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Invited perspectives. A hydrological look to precipitation intensity duration thresholds for landslide initiation: proposing hydro-meteorological thresholds
Reply to Referee # 1

We thank the reviewer for the detailed and constructive review. 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

General comments
The paper offers a hydrological perspective of precipitation intensity-duration thresholds (hereafter, ID thresholds) for landslide triggering, useful in early warning systems. The ID threshold is a well established empirical model, as it is proposed in numerous studies. Several limitations affect these thresholds, as summarized in this paper. The authors with this paper propose to move away from this "conventional" path for future research, arguing that simple, even lumped, hydrological information should be introduced.
They propose a general framework, where thresholds should represent both landslide causes (dynamic predisposing conditions) and landslide triggers. They argue that with ID thresholds only the latter are (conceptually) considered. Hydrological information is related to the former, and should be represented by something linked to soil water content.
I overall think that this is a good paper and well written. On the other hand, I also think that some improvements can be made.

We thank the reviewer for the detailed and constructive review. Actually, our conceptual approach is more general than only focusing on shallow landslide (then, it would be indeed soil moisture). Our point is that different hydrological information could be useful for landslide hazard assessment.

In particular, two main issues the authors should better discuss are:
1. How to separate between landslide "causes" and landslide "triggers" in practice?
In other words: at which instant/timescale one should think that there is a switch fromcauses to triggers?

This is an excellent point that we discussed at length ourselves. The reviewer is correct that categorizing landslides based on "cause" and "trigger" requires a kind of time scale to separate the two. The discussion on the timescale of trigger-cause is already half a century old (e.g. Sowers and Sowers, 1970). Wieczorek (1996) defined triggering as an "external stimulus (…) that causes a near-immediate response in the form of a landslide by rapidly increasing the stresses or by reducing the strength of slope materials.". In our own advanced review WIREs Water (Bogaard-Greco, 2015) we summarized the trigger-cause as: "A trigger is thelast push for a slope to become unstable, whereas thecause is the underlying, often long term, change thatoccurred preparing the slope for failing.".
So we see the trigger as 'the last push' with near-immediate effect and consequently the hydrological cause is all before that. We agree that when adapting our proposed framework, to use the cause-trigger concept for defining regional landslide initiation thresholds, it becomes eminent to start defining timescale to distinguish between trigger and cause.This will be different for different landslides and slopes.
We agree with the reviewer we did not discuss this and we will add a short discussion on the definition of the timescale of the trigger event both in introduction and in conclusion section, as well as in the description of the discussed examples, which point out how the timescales of both trigger and cause are strongly related to the effective hydrological processes of a specific site.. However, for us, the more 'mathematical' or 'precise' defining of the trigger timescale in the various situations is out scope of this invited perspective and more for follow up work, when the community starts adapting the proposed concept.

2. How to manage the higher modeling freedom (respect to PID thresholds) that one can introduce by hydrological analyses?
The reviewer is correct that looking for variables different from rainfall to define thresholds gives in principle more freedom. However, the basic idea is that the choice of the most suitable variable should be guided not only by pure statistical analysis (that is, how the threshold performs), but also, and mainly, by the identification of the hydrological processes that, for the kind of landslide and geo-morphological context, are expected to be responsible for "causing" the conditions predisposing to a landslide. The statistical analysis could be regarded as a tool to confirm if the process identification is correct or not.

More details on these two points are given in the specific comments (comments to L253 and L 262-264).
Finally, I recommend minor revisions for this manuscript.

Specific comments

L 20: "the conceptual idea is that precipitation information is a good proxy for bothmeteorological trigger and hydrological cause". It cannot be said that, in general, researchersderiving ID thresholds and their users have this conceptual idea in mind.This is a move of the authors which is not fully justified. So I think that this sentenceshould be rewritten, perhaps writing something on the fact that it is in general thoughtthat precipitation information can be linked by simple relationships to landslide occurrence,without explicitly taking into account hydrology.

The reviewer is correct we of course cannot speak for all researchers/groups who derived ID thresholds that this was the conceptual idea. They also could have other justifications, like: it gives (statistically) good/useful results. We will rephrase to make clear it is our interpretation, and that the practical background of precipitation ID is that often only meteorological information is available when analyzing (non-) occurrence of shallow landslides, and that, at the same time, the conceptual interpretation of their success could be that precipitation is sometimes a good proxy for both meteorological trigger and hydrological cause.

L 22: It is not fully clear what does "indistinct threshold" mean

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

L 36: "landslide is the most abundant hazard". Are the authors sure that "landsliding is the most abundant hazard"? Maybe say that it is "one of the most abundant naturalhazards", and add some references to literature (for instance: Sidle and Ochiai, 2013)Sidle, R. C. and Ochiai, H.: Landslides: Processes, Prediction, and Land Use, WaterResources Monograph, 2013.

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

L 39 – 45: The three approaches listed by the authors are not all aimed to assess"landslide probability" in a strict sense (only number 3 is). In fact approach (1) leadsto an assessment of landslide "susceptibility", which is not exactly a probability, butan index of landslide proneness in a relative scale. Approach (2) does not provide ingeneral landslide probability, as most of the landslide triggering threshold schemes are"deterministic", and probability is in fact only in theory – but very seldom in practice– related to landslide triggering thresholds (Aleotti, 2004; Iiritano et al., 1998). Theauthors should clarify this point.
Aleotti, P.: A warning system for rainfall-induced shallow failures, Eng. Geol., 73, 247–265, 2004.
Iiritano, G., Versace, P., Sirangelo, B., 1998. Real time estimation of hazard for landslides triggered by rainfall. Environmental Geology 35 (2– 3), 175– 183.

In a strict sense the reviewer is correct. We will replace 'probability' with 'possibility'. (and see comment L46 below).

L 42: perhaps integrate literature on this, with other more recent papers (e.g. Peruccacci et al., 2017 and references therein)
Peruccacci, S., Brunetti, M. T., Gariano, S. L., Melillo, M., Rossi, M. and Guzzetti,F.: rainfall thresholds for possible landslide occurrence in Italy, Geomorphology, 290,39–57, doi:10.1016/j.geomorph.2017.03.031, 2017.

Thanks for pointing to more recent literature.

L 46: The term hazard may have a specific definition in the natural hazards field, related to the probability of the event to occur. So the authors should clarify that they refer to "hazard" in a broader sense. Perhaps in clarifying this they should cite a generally accepted definition of "landslide hazard". This comment is related to preceding one on L 39 - 45

We are using the term (landslide) hazard in a broader sense, not in a probabilistic way: A (landslide) hazard is a natural phenomenon that might have a negative effect on people or the environment. We will clarify this in the text by adding a definition.

L 63: the authors use both ID / PID when referring to precipitation intensity and durationthresholds. Only one way should be used

Correct, we will use: (precipitation) ID

L 63: "hazard" is perhaps not fully appropriate

We will replace with: 'landslide hazard"

L 70: add references to papers where a "probabilistic transition zone" is used

We refer to Berti et al (2012) which is detailed on from L80 onwards

L 88: It seems that authors are referring to works where antecedent precipitation isused (perhaps as a "measure of antecedent soil moisture content"). Here the authors should better clarify what they are referring to, and cite pertaining papers.

We refer to measures indicating the wetness state of the soil anteceding a precipitation event triggering a landslide event. These are listed further on in the paper (L245 onwards). We will put some of those references here as well.

L 88: It is unclear if antecedent precipitation should be seen in the authors' framework as an hydrological (cause) or meteorological (trigger) variable

Hydrological cause: we detail on that from L245 onwards. We will add clarification here as well.

L 90: again, here "hazard" is perhaps not fully appropriate

Landslide hazard

Figures 1 to 3: perhaps for a better comparison of the various curves it may be useful toplot in planes with the same axis range (e.g. x-axis of Fig. 1 goes from 0.1 to 100, whileFig. 2 from 0.1 to 1000). Also, it may be better that figures have the same appearance(e.g. no grid in the plot of Fig. 1; adjust font size in Fig. 3).

Good suggestion, we will make the figures appear more homogeneous

Figure 3: It is unclear how the dark grey area representing "landslide threshold" is derived from figure 2, as the area that it covers is narrower than that covered by thresholds in Fig. 2

It is a generalised threshold summarizing figure 2. We on purpose narrowed the range for discussion reasons (like looking through eyelashes).

L 171: It is unclear in which sense the ID threshold is "generalized"

Broadly summarizing indication of the threshold.

Figure 3: P is undefined (though its meaning can be easily understood from discussionin the text).

As the reviewer suspects, P denotes precipitation, we will add this to the figure caption

L 175: It is not clear why precipitation ID thresholds are "volumetric", as an infinitenumber of (I,D) or (H,D) pairs can be associated to a given event rainfall H.

We mean to say: Every point on the threshold line depicts a volume, as $H=I\times D$

L 181: It is unclear why greater precipitation volumes should imply bigger landslides.Is this something reported in literature? I imagine that this is in general not true, asthe amount of rainfall gives little (or none) information on its spatial extension, and thusof that of the landslide. Also ID thresholds are derived using databases that usually report little information on landslide size, and to say that "the database consists for theoverwhelming majority of shallow landslides and debris flows" doesn't mean that the size of landslides is small.

Sorry for the confusion, we are referring to deeper seated landslides: 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. We will clarify.

L 192 – 196: In this discussion the authors should mention that ID thresholds are sensitive to the way a rainfall event is defined, that is, mainly the maximum zero-precipitation interval within a rainfall event (See Vessia et al., 2014; Melillo et al; 2015). Cleary, the shorter this interval is, the shorter the length of rainfall

events will be. With long maximum dryness the events can be so long that different hydrological processes can takeplace. In this case rainfall events do not represent “the last push” but a mixture between “causes” and “triggers”.
Vessia, G., Parise, M., Brunetti, M. T., Peruccacci, S., Rossi, M., Vennari, C. and Guzzetti, F.: Automated reconstruction of rainfall events responsible for shallow landslides, Nat. Hazards Earth Syst. Sci., 14(9), 2399–2408, doi:10.5194/nhess-14-2399-2014, 2014.
Melillo, M., Brunetti, M. T., Peruccacci, S., Gariano, S. L. and Guzzetti, F.: An algorithmfor the objective reconstruction of rainfall events responsible for landslides, Landslides, 12(2), 311–320, doi:10.1007/s10346-014-0471-3, 2015.

This is our point. We do discuss this point in L161-L165. However, we agree with the reviewer that this point should be highlighted here as well. Thanks.

L 253: The authors should discuss how to separate between the time scales of “causes” and those of the “triggers”. In other words, how to switch, in practice, from the “cause” hydrological analysis (storage), to the “triggers” meteorological analysis (rainfall)? In other words, how does the framework the authors propose contribute in removing the subjectivity of identifying the rainfall that represents the “trigger”/”lastpush” (see comment on L 192 – 196)?

See reply with general comments

L 253: Another point is: hydrology may be in general important also during the “triggering” process, while in the authors’ framework it is not explicitly taken into account. Arethe authors implicitly saying that “hydrology of the last push” can be taken into account without a significant processing of rainfall data?

The point we want to make here is that we propose that an effort is made to separate longer time scale causes with shorter time scale triggers. Of course we do not imply other mechanisms cannot take place, but processes evolving over shorter scales are more directly related to the characteristics of rainfall event (i.e. the intensity), while rainfall effects on long-term processes are smoothed.

L 262-264: “However there are several possible choices of hydrological variables to be plotted along the cause-axis, such as soil water content, catchment storage, representative regional groundwater level and similar”. This implicitly reveals that a high degree of subjectivity follows from the framework that the authors propose. Researchers do generally agree that subjectivity of the ID threshold assessment is significant, in spite of his simplicity. For instance, one source of subjectivity in ID thresholds is related to the choice of the maximum zero-precipitation interval to define rainfall events (seecomment on L 192-196). This is known to impair comparisons between thresholds, which thus makes it difficult to search for general landslide triggering thresholds. The framework that the authors propose seems to possibly bring a higher heterogeneity of the analyses, and thus maybe can in practice represent a step backwards for finding unifying concepts. By introducing hydrological analysis, researchers may have more freedom in choosing models and parameters for estimating the “cause” variable (antecedent soil water content). This may represent a possible way to manipulate theresults so that the performances of the resulting hydro-meteorological thresholds appear to be higher than they actually are. Thus, the authors should discuss how one can prevent this, perhaps by highlighting the importance of always performing validation analyses, i.e. to test developed thresholds against a sub-dataset which is not used in calibration.

The point we make is that (hopefully) more process knowledge will be in the graphs and less statistics. Indeed there will be a higher degree of freedom from a statistical point of view, but we argue also higher causality. The reviewer is right that this subjectivity can be a problem, but we take the stand that searching for a hydrological cause will improve the physics behind the thresholds whereas now, we rely only on statistics.

L 319: “ID thresholds neglect the role of the hydrological processes” is a strong statement. Indeed it may be written that hydrological processes are too simplistically represented by ID thresholds. In other words, precipitation is the main cause of landslides, but the main problem is: how to process precipitation information to obtain thresholds that perform well in forecasting landslides? And, of course, ID thresholds certainly do not represent the best way to processes rainfall data.

We do not agree with the reviewer that this statement is too strong. There is no attempt to put hydrology into the threshold and only in case of relatively linear relationships between hydrology and precipitation this type of thresholds would hold. But hydrology (pore pressure built-up in the subsurface), is not linearly related to precipitation. So, yes, we can look at ID thresholds in terms of statistics, but we argue one could also look it from the point of “being right for the right reason”.

L 332: I agree that one downside of spatially-distributed physically based models is that they require a “well calibration”. However to estimate catchment storage (as in Ciavolella et al., 2016), requires a well calibrated model too. The authors should discussbetter this point.

Point well taken. Indeed, in practice also some of calibration will remain required, although to a lesser extend then calibration of a fully distributed model. We will add this in the discussion.

L 233: A sketch explaining the approach the authors propose can be useful for readers.
Good suggestion, we will try.

Technical corrections

We are thankful for these technical corrections. We will correct them in the revised version
L 42: Caine instead of Cain
L 45: maybe something is missing as citations finish with a “;”
L 71: “separation” instead of “separator”
L 78: remove “,” after “conditions”
L 142: perhaps replace “for regions or areas not pertaining to this area” with “otherregions or areas”
L 147: “threshold” instead of “thresholds”
L 197: perhaps “phenomena” instead of “hazards”
L 198: “related” instead of “relate”
L 223: “thresholds” instead of “threshold”
L 255: perhaps “field” instead of “terrain”
L 283: “specific” instead of “particular”
L 318: “limitations” instead of “limitation”
L 326: “interpretations” instead of “interpretation”
L 331: “physically based” instead of “physically-based”