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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 are grateful for the detailed comments provided by the reviewer in an effort to clarify and improve our manuscript. In our response below we clarify the issues the reviewer has addressed with regards to the uses of the method and its novelty. We summarise the 6 points that the reviewer raises:

1) The reviewer argues that the method isn't especially unique, and that the sample size used is too large to be considered a 'small initial sub-sample', being approximately half the size of the test sample. In this regard, we accept that the manuscript could be clearer in highlighting that the 'small initial sub-sample' we refer to is the ∼200 landslides required to undertake the modelling that we describe in the second half of

the manuscript. In the final section of the results we have shown that virtually identical membership curves to those derived from the 2006 training landslides can be derived from just 200 randomly selected landslides. Therefore virtually identical hazard models could have been produced from a sample that is only ∼5% the size of the test sample. Our motivation for doing this is to identify how soon after the earthquake – in terms of how many landslides are needed to have been mapped - our method could have successfully generated a hazard model. While the use of fuzzy logic is not unique, we argue that our approach to identify the smallest sample required to be successful is innovative and of value. Clarification in the manuscript from Line 7 on Page 4 will be added to reflect this.

2) The reviewer argues that the factors and membership curves identified in this manuscript may not be applicable elsewhere. While this may be true, defining a global set of factors is not the aim of this study. Others have already tried such an approach with some success (e.g. Kritikos et al. 2015). Our approach and focus here is to highlight that a very small sample of landslides mapped soon after the earthquake is sufficient to undertake a successful fuzzy logic approach. If this approach is applied in future landslide-triggering earthquakes, different factors and memberships may be needed, and those used in our study may not necessarily be relevant, but this is not our intention. Again, clarification in the Introduction, paragraph 3, will be added to ensure it is clear the manuscript does not seek to derive global factor memberships.

3) The reviewer highlights that other methods may be more applicable than fuzzy logic. We agree that other methods are available, each with relative strengths and weaknesses, however our study is not aimed at comparing and contrasting the different methods that are available. We highlight that the use of fuzzy logic builds upon previous work (e.g. Kritikos et al. 2015), the approach performs as well or better than other methods, and critically in a study such as ours where time is of the essence, it is fast to apply. Given that our aim is to quickly apply this in an emergency response, the speed of fuzzy logic is a defining factor in our choice of method. The reviewer asks why we

have not tested the use of different values for the fuzzy operator. Again, we have cited previous work in testing the fuzzy operator value, which identifies 0.9 to be the most appropriate (see: Kritikos et al 2015 & Kritikos and Davies 2016). Lower values result in less over-prediction but more under-prediction, and high values vice versa. With regards to the operator resulting in increasive values, we do not believe this to be the case. The largest hazard value output is 0.93, which occurs in a cell where two input factors have values of 1.

4) The reviewer highlights that the term ‘landslide magnitude’ has been used differently by other authors, including Malamud, to refer to an event size and that our usage may result in confusion. We agree this may lead to confusion, and we address this by referring to ‘landslide intensity’, which we define as the point density of landslides per unit area, at the first mention of the term.

5) The reviewer recommends that we explain how the factors selected mechanically cause landslides. We feel part of the issue here relates to our use of the term ‘mechanically’, which we remove to avoid confusion. The intention is to show that each factor has a physical expectation (e.g. ridge tops amplifying shaking) that other studies have shown relate to landslide occurrence. We add a short (1-2 sentence) description of the physical expectation for each factor in Section 4.2 Data Analysis.

6) The reviewer suggests it is important to show how many of the landslides in the training and test samples overlap. In this regard, demonstrating the colocation of the two samples is important, and an extra panel in Figure 1 will address this. However, we do highlight that the output hazard model is demonstrating the probability of landslides rather than precise locations where landslides will occur. At the scale the model is applied (30 m resolution) and given the variation between contiguous pixels, differences in landslide locations between samples has little effect.

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