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Discussions

Interactive comment on "Temporal evolution of landslide hazard for a road infrastructure in the Municipality of Nocera Inferiore, Italy, under the effect of climate change" *by* Marco Uzielli et al.

Anonymous Referee #2

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The manuscript "Temporal evolution of landslide hazard for a road infrastructure in the Municipality of Nocera Inferiore, Italy, under the effect of climate change" tries to develop a statistical model aimed to the probability of occurrence of a landslide using Bayesian approach and apply it to a small study area located in Italy. Moreover the authors evaluate the impact of possible climatic change on such probability through the use of EURO-CORDEX climatic models.

Even if the topic can be considered important, the paper seems to be not completely developed and there are some weaknesses in the methods used and in the presentation of the results.

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Discussion paper

1) Definition of the statistical model: the authors use two variables derived by rainfall measurements as proxy of landslide triggering: 1-day rainfall and 59-day rainfall. The choice of these variables is briefly mentioned by the authors (page 6 – lines 7-9) but is totally unclear. Since the choice of the proxy variables is essential in the definition of probability of landslides triggering, this part deserves more space and more details.

2) The authors define the hazard as the product between probability of landslide triggering and the reach probability (which, in my opinion, can be defined in a more appropriate way). The authors affirm that that probability of triggering is only related to the rainfall (parameters??) and is assumed constant over the space while only the reach probability depends on the morphology and is spatially variable. I think that these assumptions are very questionable and affect the entire research. Moreover even if the authors show this definition of hazard, it is not applied and assessed in the manuscript (no figure shows hazard maps). The figure 4 (flow chart of the study is not in agreement with the results presented in the manuscript).

3) The results of the triggering probability in the future (2071-2100) are questionable as well if it is inserted in the context of IPCC AR5 results for the Mediterranean area. IPCC AR5 forecasts a strong reduction of the rainfall for this area at seasonal and annual scale. Since the authors use as landslide triggering proxy precipitation at 59 days and 1 day, the increase of landslide triggering probability seems to be a little bit controversial. The reader has no tools to try to understand the reason of this behavior.

4)The authors provide no assessment of the performance of the landslide triggering method.

5) There are different assumptions (sometimes very important, especially on the derivation of the different probabilities which compose the hazard), which are not explained with the proper details and which are very questionable. The authors should add more details each time they introduce an assumption trying to explain the possible consequences of such assumptions.

6) In the section 6.1 the stop of run-out routing is related to the exceeding of a velocity parameter and it is not clear the role of this parameter in the method used by the authors. Also other concepts, as the persistence function, are not properly explained by the authors.

7) I'm no English mother tongue but some parts of the paper are very hard to read and to understand – I suggest the use of English native speaker to re-read the paper and correct it.

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