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Interactive comment on "Antecedent wetness and rainfall information in landslide threshold definition" *by* Binru Zhao et al.

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Summary:

This study examines the value of different information content in defining and optimizing empirical thresholds for landslide initiation using landslide and rainfall data from the Emilia Romagna Region of northern Italy. The study proposes and compares four types of thresholds, each of which include different variables and information about recent rainfall and antecedent wetness.

The study uses probabilistic thresholds and objective ROC analysis to compare the value of using 3-day recent rainfall compared to (1) using recent rainfall but also adding antecedent precipitation index (API) for a hybrid threshold, or (2) using different formu-

lations of the API only. Although different antecedent wetness indexes could be used, the study relies only on the Antecedent Precipitation Index (API), which the authors actually show is a relatively poor indicator of soil moisture conditions. Overall the primary findings are not surprising, particularly in the context of prior research. Results show that accounting for both the triggering rainfall and the antecedent wetness (based on the API) improves threshold performance, largely by reducing false alarms. They also show that better representation of soil moisture with the API makes a substantial difference in threshold performances.

Assessment:

The issue of improving the reliability and performance of landslide initiation thresholds is an important one, and recent research has emphasized the importance of considering not only the rainfall triggering, but also the antecedent wetness conditions in landslide warning systems. The analyses presented in the manuscript are appropriate to address this issue, and the conclusions are supported by the results. The study provides a useful framework for evaluating information content, and I particularly appreciated how the figures could be used to identify when and under what conditions different types of information improved the various thresholds (e.g., API helps reduce false alarms during dry periods). The figures are clear and informative, and the manuscript is largely well written. There are some typos and awkward phrasing could be improved through additional English language editing, but these do not impede comprehension of the study or the contributions.

The primary strengths of the study are the parsimonious and systematic comparison of the role of different information content in landslide threshold performance, and the further reinforcement of the role of antecedent wetness in landslide warning systems. These nicely complement prior work on the topic. The primary weaknesses are the overly simplified and inaccurate representation of soil moisture with the API, and the lack of comprehensive consideration of different timescales for the various variables. It is worth noting that these deficiencies have been addressed to some extent by prior studies.

As the previous reviewer also notes, the conclusions could be more specific and unique. A primary concern is the degree to which the study presents a novel and transformative contribution to the field of Hydrology and Earth Systems Science. I do believe that some readers of HESS will be interested in the topic and it would add to a growing body of literature on hydro-meteorological thresholds for landslide initiation. It could be debated whether the study is more appropriate as a technical note than a more comprehensive research article. I have noted the most pressing considerations related to this concern in the general and specific comments below.

General Comments:

I agree strongly with the background information and justification of the study, but the objectives could be clarified as they raise some questions. The authors aim to investigate two issues:

1) The role of antecedent wetness information in landslide threshold definition, which seems to be the focus of their prior work (now published in Journal of Hydrology, https://doi.org/10.1016/j.jhydrol.2019.04.062).

Although there are some differences between the new study and the authors' prior work, there are some notable similarities (e.g., overall topic, study area, data used, techniques for analysis). Thus, before considered for publication, the new work should be revised to include reference to their previous work. Specifically, the authors must provide context for how this new work goes beyond their prior contributions. One notable difference, that could be highlighted, is the framework used in the present study to evaluate the value of different types of information content in landslide thresholds.

2) Whether or not it's necessary to explicitly consider antecedent wetness, or if it's acceptable to use only the recent rainfall condition instead.

However, in the approach they use, the proxies for antecedent wetness (e.g. API)

are calculated with rainfall and temperature data only. So essentially, this study is merely comparing whether it is worthwhile to take antecedent rainfall and somehow transform it into a wetness index before developing landslide thresholds. It seems that this has already been addressed in prior studies references in the introduction (e.g., Glade, 2000; Godt et al., 2006). So even though the study uses measured soil moisture (at 10cm depth) to calibrate the recession parameter for the API, it is still a calculation with rainfall only, which is limiting. The manuscript cites our recent paper (Mirus et al., 2018a) in which we used actual soil moisture data and found similar improvements, but it does not recognize the follow up publication (Mirus et al., 2018b; https://doi.org/10.3390/w10091274) in which we further evaluated the appropriate timescales of antecedent saturation vs. recent rainfall. As such, the discussion should better recognize the limitations of the API approach in the context of other contributions in the literature (see specific comments below).

Specific Comments:

P3.L10 – In our more recent paper (Mirus et al., 2018b), we used ROC characteristics to evaluate different durations of antecedent saturation vs. recent rainfall for landslide thresholds, as well as to illustrate the impact of different choices in ROC skill metrics for hydro-meteorological threshold optimization. This is worth noting in the introduction.

P6.L15-16 – This is more or less the thresholds we identified in the aforementioned paper in Water (Mirus et al., 2018b). So it is interesting the timescales are similar.

P7.L30 – typo. . . should be "no landslide" not "on landslide" occur.

P8.E3&E4 – Should mention that HR and FAR are more commonly referred to as the TP_rate and FP_rate.

P8.L8 – Is this the same as the optimal point criteria (often referred to as the radial distance)?

P8.L10 – This is confusing, why would you restrict the value of HR? In ideal circumstances HR should reach unity. Do you mean that there are multiple threshold values with HR=1, but rather than minimize the Euclidean distance, you do not allow HR<1.0 and the optimization focuses on reducing FAR instead?

P10.L7-8 – Is a contingency a "null" event (i.e. day with no landslide)?

P10.L14 – Maybe not that unexpected, since the comparison between soil moisture and API is fairly poor (Figure 2).

P10.L20-26 – These findings seem consistent with Figure 7 in Zhao et al. (Journal of Hydrology, 2019). Consider discussing the differences and similarities with your prior work.

P13.L6-7 – Assuming this improvement from APIv1 to v2 mostly reflects the better representation of soil moisture (Figure 2), this suggests that even better representation of soil moisture than either API version would be even better for threshold performance. Thus, one should recommend using a better model (e.g. Godt et al., 2006), which accounts for monthly variations in ET and an exponential decline to reflect faster drainage during wetter conditions. Or even more appropriate would be to use measured soil moisture or a better model of soil moisture (Mirus et al., 2018a,b).
P13.L21-22 – What do you mean by a physical-based approach? Consider providing references tha account for either the seasonality or the antecedent wetness explicitly (e.g., Napolitano et al., 2015, https://doi.org/10.1007/s10346-015-0647-5; Thomas et al., 2018, https://doi.org/10.1029/2018GL079662). Also, consider revising to "physics-based" or "physically based" rather than "physical-based."

P13.L30 – yes, see suggestions to cite in above comments (L6-7).

P14.L1-4 – Indeed, in our more recent paper (Mirus et al., 2018b) we explored a wide range of timescales and still found that 3 days does work quite well for different cities in the Pacific Northwest of the United States. Of course, different regions should expect different durations of recent rainfall to correlate with shallow landslide occurrence,

which is an important point to mention.

P14.L7-8. This was also shown by Godt et al. (2006) with a better model and by Mirus et al. (2018a,b) using actual measured soil moisture.

P14.L16-24 – Although I agree with these conclusions, they do not represent a particularly novel or unexpected finding in the context of prior published work (see references list and papers cited in this review). As such, perhaps the paper is more suitable as a technical note, than as a research paper.

P18.T2 – Not sure this table is strictly necessary or beneficial.

P19.T3&T4 – need to provide key for terms in headings, especially d, is that Euclidean distance?

P21.F2 – It seems that API in both cases is a very bad predictor of measured soil moisture. Why not use the actual measured soil moisture as we did in other studies (Mirus et al., 2018a,b)?

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