Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2018-316-RC2, 2018

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "A susceptibility-based rainfall threshold approach for landslide occurrence" *by* Elise Monsieurs et al.

Anonymous Referee #2

Received and published: 18 December 2018

The manuscript proposes a novel threshold for landslide occurrence, based on a non-linear antecedent rainfall index and a landslide susceptibility index (mainly depending on topography and lithology). The porposed threshold is applied to a large area of Eastern Africa, exploting satellite rainfall information, as rain gauge data are lacking in that area.

Such a topic is of interest for the readership of NHESS. The manuscript is well written and organized, and the English language is correct.

However, there are some major issues that should be addressed before the manuscript might be considered for publication.

Specifically, my major concerns are the following:

1. All the elaborations have been carried out without considering AR values which did not correspond to any recorded landslide, although the satellite data used would easily allow it. The authors should explictly mention this choice, explain the reasons for it, and, in the discussion of the results, try to figure out what would be the effects of the inclusion of non-lndslide AR in their calculations.

2. I don't agree with the intepretation of the roles of the variables S and AR, used for the definition of the threshold. In fact, S is an index indicating static geomorphological conditions which make a place more prone to landsliding than another (nothing to do with hydrology, at least not directly). On the other hand, AR, extended over a period of 6 weeks, clearly is not related with triggering rainfall, but mostly on the long-term water accumulation in (and drainage from) the system. So, this AR accounts for hydrological processes leading to predisposing conditions, as well as for characteristics of the triggering rainfall event (the last few days in the AR summation).

3. While I fully agree that a limitation of commonly adopted AR indices is their linearity (i.e., water accumulates always in the same way, regardless of the wetness state of the system), and that the proposed non-linear exponent is a smart way to introduce non-linearity, I disagree with the simplistic intepretation (more rain, longer residence time), which is contradicted by many well-established reuslts of hillslope hydrology, indicating that the wetter a slope is, the faster is the (subsurface) drainage out of it. Hence, i would be more cautious in the discussion of the meaning of the obtained parameter accounting for the non-linearity.

In the attached annotated pdf file, you can find several more detailed comments, which I hope con be of help for the authors to understand my comments, and maybe improve the manuscript.

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2018-316/nhess-2018-316-RC2-supplement.pdf

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