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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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We would like to thank this reviewer for her positive and thoughtful review. In our response below we address each of the issues raised by the reviewer in turn, highlighting relevant changes in the manuscript. Several of the points this reviewer raises are similar to those raised by the first reviewer, and thus where appropriate we refer to our previous reply to reviewer 1.

1) It needs to be clarified in the paper that the developed model is only applicable to the specific region and earthquake. . .

Reviewer 1 highlighted a similar issue that the membership curves and factors devel-

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Discussion paper

oped in this manuscript would not necessarily be applicable to other earthquakes or locations. Our response to reviewer 1 highlighted that while others had attempted such an undertaking (e.g. Kritikos et al., 2015; Nowicki et al., 2014) this was not the aim of our study. Given both reviewers highlight this point, a clear and definitive statement as to the precise aims of this study at the end of the introduction will be added. The aim of this study is to demonstrate a method for rapid post earthquake mapping using fuzzy logic, and thus it is intended that future applications can take the same steps outlined in our study to define their own location and earthquake specific factors and membership functions for rapid response.

2) . . .it would be more convincing if the authors can show the similarity or dissimilarity between the training and test datasets, perhaps in the space of the predisposing factors.

A similar point was also highlighted by reviewer 1, to which we agreed that addressing this issue was necessary. We will therefore add an extra panel in Figure 1 clearly showing distinctions between the training and test datasets and, as this reviewer suggests, compare the distribution of both datasets in regards to one of the predisposing factors, namely slope angle.

3) . . .providing more guidelines and justifications on the choice of membership functions is necessary. . .

In the present manuscript, we briefly address the semi-data driven approach to deriving membership functions on page 5; lines 25-30. We highlight that the primary fitting method is via linear regression to fit a line with the best 'goodness of fit', but that users may manually alter the function if desired, potentially reducing the goodness of fit. One way in which these memberships may be manually altered is in circumstances where the best overall goodness of fit more accurately represents low influence values (i.e. <0.5) at the expense of high influence values (i.e. >0.5). In such circumstances the output hazard model is therefore more likely to be 'optimistic' in its forecasting of landslide hazard. In some instances it may be more preferable to be conservative in ones forecasts, and therefore achieving greater goodness of fit for high influence values at the expense of both low influence values and overall goodness of fit would be preferred. We add a short (1-2 sentences) explanation to this effect at lines 26-27 of page 5 to address this reviewers comment.

4) Additional information on the landslide non-occurrence data that were used to compute the ROC curves should be provided. . .

In this manuscript we have applied the widely accepted approach to deriving ROC curves that have been used in numerous other landslide hazard/susceptibility studies as measures of model success. On page 6, lines 22-27 we describe the method used to derive our ROC curves and direct the reader to several appropriate references which describe ROC curve creation in more detail (Metz, 1978; Zweig & Campbell, 1993). We also clearly describe our definition of true and false positive rates, defining false positives (x axis) as being all cells with hazard values above a given threshold within which a landslide is not recorded. True positives (y axis) are defined as all cells above the same hazard threshold within which a landslide has been recorded. The hazard threshold is altered multiple times in order to compute multiple x-y coordinates from which a continuous curve can be plotted. In this regard the false positive rate does include all cells above the hazard threshold with landslide non-occurrence. While we agree with this reviewer that such an approach does have its limitations (particularly around class imbalance) it is beyond the scope of this study to address the potential benefits and limitations of ROC curves and, as previously stated, we note that such an approach to defining ROC curves is common in the landslide modelling community and therefore not at odds with other studies and models.

5) . . .Please provide more explanations on how the map of defined magnitude can benefit the emergency response in addition to providing information on the spatial distribution.

By assessing the number density of landslides within a moving window we are able to identify locations where comparatively more landslides are modelled. Sharing such hazard information with emergency responders allows them to overlay this hazard information onto maps of critical infrastructure and building locations. This enables emergency responders to identify areas where high landslide hazard (i.e. number density) coexist with critical infrastructure and/or people and thus locations where the highest landslide damage might be expected. This can allow emergency responders to prioritise locations for urgent response, potentially before information from the ground becomes available, which is likely if these locations have been badly affected as communication lines are also likely to be affected. This can allow faster and more efficient response to landslide losses. We add a short explanation to clarify this in page 12; lines 1-7 where we currently discuss the utility of the model for informing emergency response.

6) The success of the proposed method is highly dependent on the spatial distribution of the initial inventories. . . This limitation needs to be included in the discussion.

We have specifically addressed this point in detail in the results section on page 11, lines 15-19 where we describe the variation in goodness of fit for each pre-disposing factor for both the spatially distributed and clustered sub-samples. In fact, it is this observation that leads us to the conclusion that our proposed method is best suited to 'conditions of partial cloud cover where the total visible area of ground. . . is small but covers a wide area, or where large numbers of landslides are visible. . .' (Page 11; Lines 22-23). To address this reviewers concern we will add another sentence describing this limitation in the discussion section on Page 14, Line 5.

Minor Comments:

7) Page 2, Line 2: Consider adding "for emergency response" after "if an assessment of landsliding is to be useful". . .

Done.

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

Done. We will also add the same place names into Figure 1 where appropriate.

9) Page 12, Line 13: "assess" should be replaced by "assesses".

Done.

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."... 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...

This sentence refers to the fact that cloud cover post-earthquake predominantly obscured ridgelines and therefore resulted in the systematic bias in the location of landslides in the training inventory. Thus if this bias in landslide locations affected the model, the results should show an inability to forecast test landslides near or at ridgelines. However, we find this is not the case as the model does accurately model test landslides at ridgelines. To address this, we add a description of this systematic bias in the training dataset to section 3 Data (Page 4, Line 27), and reword the sentences suggested by the reviewer for clarification.

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 effort, it is necessary for many applications, such as damage assessment and loss estimation, which require accurate and reliable landslide observations.

We will add "for informing rapid modelling attempts." here. While we agree that high fidelity landslide mapping has it's uses, such as those suggested by the reviewer, we maintain that it is not necessary to map all landslides in high detail in order to accurately model the spatial extent of landslides post-earthquake, as our modelling has shown.

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