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Discussions

Interactive comment on "Invited perspectives. A hydrological look to precipitation intensity duration thresholds for landslide initiation: proposing hydro-meteorological thresholds" by Thom Bogaard and Roberto Greco

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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.

I have noted a number of editorial suggestions directly on the manuscript which I will attach for the authors. More scientific technical comments are noted as follows:

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

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

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).

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.

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.

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.

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.

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.

C3

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).

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.

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

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.

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

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.

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

Line 282: What is low/high storage?

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

C4

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.

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

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

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

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

Roy Sidle