burned areas.

Thank you for the comment and the suggestion to normalize the burned areas using values from the unburned area. The dNDVI values shown here are raw values calculated directly from the bands, which we used to illustrate the large difference between burned and unburned areas in the image.

We agree that normalizing to values outside the burn scar would be ideal, but we were unable to apply this correction reliably, for two main reasons. First, shadow effects are stronger in higher-resolution imagery: ridges with different aspects show different dNDVI values, and incorporating unburned dNDVI values from these areas could bias the normalization. Second, using all unburned areas within the image would require accounting for inconsistent acquisition timing and sun angle across images due to seasonality. Correcting for these effects would require additional technical development beyond the scope of this study. We therefore report and discuss dNDVI values only within the basins.

See example figure below (not included in the main text or supplement): comparison between optical images from January 2021 and June 2021.



Panel d appears to be missing the inset indicating the dates of images used in the differencing analysis.

Thank you for pointing this out; we added the missing dates in Figure 3.

Please provide units for the D50 grain size.

Thank you for pointing this out. We updated the figure in response to a previous comment from Reviewer #2.

Figure 4: It is relatively apparent that the dNDVI method is a good metric of evaluating vegetative health, but it may not be a good metric to indicate the overall vegetative density and cover present within the basins. Additional field methods evaluating type and density of vegetative cover present on the hillslopes would be needed to better interpret the dNDVI data.

Thank you for the comment and we agree. We responded to a similar comment by Reviewer #1.

Lines 808-813 (Section 5.2): “We also note that satellite-derived vegetation cover used in our model input can include shrubs or grasses in areas without tree cover. Areas with high tree cover presence can obscure the understory. Therefore, the vegetation cover here does not necessarily represent understory density and surface roughness, and canopy interception can vary depending on the vegetation type (Zhao et al., 2019).”

Please provide units for the D50 grain size.

Please refer to Table 2, rather than Table 3, for median grain size.

Thank you for pointing this out. We have corrected it.

Figure 5: Again, the bell-shaped curves depicted in the dimensionless discharge plots against slope are odd and difficult to interpret. Please provide additional discussion explaining the reason for the shape.

We addressed the same comment above.

The red line that represents the modeled critical dimensionless discharge separating flood flow from debris flow does not appear to differ between basins and as recovery occurs. Is this the case, and if so, can it be explained? It is expected the critical dimensionless discharge used to set the ID threshold would vary between basins and increase as recovery occurs.

Thank you for the comment. We used the lower limit of the dimensionless discharge threshold derived by Tang et al. (2019a) from the 2016 Fish Fire, the same approach used by Hoch et al. (2021) across their study sites. Because the threshold is expressed in dimensionless form — a design originally proposed by Gregoretti (2000) and Gregoretti and Fontana (2008) — it is intended as a general, physically based criterion that does not vary between basins or with time since fire. We chose the LL threshold specifically as a conservative estimate for the minimum dimensionless discharge needed to trigger a debris flow. We have clarified this in the main text.

Lines 217-222: “To evaluate whether each precipitation event can trigger debris-dominated flows or water-dominated flows, we report the peak 15-minute rainfall intensity (I15) for each

year. The threshold used to distinguish between these flow types is based on the lower limit of the dimensionless discharge threshold in Tang et al. (2019a). We also apply the same threshold when reporting our modeling results as a conservative estimate to trigger debris flows.”

Gregoretti, C. and Dalla Fontana, G.: The triggering of debris flow due to channel-bed failure in some alpine headwater basins of the Dolomites: analyses of critical runoff, Hydrol. Process., 22, 2248–2263, <https://doi.org/10.1002/hyp.6821>, 2008.

Gregoretti, C.: The initiation of debris flow at high slopes: experimental results, J. Hydraul. Res., 38, 83–88, <https://doi.org/10.1080/00221680009498343>, 2000.

Table 2: It is difficult to understand why the vegetation cover fraction drops significantly from the wet to dry season. I suspect this is due to change in soil/vegetation moisture content, suggesting a near complete reduction in vegetative cover between April and September 2024. This appears unrealistic. There is likely similar vegetative cover between these reported periods, but the vegetation has gone dormant and dried out in September compared to the earlier April period. Again, additional field data would be necessary to identify the type and density of vegetative cover in the basins and help interpret the remote-sensed NDVI products.

Thank you for the comment. Indeed, this seasonal change is substantial, and we believe it is largely driven by the drying of grasses, as seen in the field photos (Figure S7). We also see a clear difference between burned and unburned sites: burned sites show larger seasonal dNDVI changes, consistent with their grass-dominated post-fire vegetation. The unburned sites are dominated by shrubs, which do not dry out as quickly or as often as grasses. Within the burned sites, there are some shrubs and trees that retain green leaves year-round, which we confirmed through both field surveys and the corresponding dNDVI values.

References Cited not already included in manuscript

Lahmers, T. M., Kumar, S. V., Ahmad, S.K., Holmes, T., Getirana, A., Orland, E., et al. (2025). An observation-driven framework for modeling post-fire hydrologic response: Evaluation for two central California case studies. *Water Resources Research*, 61, e2023WR036582. <https://doi.org/10.1029/2023WR036582>

Nourani, V; Fard, AF; Gupta, HV; Goodrich, DC; Niazi, F (2017). Hydrological model parameterization using NDVI values to account for the effects of land cover change on the rainfall-runoff response. *HYDROLOGY RESEARCH*, 48(6), 1455-1473.

Review response to 17-Feb-2026 (RC4)

Reviewer Comments

Black – reviewer; Blue – response

This manuscript addresses an important and interesting aspect of post-fire debris flow (PFDF) science: the seasonal variability of soil hydraulic properties several years after a wildfire. Your finding that hydraulic conductivity can vary by an order of magnitude between wet and dry seasons in the fourth year. The authors are to be commended for generating a rare multi-year field dataset that provides critical quantitative evidence challenging the traditional assumption of monotonic hydrological recovery in post-fire environments.

The central premise of the paper is to track the evolution of debris flow likelihood from Year 0 to Year 4. However, the manuscript explicitly states that for Year 0 and Year 1, the authors did not measure hydraulic conductivity or grain size in situ. Instead, they assumed a constant of 10 mm/hr based on the nearby Fish Fire.

The "Year 0" condition is the most critical and variable baseline. K values immediately post-fire can vary by orders of magnitude depending on burn severity, soil texture, and ash clogging. By assuming the starting point, the entire "recovery trajectory" (the slope of the curve from Year 0 to Year 4) is synthetic. If the true Year 0 K was 20mm/h or 5mm/hr (instead of 10), the conclusions about the rate of recovery would be different.

Thank you for the comment and pointing out the long-term trend of the Ks, we also responded a similar comment from Reviewer #1. As we did not have the mini disk infiltrometer during the first survey, we performed a Water Droplet Penetration Time, and there was no change of the droplet after 10 minutes. Thus, we decided to use a more conservative value of 10 mm/hr rather than values <10 mm/hr given that there were measurements from a nearby fire (2016 Fish Fire).

Lines 202-206 (Section 3.1): “During the first field survey, due to the lack of instruments and access, we assumed a hydraulic conductivity of 10 mm/hr as a conservative estimate based on data from the 2016 Fish Fire in the adjacent San Gabriel Mountains with no vegetation cover (McGuire et al., 2021a). We also performed a Water Droplet Penetration Test (WDPT) at the basin outlets during this survey.”

Lines 467-469(Section 4.1.1): “During the first field survey, we did not observe any changes after 10 minutes during the Water Droplet Penetration Test, indicating a strong repellency. We also identified dry ravel, including on slopes outside our basin boundaries.”

The modeling results for the Year 4 Wet Season indicate a high likelihood of debris flow initiation. Specifically, the model predicts that Basins A, B, and E would trigger at $I15 = 30\text{mm/hr}$, and Basin D would trigger at just 20mm/hr . The authors note that actual rainfall events did reach intensities matching these thresholds. Crucially, the authors do not explicitly state whether debris flows actually occurred during these Year 4 events. If the model predicted failure

at 20mm/hr (Basin D) and rain hit 20mm/h, but no debris flow occurred, this is a Type I error (False Positive).

Thank you for pointing this out and responded to similar comment to Reviewer #1. We are unable to verify whether debris flows occurred immediately after these events, given that road closures prevented access to the sites for several years. Thus, the objective of our study is to focus on how different model parameters influence PFDF likelihood. We have reworded the discussion and introduction to inform readers how seasonal changes can impact PFDF likelihood, based on field measurements from this fire.

Lines 115-128 (Introduction): “In this study, we investigate how seasonal variation influences post-fire debris flows (PFDF) likelihood four years after the 2020 Lake Fire, drawing on information from five high burn severity basins. Specifically, we use data collected from three field surveys, during the first year, during a dry and wet month in the fourth year, along with high-resolution, high-repeat satellite imagery and field measurements to estimate how seasonal changes could affect PFDF likelihood immediately after the fire and four years after the fire. Our study aims to (1) evaluate long-term changes in post-fire debris flow (PFDF) likelihood in high-burn-severity areas over the four years following a fire, (2) assess the relative importance of vegetation cover, hydraulic conductivity, and grain size in controlling the rainfall intensity-duration (ID) threshold over the long term, and (3) assess the significance of seasonal variation relative to long-term post-fire recovery. Overall, we show that the rainfall ID threshold remains highly sensitive to seasonal changes in soil hydraulic conductivity even several years after the fire. This study improves our understanding of how seasonality, climatic patterns, and post-fire recovery interact to drive PFDF dynamics.”

The study uses dNDVI as a direct proxy for vegetation cover in the model. The authors conclude that vegetation has a minimal influence on PFDF likelihood compared to hydraulic conductivity. The authors need to clarify how dNDVI was converted to the hydraulic parameters in the model.

Thank you for the comment. In this case, we only use dNDVI to measure vegetation cover. We could not provide more detailed analysis such as terrain roughness or vegetation density. This vegetation cover parameter is only used as an input parameter for the modeling approach to estimate the PFDF likelihood where we classify 0 to 100% green (vegetation cover). We responded to a similar comment from Reviewer #3.

Lines 809-813 (Section 5.2): “We also note that satellite-derived vegetation cover used in our model input can include shrubs or grasses in areas without tree cover. Areas with high tree cover presence can obscure the understory. Therefore, the vegetation cover here does not necessarily represent understory density and surface roughness, and canopy interception can vary depending on the vegetation type (Zhao et al., 2019).”

Your discussion currently relies almost exclusively on examples from the Western United States. This may limit the applicability of your findings. You are suggested to compare your results with

recent major PFDF events and studies worldwide, particularly in Southwest China, focusing on the Xichang and Yajiang fires. Your study site is Mediterranean. In contrast, Southwest China (e.g., Hengduan Mountains) experiences a monsoon climate where vegetation recovery is often faster, yet PFDF likelihood remains high due to extreme rainfall intensity. How does your seasonal K variation hypothesis change in a region where soil moisture fluctuations are even more extreme?

Thank you for your insight. This would be an interesting test for future studies as it is out of the scope of the study area. Based on our study, we hypothesize that if soil moisture still plays a big role in this climate, we can expect large seasonal variations.

Lines 820-825 (Section 5.2): “We propose that the increase in debris flow likelihood in the wet season in the 4th year could reflect short-term variability such as intense rainfall shortly before our field collection, which resulted in significantly low hydraulic conductivity values rather than a long-term yearly increase of debris flow likelihood. Further studies are needed to understand how long-term seasonal PFDF likelihood varies across regions with different climates (e.g., He et al., 2025; Zhou et al., 2026; Ridgway et al., 2026).”

He, K., Hu, X., Wu, Z., Zhong, Y., Zhou, Y., Gong, X., and Luo, G.: Preliminary analysis of the wildfire on March 15, 2024, and the following post-fire debris flows in Yajiang County, Sichuan, China, Landslides, 21, 3179–3189, <https://doi.org/10.1007/s10346-024-02364-z>, 2024

Ridgway, P., Bläsi, L. T., Halso, M. C., Vetsch, D. F., and Lane, B.: Beyond the initial impact: Channel evolution following wildfire and debris flows in a mountain stream of the Upper Colorado River Basin, Geomorphology, 2026, 110485, <https://doi.org/10.1016/j.geomorph.2026.110485>

Zhou, Y., Hu, X., Gong, X., He, K., Jiang, M., Yang, J., and Peng, H.: Variability of post-fire debris-flow initiation mechanisms across different lithological zones in the Hengduan Mountains, China, Landslides, 23, 129–141, <https://doi.org/10.1007/s10346-025-02616-6>, 2026.