

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

Anonymous Referee #1

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The main purpose of the paper is to provide a large-scale definition of rainfall threshold for landslide occurrence in the WEAR area using a susceptibility-based approach. The topic is of interest for the scientific community and matches the interest of the NHESS Journal. Significant is also the Authors' attempt to face complex phenomena – like landslides are – trying to use a simple statistical method. Predicting the susceptibility to landslide activity is an important applied problem in natural hazards. The authors rely on previously published models and data. They remind us that the results have to be interpreted carefully given the limitations of the statistical model and data.

On a positive note, the paper is generally well structured and written. Nevertheless, the paper presents some limitations (see comments below), concerning both the adopted methodology and some conceptual aspects. For the above reasons, the paper should

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be acceptable after minor revisions.

General comments:

A geomorphological map of the study area is missing. It should help readers to better understand the spatial distribution of landslide (Fig.1). The role played by geologic and/or topographic parameters in landslide type and distribution are not clear and must be explained and better addressed by the Authors. A Mean Annual Precipitation map of the study area is although missing.

The methodology developed during this work is clear at all and is written in an intelligible way. Nevertheless, some precision should be given. For example, in Part 2.2: Authors should precise if (1) there are any rain gauges in their study area (maybe a map of the WEAR rain gauges?) and (2) if data of the rain gauges were used in order to perform a calibration and validation process of the TMPA-RT's data.

Specific comments:

Page 2, line 31: Can you please explain and precise what you mean by “the progressive adjustment of landscapes to the governing climatic parameters”?

Page 5, line 12-13: “this model has been produced through logistic regression based on four independent environmental factors, namely topography, lithology, peak ground acceleration and precipitation” . . . I'm not convinced by the fact that these four factors are independent. . . Topography and PGA are strongly related to the lithology, rainfall is strongly related to topography, etc. You should consider to delete “independent”.

Page 5, line 16-18: “Interestingly, as their susceptibility map covers the whole Africa, this model characteristic will not contribute to mar potential extrapolations of our calculated thresholds to similar analyses elsewhere in the continent.” I do not understand this sentence.

Page 6, line 26: “We empirically determined that $a = 1.2$ and $b = 1.2$ provide decay curves that comply with. . .” Please precise how did you “empirically determined” a and

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b.

Page 7, line 4-12: Authors should maybe discuss also fast-moving landslides (e.g. debris flows, mudflows. . .) rainfall thresholds where AR is not a key issue.

Page 10: Authors should give a short title to each subparts (5.1, 5.2, etc.)

Page 10, line 13: The coarse spatial resolution concerns also the susceptibility, some of the controlling factors data cannot be collected in the study area according a thinner resolution. The coarse temporal resolution (lack of hourly rainfall data) can also be a problem, it makes difficult the characterization of the type of landslide.

Page 11, line 9-10: “However, no AR function has so far considered that the decay time constant is likely to increase with rainfall intensity.” This is not so clear; it strongly depends on the type of landslide.

Page 12, line 5-7: “(i) probably 5 chiefly, the mixing of all types of landslides in our data set. . .);” Why authors did not try to make a “raw” classification of the landslide type in their database? Please clarify this point.

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