



EGUsphere, referee comment RC2
<https://doi.org/10.5194/egusphere-2022-243-RC2>, 2022
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Comment on egusphere-2022-243

Anonymous Referee #2

Referee comment on "Predictability of rainfall induced-landslides: The case study of Western Himalayan Region" by Swadhi Ritumbara Das and Poulomi Ganguli, EGU sphere, <https://doi.org/10.5194/egusphere-2022-243-RC2>, 2022

The authors explore the use of intensity-duration thresholds in the Western Himalayan Region. They expand/gap-fill the rainfall record from 6 rainfall gaging stations spread across the regions considered by using the Regularized Expectation Maximization method. They then analyze changes in rainfall between 1988-2006 and 2007-2016 and then use the landslide recorded between 2007 and 2016 to define maximum intensity-duration thresholds. They define both individual thresholds for each region and then group similar regions together and more general thresholds for the 2 resulting groups.

The paper is well written but could benefit from clarifications and better explanations of some of the steps. In my opinion, there are major issues in the methodology which need to be address and improved before publication. These are:

- The authors consider a 30d window to determine what the triggering rainfall is and select the one with the maximum intensity. This is critical for a couple of reason. First, there is no justification for the selection of 30d and it seems unrealistic that a certain rainfall event could trigger a landslide 20 or so days after. Furthermore, let's say you have a landslide at day 40 and one at day 50. You could identify as triggering for landslide 40d the rainfall event at day 15. Now, for the landslide at day 50 you might identify a rainfall at day 35. That would mean implying that the rainfall event at day 35 was not responsible for the landslide 5d after (the one happening on day 40) but was for the one happening at day 50. This is just a practical example of how the methodology could fail. Second, previous studies already showed that it's typically not the strongest intensity that triggers landslides. E.g., Staley et al. (2013), looked at debris flow and showed that "*there were statistically significant differences between peak storm and triggering intensities*", confirming that it's not always the strongest rain to trigger them. While the 30d window might be less unrealistic for deeper landslides

and for other properties (other than maximum intensity) but not for shallow landslides or when looking at maximum intensity. Surely what happens days before the landslides is important, but more as an antecedent condition than a triggering factor.

- The authors decide to use a maximum intensity-duration threshold. That is different from both the most used applications: mean intensity-duration ID or total rainfall-duration ED threshold. For ID, mean intensity is expected to decrease with duration, capturing both strong-short events and long lasting, typically less intense, events. For ED, as duration increases, you need more overall rainfall, so ED have a positive exponent. Now, there is no expectation of a dependency between max intensity and duration, if not only that has events become longer (and/or possibly that short events, if they are convective, they have stronger intensities, but this would depend a lot on the local climatology).
- The author used a power law fit to find the threshold (can that even be called a threshold if it's defined to best fit the triggering events?). Why didn't they use other methods available from literature? Why didn't they consider also non-triggering events in the definition (since they are available)?
- The authors consider for each gage the landslides within a 100km radius. Is that realistic? Is the spatial variability in rainfall such that 100km can be considered more or less homogeneous, especially in the case of convective events? Furthermore, do the authors make sure the same landslide do not get assigned to multiple gages (it's hard to tell from the figures, but it seems they could be less than 200km apart).

Additionally, there are other aspects/issues that the authors should address:

- On the comparison of rainfall between the two timeframes (historical and recent). More details are provided to explain how the gridded and gage products are combined. And how is the rainfall patterns analysis done? Does it use the gridded product? A combination of the two? The gridded product, according to the reference provided, only covers 1965-2005, which does not overlap with the landslide database timeframe, how was it then useful? How was missing data dealt with over that timeframe?
- Based on Figure 9, it looks like there are landslides every day. Based on this Figure, you'd be better off just saying "whenever it rains between June-July-August-September, expect a landslide". While the triggering events, I assume, are the rainfall of the gage closest to the rain-gage, what are the triggering events? Which gage is chosen for those? Shouldn't there always be multiple overlapping events (for each gage) of which one (or more if there are more landslides at the same time) is triggering and the other not?

Finally, I have some minor/editing comments:

- The scalebar in Figure 1 and 6 is deceiving. I believe it is for the smaller subplot representing where the regions are within India, but they could easily be

misunderstood for representative of the study area bigger subplot, leading the reader to overestimate by a lot the size of the domain

- What is the temporal resolution of rainfall? The authors refer to daily data, report values for daily durations (in the intensity-duration plots), but then use intensities in mm/h. Are hourly records available? If yes, why are the authors then using daily sums which could lead to underestimation of the threshold (e.g., Marra et al., 2019, Gariano et al., 2020, Leonarduzzi et al., 2020)? If not, why are hourly intensity reported? How are they computed?
- Two stations are unavailable in the timeframe of the landslide database, does it make sense to still consider them?

Gariano, S.L., Melillo, M., Peruccacci, S. et al. How much does the rainfall temporal resolution affect rainfall thresholds for landslide triggering?. *Nat Hazards* 100, 655–670 (2020). <https://doi.org/10.1007/s11069-019-03830-x>.

Leonarduzzi, E. and Molnar, P.: Deriving rainfall thresholds for landsliding at the regional scale: daily and hourly resolutions, normalisation, and antecedent rainfall, *Nat. Hazards Earth Syst. Sci.*, 20, 2905–2919, <https://doi.org/10.5194/nhess-20-2905-2020>, 2020.

Marra, F. Rainfall thresholds for landslide occurrence: systematic underestimation using coarse temporal resolution data. *Nat Hazards* 95, 883–890 (2019). <https://doi.org/10.1007/s11069-018-3508-4>.

Staley, D.M., Kean, J.W., Cannon, S.H. et al. Objective definition of rainfall intensity–duration thresholds for the initiation of post-fire debris flows in southern California. *Landslides* 10, 547–562 (2013). <https://doi.org/10.1007/s10346-012-0341-9>.