Response to reviewer Marc comments

Note: As in our response to Reviewer Mirus, here the Reviewer text is in black, and our responses are in blue.

Marc Review of "Characterizing the scale of regional landslide triggering from storm hydrometeorology" by Perkins et al.,

The authors present an analysis of several storms induced landslide events, most relating to atmospheric rivers phenomena in California. They retrieve rainfall from a gridded gauge product (covering >10 years, 6hr resolution, 4km spatial resolution), and a leaky bucket model to constrain regolith moisture and derive a soil moisture anomaly, A*, relative to a 15 yr return event. They show that 15 yr return appear to be the minimal return time for causing extensive landsliding based on 4 well constrained case and then show and discuss the advantage of using a soil moisture anomaly (rather than simple rainfall anomaly) to understand landsliding triggered by rainfall in California. The work is a nice progression from previous work arguing for the use of anomaly to study landslide event (Rainfall anomaly for Marc et al., 2019, or soil moisture anomaly for Saito and Matusyama 2012, but with a more complex methodology and rather preliminary data).Therefore the authors' work goes provides first basis for simple, physically meaningful and regional scale indicators that could provide a basis for landslide hazard forecasting during storms.

In terms of methodology and presentation, I had reviewed a previous version of this work and a lot of my previous concerns in terms of methodology and clarity have been addressed and this version of the draft appears very clear and well thought to me. I therefore congratulate the authors, as I think the work will be a very good contribution to Esurf! I provide below a series of minor comments where I have identified potential improvements.

Sincerely, Odin Marc

We thank Reviewer Marc for providing some extremely helpful comments that led us to think more about what the data are indicating in terms of the processes at play during the storms in our catalog, and to ultimately produce a much stronger and more considered manuscript.

Line By Line Comments

Introduction

→ It is maybe a personal feeling but I had the impression of a small disconnect in the Introduction with the paragraph about the storm/AR categorization… Given the work is about Rainfall induced landslides and better understanding/forecasting them I thought this could rather come after the paragraph detailing the state of the art in terms of relating landslides to rainfall and soil moisture. But this is up to the authors.

Thank you for the suggestion. We agree that repositioning the paragraphs here will indeed lead to a more logical flow of ideas and draw focus to the primary aims of the manuscript.

→ Up to you but it may be interesting to mention the usefulness of simple leaky barrel approaches to understand the timing and conditions of landsliding in other context than California, such as monsoon induced landslides in Nepal as presented and discussed in Gabet et al., 2004, and Burrows et al., 2023.

Thank you for the suggestion, we will be sure to fold in these additional references.

→ Last, this is optional and up to you but I personally think within the general framework of combining basic characterization of the topography (typically slope gradient) and of the forcing to understand and forecast landsliding I think the studies on seismically triggered landsliding and rainfalltriggered landsliding are quite complementary and illustrative of similar concept.
Thus Marc et al., 2017 and Tanyas and Lombardo 2019 have basically developed and validated to some extent a Landslide Potential Area (LPA this study) for EQ induced landslides, or in their term characterizing a Landslide Affected Area, based on the intersection of a minimal slope criteria and a minimal ground shaking criteria. So this work and following may be worth to mention in intro or discussion to introduce/discuss the LPA concept.

These earthquake-induced landslide studies are indeed a great parallel for this work, and in our revised text we will be sure to describe in more depth the idea of a Landslide Affected Area and its underpinnings in the literature on coseismic landslides.

Figure 2 : Fig 2D has something weird with the polygon of 2d ? And the caption is missing an explanation of what these boundaries are exactly…

Apologies for this oversight in not specifying the black line in the caption. This line represents the GPS tracks from the post-event landslide field verification survey. This was an attempt to more honestly convey that the landslides were mapped primarily in the field rather than within a defined box using satellite/aerial imagery. In a revised manuscript we will be sure to specify this information within the figure caption.

Also you say in the caption : **"A regionally consistent threshold would plot as a horizontal line"** Do you mean an absolute, constant threshold (thus constant across the region) ? If yes I don't find "regionally consistent" the best term… wording here could be confusing I would say maybe better to rephrase.

Yes, in hindsight this wording is indeed confusing! Perhaps a better term would be "a universally constant AWI threshold," which we will adjust in our revision.

Figure 4 : This is interesting to discuss what LPA is and from what it results but does not help to assess its validity. Could you show on Fig 4 the available landslide event data ? Basically for the 4 calibration storms could you display the landslide location (in a less zoomed manner than on Figure 2)? Also could you compare/correlate LPA to the actually measured landslide affected area ? (typically the convex hull containing all or 95% of the landsliding? See Marc et al 2017, Tanyas and Lombardo 2019). Another question coming is whether LPA is correlated to the total landslide area? Did you check that?

In our revision we will be sure to place the landslide calibration sites for the relevant events in Figure 4. Unfortunately, in the case of our calibration events a comparison between the mapped landslide area and the LPA is somewhat irrelevant because the mapped calibration sites are so small spatially (10s of km or less) compared to the geography of California (>1000 km north to south).

However, we do show a more visual representation of this approach in Figure 8. Here we utilize the California Geological Survey Reported Landslides database, which underrepresents the total number of landslides but has good spatial coverage across the state, to compare to the distribution of A^* for two large atmospheric river storms that occurred in January 2023. For each storm, the distribution of above-threshold A^* overlaps with the zones of relatively high landslide concentration. Because landslides for each storm occur across multiple mountain ranges separated by large valleys, a convex hull around the entire inventory will over-represent the actual landslide-affected area compared to LPA; however, one could certainly attempt to do this for each specific mountain range. Further work testing this metric using spatially extensive landslide inventories may allow for a better comparison between the actual landslide-affected area and LPA.

Fig 5 : I am a bit skeptical about the proposition that seasonality is the main control, or at least I wonder how important are other aspect : Is Dec 05 also extremely high because the storms affected the north-western part of california with more extensive hillslopes above 5°… ? Or Because A* was not just above 1 but quite greater compare to the other storms (see Fig 2E) ? To better test your explanation about seasonality maybe you could show/check the Area with A*>1 against seasonality, independent of hillslopes. And then maybe discuss the role of the storm location relative to the topography.

We appreciate this insightful comment! Yes, where the storm track passes is certainly relevant to the resultant LPA. The northern California Coast Range has a very high distribution of slopes > 5 degrees compared to other regions of the state, so above-threshold hydroclimatic conditions should absolutely yield a higher LPA than, say, the San Francisco Bay Area and adjacent Central Valley region that contain a higher percentage of flat slopes.

As we discuss in our response to reviewer Mirus, we unfortunately discovered an error in the code that resulted in constant amount additional water added to the AWI model (0.18 m, an equivalent of the Ro value used in the analysis). This resulted in an artificially high threshold recurrence value of 15 years. With the corrected AWI values, the threshold recurrence interval now appears to be 10 years (see figures in our other Response document), more in line with the R^*_{48} values utilized by Marc et al. (2019). We note that this change does not appear to impact the accuracy of the results, but merely shifts the threshold recurrence interval down approximately five years. The figures presented throughout this Response are now corrected to account for this error.

From examination of our revised Fig. 5 below, one can see that this Dec 2005 event does have the second highest median over-threshold A^* (and highest excluding the Jan 2005 storm that is a clear outlier from the other events). So, it appears that median A^* does play a role in this case, although most events are quite similar given their respective interquartile ranges. The fact that this historic storm occurred over a broad, mountainous region also played a strong role in the resultant LPA.

Regarding the role of seasonality (e.g., Fig. R1b below), we conducted a brief test to see how the antecedent A^* (presumably driven by recent storm history preceding the landslide-inducing storm of interest) played a role in the resultant LPA for each storm event.

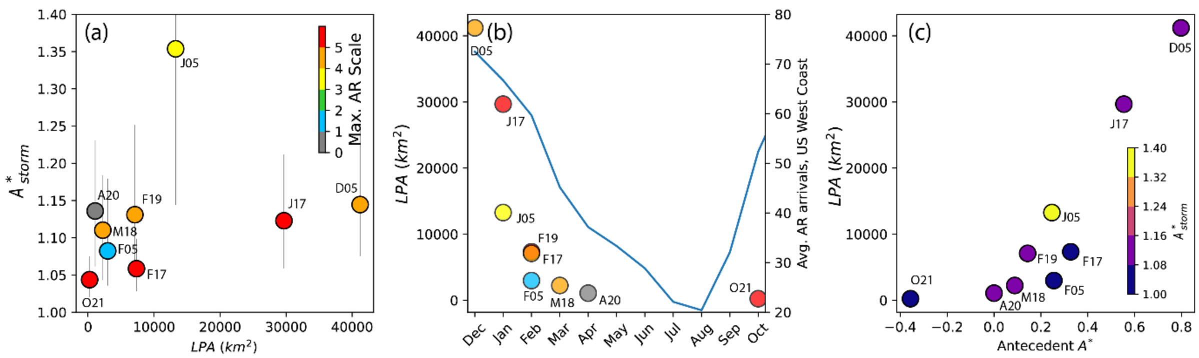

Figure R1. Relationships between LPA, median over-threshold A^*, seasonality, and antecedent conditions for each event in our catalog. (a) shows the median over-threshold A^* (with our threshold equal 1) and corresponding event LPA. Dots are colored by the AR scale of the associated storm. Panel (b) shows monthly values of LPA on the left axis, and the right axis shows the avg. monthly number of AR arrivals along the US West Coast (Mundhenk et al., 2016). Panel (c) illustrates the relationship between the event LPA (y-axis), and the median A^* value of over-threshold pixels at the onset of the storm window.

Fig. R1c shows that the largest events in the catalog have a relatively high antecedent A^* condition for the grid cells that ultimately exceed A^*=1 during the storm, whereas the smaller-LPA events tend to have lower antecedent A^* values. These data therefore indicate that antecedent A^* is likely a necessary but not sufficient condition for generating a large landslide potential area. Apart from the 2005 La Conchita event (yellow circle), much of the median A^* data are lumped together and do not show a strong variation with LPA, and therefore it is difficult to discern the role of storm strength through this methodology. This, and additional factors such as the storm trajectory relative to topography discussed earlier in our response) also play a role, and we will be sure to include an enhanced discussion of these factors in a revised manuscript.

Section 5.1 / Fig 6 → nice and clear, great job showing the difference between R* and A* ! However in the caption prefer : “Little impact could be **expected/anticipated** for distributed shallow landslide occurrence” (because there is not yet a prediction system based on A*).

We can be sure to change this language in a revised manuscript.

Fig 7: This is interesting and could address some of my concern of the actual comparison between A* and landsliding (when there is data): But for this showing only dbZ from rain radar is a step back because we lose the effect of soil moisture; The simplest would be to show both: Show the dbZ and below the A* map derived from gauges with slopes maybe? With the landslide report in both for comparison… This would allow to make your point more clearly or to discuss the respective limits of using dbZ or A* only to track landslide hazard.

L495-500 go in this direction but would be more clear if Fig 7 would contain both: A* derives by gauge vs dbZ for localized hotspot… This discussion goes back to the importance of the specificity of the dataset used to derive R* or A* in your case. Radar being rare, should we use gauge or satellite QPE when we don’t have it? Recent work such as Thomas et al 2019, Ozturk et al., 2021, Marc et al., 2022 discuss the issues of advantage, limits and potential use of satellite derived precipitation estimates for assessing landsliding.

With this figure, our primary intent was to show that rainfall characteristics at different meteorological dynamic scales can contribute to observed patterns of landslides, rather than use the radar itself as a predictive tool. However, in the case of Fig. 7b we do describe a situation where the gauge-based QPE inadequately predicts the rainfall that the radar data shows occurred at the landslide locations (Lines 495-499 in the pre-print). Given the lack of strong radar coverage in mountainous regions in California, and the variable nature there of z-R relationships needed to successfully convert radar dbZ to true rainfall intensities, we opted for a gauge-interpolated product produced by the US National Oceanic and Atmospheric Administration (NOAA). However, it is imperfect and perhaps a combined product may help coverage in areas where gauge data may be too sparse to capture rainfall features at the micro or mesoscale. In our revision we will include corresponding maps of A^* alongside the radar imagery.

L504: Large regional scale

Noted. We will be sure to adjust the language appropriately here.

L543: “a rare and comprehensive, time-consuming effort “→ Maybe rather write “a time-consuming, but essential, effort” Indeed it is still done routinely in many areas (Japan, Taiwan for example) and has been done for a fair number of cases. The current sentence could suggest to some reader that it’s not an essential part of future work.

This is a fair point, and we completely agree that the work of gathering comprehensive landslide location data is essential for many facets of landslide science.

Fig 8 / L545 : Nice ! However you should make Fig 8C in Semi – Log or Log – Log ! It's clearly a non linear trend and we cannot really see the trend and the data with low landslide density.
If it is a power-law (linear in log log) having a rough estimate of the preliminary exponent (near 1 ? >1 ? <1 ?) could help to derive interpretation and for comparison with other/later work would be useful.

The dataset used for this plot is imperfect, as the "reported landslides" from the database are not mapped comprehensively in the way that is typically done through imagery analysis. For example, although only dozens of landslides are reported in the eastern San Francisco Bay Area for the 30 Dec. 2022 storm (Fig. 8a), author Perkins observed many hundreds of landslides from a fixed wing aerial survey in this region immediately following the event. Nevertheless, we agree that it is a worthwhile endeavor to highlight the nonlinear response of landslide density to increases in A^*. To that end, below we've provided an example of a figure that could be incorporated in a revised manuscript, where a best-fit power law and exponential function are shown with the landslide density data. The fits are not very good, at least partially as a result of the incomplete landslide reporting that may leave many areas of high A^* without correspondingly high landslide spatial densities (i.e., bottom-right corner of the plot below). Here the methodology for calculating landslide density is slightly different from what is presented in the manuscript, as we wanted to calculate actual landslide counts within a grid (a suggestion by Reviewer Mirus) rather than relative increases showcased by the kernel density approach in the pre-print (Fig. 8c).

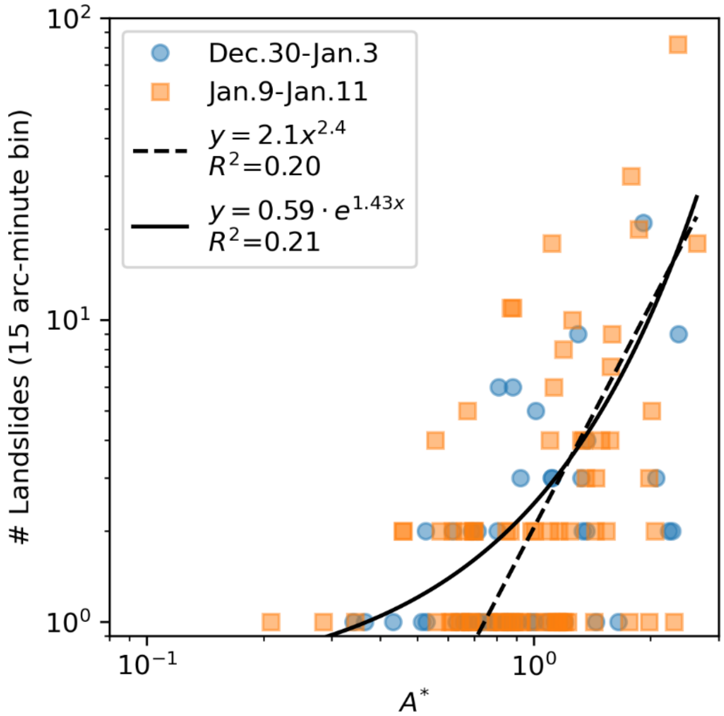

Figure R2. A plot of landslide spatial density as a function of the mean underlying A^* for 15 arc-minute (~22 km at 37^o latitude) bins for both storms in Fig. 8 of the manuscript. Here the data are plotted in log-log space, illustrating the nonlinear increase of landslide density with increases in A^*.

Last, on such plot (which is a conceptually similar to Fig 2A of Marc et al 2019, where R* was used not A*) one would wonder how much the scatter of Landslide Density vs A* would be reduced by normalizing by a slope Gradient term or separating different lithologies (possibly with different regolith thickness or strength). Your data may only allow to do it with slope, but it could be nice to check or at least mention it.

We agree with Reviewer Marc's comments here on the role of additional factors such as topographic slope that may influence the likelihood of failure. However, given the highly coarse nature of the dataset used here, we opt to leave the data as-is and intend to explore the impact of slope on landslide spatial density in a future study where landslides can be more completely mapped across a broad region.

L547-548: The phrasing here is a bit ambiguous here and may merit one or two more sentence, or rephrasing. Because in Marc et al., 2019 we hypothesize that if the landslide density correlates with R/R10 it is because the landscape has co evolved with climatic conditions (through repeated landsliding). Indeed the landscape do experience only R (the rainfall during the storm) but it's property setting its response to R could have been influenced by the previous storms, and thus correlates with R10. I think the same reasoning apply here with A and A15. So the sentence oppose soil strength / root / vegetation and climatic normalization whereas the understanding of this normalization (as proposed in Marc et al 2019, and in some geomorphological references) is that at least some of these parameters are captured by the past extreme statistics (R10 or here A15).

So unless you put forward an alternative interpretation, I would suggest that you specify (in some way) that the normalization probably works because it does capture some of the secondary landscape parameters that control hillslope stability. Of course they may not control it all as some parameters may evolve independently of past extreme, or on faster timescales.

We agree that adding a little more language on the nuances of what may be captured in the normalization process is worthwhile to include in our revised manuscript. Although we describe these factors more in-depth earlier on in the manuscript (Lines 126-135), we are notably lacking here in the discussion when circling back to these factors and what parameters governing slope stability may be wrapped up in A^*.

L554-560: This is interesting discussion toward Forecast ! However it could be nice if to add one or two sentences towards broader views: Testing the A* methods with other data sources (such as satellite derived rainfall or weather forecast models) which could be done in other geographic contexts/areas (including data poor for the satellite).

Thank you for this suggestion. We will certainly bring in a bit more discussion on how to test this approach using different datasets, particularly as we are using a QPE product that is limited to the states of California and Nevada and therefore a more general product will need to be tested using other data.

References (used in the review but not in the manuscript):

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Ozturk, U., Saito, H., Matsushi, Y., Crisologo, I., and Schwanghart, W.: Can global rainfall estimates (satellite and reanalysis) aid landslide hindcasting?, Landslides, https://doi.org/10.1007/s10346-02101689-3, 2021.

Tanyaş, H. and Lombardo, L.: Variation in landslide-affected area under the control of ground motion and topography, Engineering Geology, 260, 105229, https://doi.org/10.1016/j.enggeo.2019.105229, 2019.

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