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Reply on CC2

Robert Emberson et al.

Author comment on "Insights from the topographic characteristics of a large global catalog of rainfall-induced landslide event inventories" by Robert Emberson et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-250-AC3, 2021

Milledge Comment:

This is a really nice paper that compiles an impressive set of inventories and draws several useful and thought provoking conclusions. The most interesting findings each prompted a question for me that I felt it would be helpful (but not essential) if the authors could comment further on.

We thank the commenter for the supportive words. We appreciate their input and the influence their own study had on this research.

First, landslide likelihood exhibits consistent continuous increase with slope across the range of slopes for which there is sufficient data to resolve a likelihood. To me this appears to contradict other recent findings that there is a threshold slope above which landslide likelihood flat-lines or even declines (Marc et al. (2018), for a subset of the inventories examined here, and Prancevic et al (2020), for shallow landslides). Why do you think these studies find such different behaviour?

First one caveat to keep in mind is that it is actually not so straightforward to compare these results to Marc et al 2018 (plotted as a function of S-Sm and not S/Sm) nor Prancevic who plotted S/S10 (the minimal slopes above which 90% landslides occurred).

But in any case we agree these two studies suggested saturation (ie hazard scaling with respect to slope rapidly stopping) was the dominant behaviour in their inventories. What we observe is more a broadening view than a contradiction : Indeed we do find again several cases with such saturation and then a decay (often not in the high significance but still) : Morakot, Kii, Blumenau, Dominica, Hiroshima (etc). In contrast some cases do seems to have increasing hazard until 2-3 times the median slope (Thrissur, West Pokot, Burundi). We can only speculate on the reason for why these cases behave differently: perhaps they are located in landscapes out of equilibrium (Africa, Indian Escarpment) where the median slope is low but steep slopes favoring landslides are still common. Perhaps it relates to the rainfall triggering mechanism which seems not limited over steep slopes (as for one other case in Marc et al 2018). We do not think the main differences lie in the source material (bedrock or soil) as decline occurs for bedrock cases (Morakot, Kii) and increase occurs for likely soil cases (Africa). So, it clearly seems that multiple processes could lead to specific dependence with slope over the steepest slopes (>2median) of a landscape, and that detailed investigations of specific cases should be pursued.

Second, normalising by median slope works well at collapsing the data. This is consistent with the findings of Marc et al 2019 and Prancevic et al 2020 who both collapse the data in a similar way. The connection that you draw to landscape scale strength controls on the slope-likelihood relationship, is really exciting. How do you think this relates to the idea of threshold hillslopes (e.g. Burbank et al., 1996)?

Another excellent question! First we must recall that our work is about instantaneous landscape response to a forcing event, while the threshold hillslope concept is about the emergence (over geomorphic timescales) of a dominant hillslope angle, likely linked to bedrock strength. Clearly since Burbank 1996 and even more now with high resolution datasets we observe sometimes large portions of the landscapes are above the threshold. Simply over long timescales they should be preferentially eroded but it does not mean that every landslide events must focus on these zones. It may depend on the landslide trigger, or on other preparatory factors (weathering, fracturing) not instantaneous but fast relative to landscape geomorphic timescales.

While this certainly warrants further analysis and observation, it may be that the threshold hillslope model can be generalised to consider erosional behaviour below the threshold, the rate of which depends on the distance from median values in a given landscape. As mentioned above, the decay of any relationship significantly above the typical 'threshold' slopes observed in our inventories suggests that the limited parts of the landscape that persist above the threshold are subject to specific conditions (lack of fracturing, fortuitously aligned bedding planes, etc) that prevent the landslide-driven erosion from reducing these areas to the threshold values.

Third, the compound topographic index is not a good predictor of landslide initiation likelihood even in a multiple regression. I don't have a question here but for me this is a very interesting result and your discussion of the implications of this for topographic controls on pore pressure are helpful.

Certainly, we agree here. It's perhaps surprising and we note that looking at landslides triggered perhaps under less intense rain regimes may be appropriate to establish how consistent this relationship is.

Fourth, drainage area is reported as a good predictor of the entire landslide footprint and therefore of landslide hazard. You draw a parallel to Milledge et al. (2019) and I agree in that: 1) both studies highlight the importance of runout for landslide hazard; and 2) drainage area identifies areas of flow concentration. However, Milledge et al. (2019) found that 'hazard area' (which incorporated a slope inclination weighting) rather than simply drainage area was a good predictor of landslide hazard. Unweighted drainage area actually performed fairly poorly in that study. I wonder what you think the reason for this difference might be?

We would suggest that perhaps the difference lies in the different triggering processes associated with the inventories analysed by the respective studies. The runout zones seem to track flow-paths more closely for the rainfall-triggered events than the earthquake triggered ones; however, this remains somewhat hard to explain given we observe unweighted flow accumulation is a poor predictor for the landslide scar areas. An interesting conundrum for future study.

One caveat in flow area interpretation is that the landscape probability decay very strongly from hillslopes (most of the landscape with small to moderate drainage) to channels (very small fraction of landscape with very large drainage area) as it is well shown in Fig 3C of Milledge et al 2019.

For scars (not studied by Milledge et al., 2019), we found a hazard near 1 for flow accumulation near the median: it makes sense as the median is dominated by hillslopes where all scars initiate. Then there is a decay for almost all cases but most bins are not statistically significant (because scars overlapping for channels are rare (or even erroneous))

Flow accumulation for whole landslide is after normalization much more compact than what was found by Milledge 2019, and show a strong decay for area below the median (our cases rather agree with Gorkha and Haiti EQ). This is likely due to the fact that many slides runout until channel areas and thus upslope area (scars) are a small proportion of the whole landslides and are compared to most of the landscape.

It seems to be the case for some EQ and not for others (like Finisterre or ChiChi). Earthquakes that do not display this decay may have more landslides entirely (scar + runout/deposit) distributed in the hillslope domain, because they initiate higher on slopes (Meunier 2008) and/or have less long runout.

In contrast the elevated hazard for large normalized area is roughly consistent for cases with high statistical confidence, but much more diverse and showing reversal (ie decay with increasing drainage area) for many inventories when looking at the less statistically significant portion. Morakot or Zimbabwe show the start of the reversal in their significant part. Still it is true that some event show statistically significant increase of hazard with drainage until very large normalize drainage (like Kii, Thrissur, Burundi) which may relate to a large proportion of long runout landslides (clearly the case for Kii where in the south some landslides where debris flow like)

Finally, one very minor point on the presentation of the results. I wasn’t clear what was represented by the landslide likelihood ratio (Figures 3-9). Is this a likelihood ratio, which I understand to be the ratio of likelihoods or is it a ratio that results in a likelihood?

To clarify: this is the ratio of probabilities. We will clarify this in revision.

References

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Please also note the supplement to this comment:
https://nhess.copernicus.org/preprints/nhess-2021-250/nhess-2021-250-AC3-supplement.pdf