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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Predicting storm triggered debris flow events: application to the 2009 Ionian-Peloritan disaster (Sicily, Italy)" *by* M. Cama et al.

Anonymous Referee #2

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The manuscript of Cama and co-authors presents a landslide susceptibility assessment of a 10 km2 area in Sicily (Italy) based on two landslide inventories observed after two heavy rain storms in 2007 and 2009. The topic of this study fits very well into the scope of NHESS and the corresponding special issue. The data analysis is (according to my limited expertise of stochastic modelling) state-of-the-art and presented in a comprehensible way. However, I have major concerns about the innovation of this manuscript.

Regional landslide susceptibility assessment based on inventories of observed landslides and stochastic modelling has been done in the past 10 to 20 years worldwide.

Thanks to the increased availability, quality and spatial resolution of GIS-data and thanks to new stochastic models such analysis have become more and more sophisticated in recent years. This work of Cama and co-authors is another nice example of such studies. It uses a very interesting data set – including a relatively large amount of observed hillslope debris flows – which (to my knowledge) has not yet been exploited for this purpose. The data set is particularly interesting because it includes two storm events that occurred in the same region and within only two years. I think, this is the major added value of this manuscript.

On the other hand, I must say that the presented analysis and the conclusions from this work are very similar to previous studies. For example, I notice an astounding similarity to a paper by Von Ruette et al., 2011, Geomorphology, 133, 11-22, which has the same methodology, a very similar way to validate the model, the same way to present the results – and very similar conclusions. I wonder, what do we actually learn from this new study which is different from the Von Ruette-study? Of course, this is a different catchment with different geomorphological conditions leading to a slightly different set of predictors. But I really don't see new general insights either with regard to the usefulness of the method or with regard to governing landscape predictors. If this paper shall be published in NHESS it requires – at least – a clear statement of the new lessons learned (from this data set) compared to previous studies.

Hand in hand with this comment goes my suggestion that the introduction needs to present and discuss previous (similar) studies much more extensively than what has been done so far. This is important for the reader to understand in what way the present study of Cama addresses a new (open) question.

My final general comment concerns the language of the paper. Although the text is generally understandable I think that a careful language check by a native English speaker would be necessary to avoid formulations that seem to be wrong or complicated. For example, the authors speak about "more and more diffused databanks" (on page 1733, line 21) or "the operative validity of such expectation" (abstract line 6), which sounds odd to me.

Specific comments:

- Page 1738, lines 13-15: the authors explain that the term "debris flow" is most appropriate for the observed landslides; but subsequently, they often use the term "landslide" (e.g. in the header of chapter 3.1 or on page 1741, lines 1-6). I suggest to be consistent throughout the entire manuscript in using the term "debris flow" or – even better – "hillslope debris flow".

- According to the text on page 1749, line 21 and subsequent, the final models for 2007 and 2009 included a different number and set of predictors. The significance of each of the predictors is only discussed in the text, but I'm missing a table summarizing the contribution and significance of each of the predictors to the model. Such a table would be a basis to make a comparison with other sets of predictors found in other studies.

- I think that the Figures 15 and 16 showing the difference in assessed susceptibility from the 2007 and the 2009 data set is the most interesting result and worth of a slightly extended discussion. For example, what could be the reason and the consequences of these differences? To what degree could the fact that antecedent soil conditions of the 2009-event may be influenced by the 2007 –event explain these differences?

- Overall, the number of figures could be reduced. 8 Figures only showing the used data and describing the area and the events is a little bit too much. Fig. 9 is actually a Table and not a Figure.

- Abstract, page 1732, line 4: "a past known landslide scenario" sounds incorrect to me. "past event" would be correct; scenario is future-oriented.

- Figures 2 and 3: the term "Hyetograph" is (to my knowledge) not correct; these figures show "time series" of precipitation, and not "distributions" of precipitation.

- page 1738, line 19: the main events were "preceded" (not "anticipated") by rainfall events

- page 1739, line 15: It doesn't require (without s)

- page 1746, line 21: "... can be obtained as proposed by (or as demonstrated by) Chung and Fabbri"

- figure caption Fig 6: "... containing 73 debris flows (not "phenomena")...

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