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 meainingful 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

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.

→ 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

→ 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 rainfall-triggered 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.

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… 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.

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 ?

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.

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*).

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.

L504 : Large regional scale

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 case. The current sentence could suggest to some reader that it's not an essential part of future work.

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 interepretation and for comparison with other/later work would be useful.
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.

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 nomalization 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.

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).

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/s10346-021-01689-3, 2021.

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