Natural Hazards and Earth System Sciences Discussions

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Comment on nhess-2021-250

Dave Milledge

Community 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-CC2, 2021

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.

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?

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)?

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.

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?

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?

References

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Milledge et al., 2019. Simple rules to minimise exposure to coseismic landslide hazard. *Natural Hazards and Earth System Sciences*, *19*(4), 837-856.

Prancevic et al., 2020. Decreasing landslide erosion on steeper slopes in soil□mantled landscapes. *Geophysical Research Letters*, *47*(10), p.e2020GL087505.