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Natural Hazards and Earth System Sciences
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Discussions

# *Interactive comment on* "Rapid post-earthquake modelling of coseismic landslide magnitude and distribution for emergency response decision support" *by* Tom R. Robinson et al.

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The paper proposes a method of post-earthquake landslide mapping, which uses a fuzzy logic model developed from the landslides that are initially mapped after an earthquake. The paper applies the method to the 2015 Gorkha earthquake and shows that a model developed from inventories with wide spatial coverage performs well in predicting the spatial distribution of landslides. The paper concludes that method is most appropriate for "conditions in which imagery is affected by partial cloud cover, or in which the total number of landslides is very large so that the mapping requires a long time".

The paper is clearly written. The method is well described and easy to understand.



The results are logically organized. The idea of developing a model specific to the region and earthquake is interesting, although the results can be highly dependent on the spatial distribution of the initial landslide inventories.

Major comments

1. It needs to be clarified in the paper that the developed model is only applicable to the specific region and earthquake. A new model has to be developed in order to apply the proposed method to a new region. Although the author mentioned as one of the benefits of the model in the discussion that "the model is tailored to the specific location and earthquake", in my opinion, a clear statement in the result section and perhaps introduction will help to avoid any confusions readers may have when trying to apply the method.

2. The authors used a landslide dataset mapped by the DU-BGS group prior to 7 May 2015 as the training data and another dataset mapped by the ICIMOD-NASA-UA group prior to 2 June 2015 as the testing data. Although the two datasets were independently developed, as shown in Figure 1, many landslides in the two datasets are closely located. In the result section, the authors claimed that "all models achieve AUC values > 0.7, suggesting that any combination of these factors is able to model spatial landslide distribution with reasonable accuracy." I think the consistent high AUC values may also be explained if the training dataset and testing dataset are very similar. Therefore, it would be more convincing if the authors can show the similarity or dissimilarity between the training and testing datasets, perhaps in the space of the predisposing factors.

3. To apply the proposed method to a new region, new membership functions need to be selected. Because the authors used a semi data-driven process, providing more guidelines and justifications on the choice of membership functions is necessary, which can help the readers who are interesting in reproducing the work or applying the method to a new region.

4. Additional information on the landslide nonoccurrence data that were used to com-

pute the ROC curves should be provided. How many landslide nonccurrence locations were used? Were all locations where no landslides were identified in Figure 1b assumed as nonoccurrence? Was there any sampling scheme applied? My concern is if the testing data include significant more nonoccurrence data than occurrence data (i.e., severe class imbalance), it might become difficult to use the ROC curve to differentiate models of different accuracy. For example, there are two models with the same True Positive Rate; one model predicts many more false positives and therefore significantly less accurate than the other model. The difference of the False Positive Rate between the two models can be very small because of the large number of true negatives.

5. Landslide magnitude is defined as “number density” in the paper. It seems to me that the defined “number density” is a statistic that summarizes the distribution within a moving window rather than a parameter that can be linked to the potential impact of a landslide. Please provide more explanations on how the map of defined magnitude can benefit the emergency response in addition to providing information on spatial distribution.

6. The success of the proposed method is highly dependent on the spatial distribution of the initial inventories. Limited or clustered spatial distribution will likely produce inaccurate or unstable prediction. This limitation needs to be included in the discussion.

Minor comments

7. Page 2, Line 2: Consider adding “for emergency response” after “if an assessment of landsliding is to be useful”. An assessment that is not rapid but provides detailed information on the characteristics of the landslides can still be useful for other purposes.

8. Page 10, Line 18 – 27: Many place names are referred to in the text, but not labeled in Figure 7. Consider adding the place names to Figure 7.

9. Page 12, Line 13: “assess” should be replaced by “assesses”



10. Page 13, Line 23: I have trouble understanding the sentence “The model has been shown to be successful despite potential systematic bias in the initial landslide inventories, such as cloud cover above or below specific elevations.”, and I couldn’t find related results or discussion in the result section to support this sentence. Consider rephrase the sentence. Also Consider rewrite “This suggests that if the inventories are systematically biased, the results are unaffected”. In my opinion, the analysis is not enough to conclude that the results will be immune to any systematic bias (e.g., areas affected by human activities).

11. Page 14, Line 22: Consider rewrite “this suggests that systematic high fidelity mapping of landslides following an earthquake is not necessary” here and also in the abstract. Although high fidelity landslide mapping takes a lot of time and efforts, it is necessary for many applications, such as damage assessment and loss estimation, which require accurate and reliable landslide observations.

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