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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 #1

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The contribution "Predicting storm triggered debris flow events: application to the 2009 Ionian-Peloritan disaster (Sicily, Italy)" by M. Cama and co-Authors is good, clear, well-written and potentially publishable. The Authors present a susceptibility model, based on step-wise binary logistic regression, for the prediction of rainfall-induced debris flows. The method is applied on a well-known catastrophic case study occurred in Sicily (southern Italy) in 2009. The topic address scientific questions within the scope of NHESS. The discussion about the non-linearity of the process is widely shareable.

The theoretical background is well-argued. The readability of the whole paper is notable, with a fluent English. Study area, materials and methods are well described.

Results, discussion and conclusions are well organized. Maybe, the section named "Discussion and conclusions" could be split into two different sections. The same for the "Introduction" section, which could be split into a proper Introduction and a review of the huge literature about susceptibility analysis (some other references should be added).

I have four general comments and some specific comments that should be addressed before the manuscript can be accepted for publication in NHESS journal.

GENERAL COMMENTS

1) The only downside of the work concerns the definitions of the indexes obtained from the contingency table, and the related ROC analysis (Page 1748, Lines 8-19, and Figure 9). This aspect is often subject to misunderstandings. Several Authors have addressed the problem in different fields, e.g. Wilks (1995), Fawcett (2006), Rossi et al. (2010); Staley et al. (2012), Gariano et al. (2015). In particular, the corrigendum paper written by Barnes et al. (2009) seems clarifying the topic and limiting confusion and misinterpretations. I have a list of comments and suggestions about this topic:

- Authors should write the Contingency Table in a better way, as proposed by Barnes et al. (2009). In the way they have written it, it's difficult to understand which are the false negatives and the false positives.

- Authors should note that the correct form the write the "1-specificity" index (also note as False Positive Rate or Probability Of False Detection) is (FP / (FP + TN)): cf. e.g., Barnes et al. (2009), Rossi et al. (2010), Gariano et al. (2015).

- I made some calculations from the numbers reported in Figure 9 (which I suggest to change into one or more tables), thus I'm not so sure that the skill scores were calculated correctly by the Authors. But this can be a consequence of the way in which they have written the contingency table. The position of False Positives and False Negatives is difficult to understand. Thus, I would like to ask the authors to rewrite

better the table and to check calculations.

- I didn't understand why the numbers reported in the contingency table are with decimal places and are not integers. There should be not reported the numbers of cells? Please explain.

- I suggest include in the analysis also the Hanssen-Kuipers discriminant (cf. Peres and Cancelliere, 2014; Gariano et al., 2015). This index gives a measure of the accuracy both for events and non-events.

2) Since the susceptibility analysis concerns rainfall-induced phenomena, I suggest to include in the analysis also another predictor (another variable) referred to the rainfall. As an example, an index of the measure of the spatial and temporal variability of rainfall maxima (daily, hourly). Some works (e.g., Minder et al., 2009; Iovine et al., 2014) have addressed the problem including such an index in the analysis. Alternatively, Authors could made a more detailed analysis of antecedent rainfall conditions, e.g. comparing the conditions preceding the failures with the same time intervals in other periods without events.

3) For what concerns debris flows, an important issue to be known is the area in which they can develop and the maximum distance they can reach. This is an important issue for civil protection purposes. Thus, the Authors should specify that the proposed method can investigate only the source (triggering) areas of those phenomena. Propagation channels, alluvial fans, and invaded areas could be predicted by using different causal factors/models.

4) Some of the Authors of this manuscript have already addressed the topic in several previous work. I suggest to highlight differences and improvement of this work with respect to the previous ones.

SPECIFIC COMMENTS

I suggest to use all over in the text "calibration set" instead of "training set", and "vali-

dation set" instead of "test set".

Page 1736, Line 19: "According to Koppen classification. . ."; I suggest to include a reference (e.g., Koppen, W., 1948).

Page 1742, Line 10-11: please explain better the following sentence ". . . the odd ratios (OR), which is calculated by simply exponentiating the β". This sentence seems a bit ambiguous if compared to the definition of "odds ratio" proposed by Stephenson (2000). Probably, they are two different indexes. Thus, I would ask the Authors to explain and define better the "odd ratios".

Page 1743, Line 16-17: Authors state "The iterative calculation stops when the addition of any of the left variables does not meaningful increase the performance of the model". Please explain better "meaningful"; is there a threshold value? If yes, please explain it.

Page 1746, Line 21: Please rewrite better the following sentence "Training and test landslides can be obtained by Chung and Fabbri (2003)".

Page 1749: Numbers reported in the text do not always meet them reported in Figure 9. Please check.

Page 1767: Please change Figure 9 into one or more Tables.

TECHNICAL CORRECTIONS

Page 1733, Line 22: I suggest to change "databanks" into "databases".

Page 1735, Line 23: "Brenning et al., 2005"; in the reference list there is only "Brenning".

Page 1735, Line 24: "Guzzetti et al., 2005"; in the reference list is "Guzzetti et al., 2006".

Page 1742, Line 5: I suggest to replace "βn" with "β1 ,β2 ...βn".

Page 1742, Line 13: please correct "β s".

Page 1742, Line 15: please correct "β n".

Page 1744, Line 3: change "the use a regular grid" into "the use of a regular grid".

Page 1747, Line 10: "Bai et al., 2010"; in the reference list is "2009".

Page 1753, Line 6: Please correct "inside a 10 km2 are there are".

Page 1753, Line 12: "risk prospective" or "risk perspective"?

Page 1759, Figure 1: Red and blue points indicating the rain gauge in Figure 1a are scarcely visible. I suggest to try to use a different background. The same for Fgure 1b. Moreover, please rewrite the caption with more details.

Page 1760, Caption of Figure 2: I suppose that "16 October" should be "26 October".

Page 1764, Figure 6: Please use the same format for the legends in figures a) and b).

Page 1771, Figure 13: Please note that "TP-rate" and "FP-rate" have not been defined in the text. Moreover, I suggest to do not use rounded lines to draw ROC curves.

Page 1774, Figure 16: Please use the same scale for horizontal and vertical axes.

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