Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2017-241
Invited perspectives. A hydrological look to precipitation intensity duration thresholds for landslide initiation: proposing hydro-meteorological thresholds

Reply to Referee # 4; *B. Mirus,* *bbmirus@usgs.gov*

We thank Ben Mirus for this pro-active, stimulating and detailed review of our discussion paper. 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

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 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.

We agree that Figure 3 is key in our argumentation of the issues linked to precipitation ID thresholds.

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) anda rainfall triggering event are needed to explain shallow landslide initiation is alreadygenerally accepted and has in fact been implemented in a number of landslide initiationthresholds. 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, thoughstorms 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.)

The objectives of this invited perspective are to: (a) critically analyse the precipitation ID thresholds for shallow landslides and debris flows from a hydro-meteorological point of view; and (b) propose a conceptual framework for lumped hydro-meteorological hazard assessment based on the concepts of trigger and cause.
We think we do not claim to be 'truly novel' in our paper. Obviously, as also discussed in the paper at length, we write this perspective-paper standing on shoulders of giants; the many colleagues who have shed light on this topic before us. In our article, we brought it into an overarching conceptual framework under which new research could (should) be done (L340-L350), also because many of the existing examples - like the two you point out - look for the causal relationship between antecedent precipitation and landslide occurrence merely by maximizing some measure of correlation, while our proposed framework invites to look at the most relevant hydrological process for the considered context. Only in the abstract (L27) we did write "novel trigger cause concept". Here we will reformulate in "we propose a trigger-cause conceptual framework"

The framing into ‘trigger-cause’ and then especially the “cause” is indeed debatable We defined the word “(hydrological) cause” as the predisposing (hydrological) condition of an area under study (L260, L320). We will define our use of the word “cause” earlier in the paper (section 3).

(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 oflandslide-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 oflimited data availability.*

Indeed, we decided to first set-up the problem, then the concept and lastly, the “afterthoughts”. However, we do mention the issue of data availability several times (e.g. L255, relatively in the beginning of section 3). We do agree that the data availability is the issue (at the moment) and we agree to stress that in our paper also in writing why the traditional ID thresholds are often preferred/used. May we add that once the driving hydrological process is identified, modelling can supplement the lack of data, while this is impossible with rainfall alone?
The fact that rainfall data is widely available is, in our opinion, maybe one of the reasons for misuse of ID thresholds. I our opinion “somewhat flawed” seems an understatement. Could it not be that relying on rainfall records prevented us from digging deeper? Secondly, many papers address the issue of how representative this rainfall information is. By ‘uncritically’ linking landslide occurrence to the nearest precipitation record, we seem to prefer practical/statistical correlation over causal relation. In this perspective, this is one of the two aims.

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 ofthe proposed approach within the context of such prior work.
(b) Provide more concrete guidance on how the “cause-trigger” framework could beapplied for future studies. In particular, how should researchers constrain the durationof storms to distinguish between “cause/predisposing factors” and “trigger”?
(c) Include a more balanced discussion of what data could reasonably be obtained toinform the “cause” axis for any landslide early-warning threshold relative to the widelyavailable (and forecastable) input of rainfall.

See above

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”

The format of the title is set by NHESS. We will discuss this with the editor in charge. We agree: Hydrological perspectives etc is to-the-point.

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."
The intention of an invited perspective is also to raise discussion and have sharp edges. We therefore allowed ourselves a writing style which is "less scientific" as generally seen in scientific articles. But as all reviewers bring this point on, we clearly 'overdone' it and we will discard such strong pronouncements.

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 auseful tool for landslide early warning.
Agree. We will delete these words as indeed indistinct threshold is not defined. We will rephrase:
"this approach suffers from many false positives …."

L27: Again, calling this a novel conceptual framework is an overstatement. See general comments.
See reply above. Will change in: A conceptual framework

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, sincemany health or other hazards could be considered more abundant and/or detriment also socio-economics.

Agree, we will rephrase and add reference for this: "one of the most abundant natural hazards"

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.
See also Reviewer #1: We will replace 'probability' with 'possibility'

L59: I recommend also citing Anagnostopoulos et al. 2015 when discussing model complexity.
Thanks for the suggestion.

L74-75: Perhaps include some more recent citations that are less than 10 years old?
You have a point here, we will add more recent references.

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.
Yes

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 measuresof antecedent moisture content.
Correct, "limited" should be removed: "however they were mainly including measures of antecedent soil moisture content, which may not represent the most suitable variable for any kind of landslide"

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"
As replied before we have been somewhat over-enthusiastic in our writing style. We agree to reduce the use of the strong pronouncements.

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

L149: What is an "absolute" value of a threshold? Do you mean, for example the xandy-intercept values? Revise for clarity.
L152: Again, what is the "absolute" precipitation ID?
We mean unscaled measured precipitation values

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.

Good suggestion. Thanks for pointing us to it. This is indeed a nice example.

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.
Sorry for the confusion. The amount of rainfall refers to unit surface area, so it cannot be related to landslide size, but it can be related to landslide depth. Indeed, we are referring to deeper seated landslides. We reformulate.

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

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

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.
Good point, the ID thresholds are maybe too easy to use: We change the word "use" into "interpretation". This makes the interpretation of ID thresholds cumbersome.

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 forwinter vs. summer.
Thanks for the suggestion. We would also add some remark about the fact that, when this kind of thresholds accounting for previous precipitation (seasonal variation) have been proposed, then the considered antecedent duration is usually the mere result of a correlation analysis. By exploiting process knowledge, instead, you directly can look at the most appropriate physical cause-effect relationship (and thus variable).

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 etal., 2017)
Thanks for this insight. We add this.

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.
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. The choice of referring to a factor of safety larger than 1.0 was dictated by the actually observed soil conditions during the monitoring period"

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

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.

Although we see your point we prefer the word “cause” (defined as “predisposing (hydrological) condition”), because it is a way to highlight that we are looking for the identification of the causal hydrological process.

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.
Yes, this is indeed an outlook we have. And yes, your examples are excellent. I think this is a very good suggestion for future work.

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

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 byhow quickly the catchment drains.
Indeed not well formulated, it indeed links to soil hydraulic characteristics. Chitu et al found that two catchment with low infiltration capacity and thus larger direct runoff fraction links to landslides whereas in the other catchment the underlying hydrological process was infiltration and pore pressure build-up. We will reformulate.

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 favouring 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.
Yes, you are completely right here, this example is not a shallow landslide, but indeed a deeper-seated, reactivating, coastal bluff type of landslide. This will be explicitly mentioned, and yes, regional GW does not need to be a good proxy for shallow landslides but we also did not claim that either. But we agree this was not clear. Thanks for pointing to this.

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.
In this perspective we aim to give direction to future research by providing a problem analysis and an overarching framework. Cautiously, we also give some examples, and we later on discuss that the approach will (currently) suffer from data availability. In my own experience, we ran into this problem as well when searching for hydrological information to use. However with more and more data coming available, we argue the community could try to make the step from “statistical/practical’ threshold to “causal relationships”.

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.
Thanks for the suggestion. We will change into “predictive accuracy”

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?
Indeed the last paragraph is somewhat wordy. One reviewer likes it a lot, the other one a bit less. We will try to use shorter sentences in the last paragraph to increase readability.

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?

Indeed is the explicit mentioning of RS unnecessary and an “afterthought”. We will replace by mentioning the increasing hydrological data that become available (without specifying).

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