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Invited perspectives. A hydrological look to precipitation intensity duration thresholds for landslide initiation: proposing hydro-meteorological thresholds
Reply to Referee # 2; Roy Sidle

We thank Roy Sidle for the detailed and constructive review. Also his insights in original literature is very valuable. Thanks so much. Below we address and reply to the comments and questions. In Italic typesetting the original review is given, and in roman typesetting our replies.

Thom Bogaard and Roberto Greco

This paper offers a refreshing and needed critique of the ID relationships commonly used in regional (and global) landslide predictions. Furthermore, it proposes an improved approach that includes metrics of predisposing 'causes' and 'triggers' of shallow landslides. As such, it should stimulate new research in this arena that will benefit landslide and hillslope debris flow prediction. It will be a valuable contribution to NHESS with some moderate revision.

I noted several times in my review that follows that ID relations (at least some previously published ones) have erroneously reported data as 'individual storms', which were obviously not individual events (i.e., very, very long durations). Additionally, on the opposite side of the ID 'x-axis' there are instances of very, very short storms of high intensity triggering landslides – these appear to be bursts of intensity on saturated soils as noted by the authors or they could in fact represent a totally different process, like channel bed mobilization causing a debris flow. My recollection of reading through some older reports in which data were used to develop ID thresholds is that in some cases the described mass failure was more of a within channel debris flow. Off the top of my head, I am thinking of some of Rapp's early papers that were included in Caine's threshold. In any event, these anomalies should be considered or mentioned herein.

The reviewer clearly indicates the importance of rainfall variation over a rain period. This indeed is extremely important as also discussed in our paper (L163-L166; L327). However, we agree, we should discuss this part more. We will add a new paragraph around L163 in which we detail on the effect of a) the uncertainty arising from precipitation measurements using gauges and b) the effect of using average intensities in the ID thresholds and that when moving from short to very long events it is not possible to have identical definitions of what an event is.

I have noted a number of editorial suggestions directly on the manuscript which I will attach for the authors.

Thanks, we are thankful for these and will benefit from them.

More scientific technical comments are noted as follows:

Title: I would say "Hydrological perspectives on precipitation intensity – duration thresholds. . ..."

The title starting with "Invited perspectives:" is a format of NHESS. We will discuss the title based on your suggestion (and of Ben Mirus) with the executive editors of NHESS.

Lines 28-29: reword – "discuss" based on "associated discussion"

OK

Lines 86-88: yes, we tried this in our 1985 Hillslope Stability and Land Use book using antecedent rainfall information, but the problem was the lack of documentation of such antecedent rainfall data in earlier studies. Overall, we felt that it did improve the ID thresholds (at least conceptually).

Thanks for sharing, we will make appropriate reference to it.

Line 109: The term 'stormwater management' implies to me more of an urban planning context; that may be my bias, but you may want to add 'flood prediction' (or something like this) as well.

We indeed use the term for urban stormwater management in this context. We will add the flood prediction.

Line 125-126: I think that this is a key difference between practical applications of IDF and ID curves; that is most (or at least many) shallow landslides respond to sort-term intensity bursts which are not articulated in typical IDF's. You may want to mention this.

Agree, indeed the rainfall durations considered for the derivation of IDF curves have nothing to do with the beginning or with the end of a rainfall event: they are time intervals of given duration during which some rainfall fell. And then, extreme observed values are considered (e.g. annual maxima). Shallow landslide tend to respond indeed to short term high intensity of rainfall which are often not visible anymore when averaging over larger time steps. We will include this aspect in our paper (see also below)

Line 144: Try not to start sentences with "Figure x shows. . ."; this can be seen in the Figure and caption. Just directly say what you wish to say about the data in the figure and cite the figure in parenthesis at the end of the sentence.

We will reformulate this.

Lines 155-157: rework this sentence – understandable, but a bit confusing. Maybe just put 'mostly debris flow and some shallow landslides' in parenthesis. Furthermore, I think there are some issues with such very short 'landslide producing storms' reported in the literature that are captured in these cited thresholds. As you note, they are probably mostly debris flows, and upon inspection of some earlier papers that reported such short-term events, it seemed that the authors were referring to possibly a different process – e.g., debris flows caused by channel bed mobilisation. I looking into this matter in our 2006 landslide book, we actually threw out some of these short-term rain events when constructing new ID curves because we were convinced that they represented different triggering processes.

We rephrase as follows: "For landslides triggered by short precipitation events ($D \leq 1$ hr), the slopes of the IDF and ID curves substantially coincide (Figure 3). ".

Furthermore we will add it after line 161: "This is counter-intuitive, as during long-lasting wet periods landslides are usually more frequent, while many debris-flows triggered by very short and intense storm originate from channel bed mobilization rather than being (new) mass movements"

Lines 158-166: I agree that this is problematic, and I feel (as you state) that ignoring short-term peaks of rainfall in an otherwise long-duration, lower intensity event is the main reason for this problem. Based on my work and that of others, I always say that one common scenario for shallow rapid landslide initiation is a long storm of low to moderate intensity, with a peak intensity occurring near the end of the event. Another issue here, I agree that the longer return periods for landslides triggered by long-duration, low intensity storms is counterintuitive; however, when we looked into the actual data for some of these so-called long duration events that triggered landslides (in reviewing references for the 2006 AGU landslide book), it became apparent that some of the data included in these ID relationships were not strictly 'individual events', rather these were based on a longer period of rainfall leading up to the landslide. – thus, a direct comparison with some of these so-called long-duration landslide triggering 'events' with IDF curves for actual individual events may be a bit problematic. You probably should mention this potential discrepancy. My point is probably only relevant for the very long 'events', but it may be worth mentioning.

(See also reply after general comment at top and after Line 125-126) The importance of rainfall intensity variation should be elaborated on, and as stated before, we will add a paragraph discussing this in more detail.

Lines 185-188: This sentence is a bit confusing; it seems that you are referring to reported data when you saw the 'vast majority of empirical thresholds fall between . . .". Are you saying that for other studies most of the landslide reported would fall between thresholds of 10 to 100 mm? If so, you need to cite some references. But I am not sure that is what you are trying to say here. Anyway, please clarify. (and you overuse the expression 'vast majority' – just say most or the majority).

We reformulate: "Many of the reported empirical precipitation thresholds has between 10 and 100 mm of accumulated precipitation. However, also <10 mm and >1000 mm volumes needed for landslide initiation have been reported."

Lines 194-196 See my previous comment about data for very long duration 'events' that are likely not individual events.

Lines 209-210: In addition to my comment in the text, also see my previous comment about data for very long duration 'events' that are likely not individual events.

Agreed. See response above

Lines 227-231: Very complex sentence and a bit awkward. Can you rewrite this or try to break it up a bit?

We reformulate: "Hence, it was possible to define non-dimensional variables comparing the meteorological triggers with the infiltration and storage capacity of the soil cover. This non-dimensional hydro-meteorological threshold performed slightly better than the precipitation ID threshold in separating events resulting in factors of safety smaller and greater than 1.3."

Lines 240-241: I don't mean to be beating a 'dead horse' again, but such long events are obviously not 'events'; they were probably included in databases because this was the only precipitation record reported.

Agreed. See response above

Line 257: Why do you say 'was preferred"? by who?

We intended to express that in several studies another approach was followed, that of distributed physically-based modelling. We will change in: "This is a largely unexplored terrain, although we recognize that data availability can be cumbersome. " The discussion on the option of using physically-based distributed models is done elsewhere in the paper (In introduction and in conclusion L330).

Lines 258-260: Reword the first sentence to note that the trigger axis refers to the rainfall characteristics (intensity) responsible for initiating the landslide. When you say "depends on the local situation" – I think you mean both available data and the rainfall characteristics that are responsible for landslide initiation in that area.

Reformulated as: "Concerning the 'trigger'-axis, there is little debate; it is the rainfall intensity responsible for the short-term last push initiating a landslide."

Line 276: What do you mean by 'discharge intensity"? This is a rather unconventional term.

Reichenbach et al (1998) used: event intensity (in $m^3sec^{-1}km^{-2}$). We will explain and use "specific discharge".

Line 282: What is low/high storage?

We mean to say how much water is stored in a catchment compared to its maximum storage capacity. We clarify.

Lines 286-287 (and the sentences that follow): I think this phenomena occurs for deep seated landslides like earthflows of slump-earthflows – maybe better to state this to avoid confusion, because you are mostly focussing on shallow landslides. There is some older work in Japan that has clearly showed such relationships with earthflow reactivation and a threshold groundwater depth. I believe mark Reid also published a paper on this from earlier work in Hawaii.

Thanks, indeed this mainly holds for deeper seated landslides. Thanks for the reference. We will rephrase: "In some cases, mainly deeper seated landslides, ….."

Line 322: Again, not all data in these ID relationships were for individual events.

Agree

Line 323: you mean even when they are developed for small areas?

Yes. Thanks

Line 338: These will be particularly valuable in developing countries.

True!

I really like the message in the last paragraph of the Conclusions! Well articulated.

Thank you