Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2017-241-SC2, 2017

Natural Hazards and Earth System Sciences
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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Received and published: 6 September 2017

The authors present a much needed discussion about some systematic problems with precipitation intensity-duration (ID) threshold approaches for predicting shallow landslides. In particular, they point out an unfortunate lack of reasonable constraints on the max/min duration of rainfall events, and also discuss how these unbounded events can affect the average intensity and predictive capabilities. Both issues are largely ignored in many studies focused on developing and testing ID thresholds, so it's a worthy discussion about some crucial sources of error. The authors also highlight the potential

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Discussion paper

importance of hydrological information in addition to precipitation characteristics, which have not been systematically incorporated into landslide early warning criteria. In my opinion the most innovative contribution presented in the manuscript is the comparison of the rainfall intensity recurrence intervals and ID thresholds for landslide initiation from the literature, along with the contour lines of cumulative storm totals (Fig. 3). This is a new and intuitive way to broadly illustrate their point about some problems with the ID threshold concept. However, my primary concerns with the manuscript are twofold:

(1) Limited concrete guidance is provided on how to apply the proposed "cause-trigger" framework, so the potential novelty of the approach seems somewhat overstated.

The general concept that both the predisposing factors (e.g. antecedent wetness) and a rainfall triggering event are needed to explain shallow landslide initiation is already generally accepted and has in fact been implemented in a number of landslide initiation thresholds. For example, two different rainfall thresholds developed for the Seattle area explicitly account for antecedent factors: (a) the recent-antecedent cumulative precipitation threshold compares the 3-day triggering rainfall to the 15-day antecedent rainfall (Chleborad et al, 2008), and (b) the Antecedent Water Index is used with an exponential ID threshold for events between 10min and 10days in duration, though storms are generally less than 24hours (Godt et al., 2006). The authors also cite several other papers (including some of their own published work) that in various ways incorporate antecedent wetness as a measure for the predisposing factors prior to the triggering rainfall event using soil water balance modeling or catchment storage. As such it is not clear how the “cause-trigger” approach is truly novel, but rather seems to be a new term for a topic in need of further exploration. (As an aside, the term “predisposing factors” seems to be a more appropriate term. . . without context “cause” could be misleading since both predisposing factors and a triggering event are needed to cause a landslide.)

(2) The critical issue of data availability is understated.

The topic of data availability is largely avoided until the very end of the conclusions, at which point it comes across as an afterthought instead of the main reason the precipitation ID threshold has been employed successfully for decades. Without continuous records of appropriate data during historic landsliding events it is challenging (if not impossible) to develop and test alternatives to the precipitation ID threshold. The reality is that rainfall data is widely available and has been for some time, which has facilitated useful, albeit somewhat flawed tools for assessing landslide potential for a number of landslide-prone areas. Secondly, rainfall can be predicted in advance with considerable accuracy, so despite some errors in ID thresholds, the trade-off between appropriate lead-time using weather forecasts and threshold accuracy must at least be considered when arguing for alternative threshold approaches. Without a more balanced discussion of data availability it's not entirely clear whether the authors are arguing for better analysis of rainfall data that distinguished between the "causing" rainfall and the "triggering" rainfall or if the authors suggest that rainfall is not an appropriate data source for the "cause" variable and the ID threshold concept has been employed incorrectly for very long and very short duration storms. Although the Invited Perspective highlights both these problems with the ID threshold approach, it remains unclear how the "cause-trigger" approach can be used to solve these problems within the context of limited data availability.

After addressing these two issues regarding the novelty of the proposed approach and the availability of data for landslide initiation thresholds, the authors' hydrologic perspectives on the precipitation ID approach for landslide prediction will be a valuable contribution and will surely be of considerable interest to readers of NHESS. I suggest a number of general and specific revisions prior to publication, outlined in the sections below.

Thank you for your consideration.

Kind regards, Ben Mirus

General Revisions:

(a) Explain how the details of how the “cause-trigger” concept can be distinguished from prior contributions that consider antecedent conditions, or qualify the novelty of the proposed approach within the context of such prior work.

(b) Provide more concrete guidance on how the “cause-trigger” framework could be applied for future studies. In particular, how should researchers constrain the duration of storms to distinguish between “cause/predisposing factors” and “trigger”?

(c) Include a more balanced discussion of what data could reasonably be obtained to inform the “cause” axis for any landslide early-warning threshold relative to the widely available (and forecastable) input of rainfall.

Specific Edits:

L1: I agree with the revision to the title suggested by Roy Sidle and would further suggest removing the second phrase since there no specific hydro-meteorological thresholds are proposed. Thus the suggested title is shorter and more precise: “Hydrological Perspectives on the Precipitation Intensity Duration Thresholds for Shallow Landslide Initiation”

L13: Provide some citation or definitive evidence for the strong, yet disputable statements like “vast majority” and “never” . . . otherwise such pronouncements should be avoided in scientific writing. Furthermore, as is later argued in the manuscript, precipitation does not actually initiate the landslide. Suggest revising to: “Many shallow landslides and debris flows are rainfall induced."

L22: What does "indistinct" mean? Thresholds are by nature distinct. On the other hand, the errors resulting from application of distinct thresholds over broad areas reflects the heterogeneity of natural systems. In theory, each hillslope/hollow has a unique threshold that must be averaged over some area and some time to create a useful tool for landslide early warning.

L27: Again, calling this a novel conceptual framework is an overstatement. See general comments.

L36: References to support this claim? I was not aware landsliding is the MOST abundant hazard. At the very least it should be qualified as a natural hazard, since many health or other hazards could be considered more abundant and/or detrimental so socio-economics.

L40-43: Unclear from the description provided here how 1) and 3) are different when applied to assessing landslide probability. Perhaps some example citations later in the paragraph could help distinguish between the two.

L59: I recommend also citing Anagnostopoulos et al. 2015 when discussing model complexity.

L74-75: Perhaps include some more recent citations that are less than 10 years old?

L78-79: Yes. Also there is considerable error introduced by the heterogeneity that must be "averaged out" for a PID threshold to be developed over an area of interest.

L86-88: Are you proposing something that is better than soil moisture? It seems that soil moisture would be better than the other variables suggested (albeit harder to measure), so it is seems counterintuitive to state that these studies are “limited” to measures of antecedent moisture content.

L88: Never say never. In general it is unwise use this word in scientific writing unless it can be rigorously confirmed, which is almost “never” possible. Suggest revising to "not" or "have not been the subject of"

L106: Here and elsewhere the abbreviation switches from PID to ID... either is fine, but use only one consistently throughout.

L149: What is an “absolute” value of a threshold? Do you mean, for example the x- and y-intercept values? Revise for clarity.

L152: Again, what is the “absolute” precipitation ID?

L153: At some point in this part of the discussion you should mention the novel use of duration-frequency curves by Fusco et al., 2017. They use this concept to examine temporal and spatial patterns of pressure head states that predispose slopes to recharge and/or landslide initiation. I think it is a nice example of how the “cause” concept you propose could be implemented practically.

L167: Yes. This also leads to questions about how storm durations are defined, particularly since longer storms are more likely to include actual breaks in precipitation where drainage and ET can be more effective in reducing landslide initiation potential.

L181-184: Revise these sentences for greater clarity. It seems like the main point is that if larger cumulative precipitations are needed to initiate landslides at lower intensities we would expect that to be reflected by the larger landslides in the inventory, but the inventory you reference is mostly small landslides, so an alternate interpretation is that the slope drains while it’s raining? At least the last sentence is incomplete to communicate the message more clearly.

L186: Technically this (between <10 and >1000) is not a range, it is unbounded. Do you mean between >10 and <1000? Revise for accuracy.

L187: Again, such strong statements like “vast majority” should be supported by a number of independent citations or other evidence. Otherwise avoid this term.

L200: Not sure this is the most appropriate phrasing. The real utility of ID thresholds is that they are not at all cumbersome to use, but rather involve a very simple and easy interpretation: does the rainfall intensity and duration plot above or below the threshold line? Maybe more important point is that ID thresholds applied locally need a "calibrated range" for storm duration whereas regionally and globally they are misleading since there is too much spatial variability in rainfall and hillslope hydrologic responses for accurate predictions.

L206: Napolitano et al., 2015 is another good reference to include here as they also used seasonal variations in antecedent soil wetness to identify different thresholds for winter vs. summer.

L218-219: Indeed, this is the concept underlying the recent-antecedent cumulative rainfall threshold of Chleborad et al., 2008, except they use prescribed durations, which have since been statistically tested with receiver operator characteristics (Scheevel et al., 2017)

L229-231: The wording of this sentence is confusing. What is the significance of this separation between near-failure events at FS < 1.3? Without reading the papers listed before it's not really clear why this is relevant.

L247: Not exactly, for such studies soil water content is not usually measured directly. Suggest revising to "proxy" instead of "measure".

L261: Again, I much prefer the term "predisposing condition" (or factors) over "cause" since both the trigger and predisposing factors essentially conspire to "cause" the increased pore pressures and reduced strength that initiates a landslide.

L263: Although perhaps beyond the scope of this paper, the "cause-triggers" approach ought to be more universally applicable to landslides, including those triggered by earthquakes or erosion. For example, an earthquake may trigger more landslides in wet vs. dry soils. Similarly, erosion at the toe of a slope may allow it to become more predisposed to failure during a rainfall event. Perhaps worth considering as you explore and develop this framework in the future.

L264: What kind of "storage" is this? Do you mean how much water is stored in the catchment (e.g. an effective saturation)? Or do you mean how much water the catchment can store (i.e. storage capacity)?

L286: How is a catchment itself more or less permeable? Bedrock permeability is clearly a measure you could consider, but very permeable bedrock would result in all

groundwater recharge and no runoff (or landslides?). Perhaps more relevant would be the thickness and hydraulic conductivity of the soil, which ultimately are reflected by how quickly the catchment drains.

L290-291: There are a lot of things mixed up in here, which makes it difficult to relate to the primary topics of the article. First, this is not a shallow landslide, so perhaps this is a bit of a tangential argument for this paper, but it seems that the main point is mobility for a large, slow moving landslide can be related to groundwater levels. That's fine. However, it's not clear groundwater levels would be a good proxy for conditions favoring shallow landslides, particularly since deeper groundwater levels might not respond until after shallow soils on hillslopes have drained and are no longer susceptible to failure.

L310-314: OK. This makes sense, but can you provide more concrete guidance or framework for evaluating the appropriate "cause" variable? Also, can you provide some balanced perspective of how readily available those types of data may be relative to rainfall? An example Figure 4 might be helpful.

L322-323: This is a somewhat subjective (i.e. value) judgment, which is tangential to the discussion presented here. The perceived or tangible value of predicting even 1/100 landslide events correctly at the expense of many false alarms is an entirely different question. Probably "predictive accuracy" would be more appropriate.

L340-349: I completely agree with these statements and don't wish to argue with the sentiment, but at the same time the conclusions are rather wordy and not particularly satisfying or informative. Another (shorter) way of saying this is that hydrologic information could improve individual thresholds for shallow landslide initiation, but the type of hydrologic information that is most appropriate will vary based on location and data availability. So then how do we go about addressing this issue?

L350: The last-minute mention of remote sensing comes across as a bit of an afterthought and it is not clear how this very broad suite of information products can be used to constrain hillslope water balance. Why not soil moisture monitoring?

References Cited:

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