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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

Anonymous Referee #1

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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. 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 from causes to triggers?

2. How to manage the higher modeling freedom (respect to PID thresholds) that one can introduce by hydrological analyses?

More details on these two points are given in the specific comments (comments to L 253 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 both meteorological trigger and hydrological cause". It cannot be said that, in general, researchers deriving 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 sentence should be rewritten, perhaps writing something on the fact that it is in general thought that precipitation information can be linked by simple relationships to landslide occurrence, without explicitly taking into account hydrology.

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

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 *natural* hazards", and add some references to literature (for instance: Sidle and Ochiai, 2013)

Sidle, R. C. and Ochiai, H.: Landslides: Processes, Prediction, and Land Use, Water Resources Monograph, 2013.

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) leads to an assessment of landslide "susceptibility", which is not exactly a probability, but an index of landslide proneness in a relative scale. Approach (2) does not provide in general 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). The authors 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.

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.

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

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

L 63: "hazard" is perhaps not fully appropriate

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

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

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

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

Figures 1 to 3: perhaps for a better comparison of the various curves it may be useful to plot in planes with the same axis range (e.g. x-axis of Fig. 1 goes from 0.1 to 100, while Fig. 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).

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

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

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

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

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, as

the amount of rainfall gives little (or none) information on its spatial extension, and thus of 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 the overwhelming majority of shallow landslides and debris flows" doesn't mean that the size of landslides is small.

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 take place. 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 algorithm for the objective reconstruction of rainfall events responsible for landslides, Landslides, 12(2), 311–320, doi:10.1007/s10346-014-0471-3, 2015.

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"/"last push" (see comment on L 192 – 196)?

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. Are

the authors implicitly saying that "hydrology of the last push" can be taken into account without a significant processing of rainfall data?

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 (see comment 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 the results 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.

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 problems 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.

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 discuss better this point.

L 233: A sketch explaining the approach the authors propose can be useful for readers.

Technical corrections

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 “other regions 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”

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