Response to comment from Dr Gianvito Scaringi by Milledge et al.
We thank the Dr Scaringi for his interest in our paper and for his useful suggestions, which we feel have considerably improved the article. In our response below his comments are in normal text, and our replies are in bold.

Dear authors,
I enjoyed reading your manuscript, which I believe can be a useful contribution towards landslide risk reduction in highly seismic regions. I have a few questions, mostly regarding the robustness of your findings, which I list as follows:
- You mentioned multiple times that the DEM resolution can influence some of your results. It would be nice to quantify this influence at least for one inventory for which a higher resolution DEM is available (e.g. Northridge). Perhaps, moving from 30 m to 10 m DEM will only produce marginal improvements while increasing the computational cost significantly, or on the contrary it will change the result significantly.

We have tested the impact of varying DEM resolution from 10 – 90 m for the Northridge study area. We find that performance of slope, skyline angle and upslope contributing area improves slightly at finer resolutions. Hazard area preforms best at the same resolution as that used for parameter optimization (in this case 30 m). Nevertheless, we find that the hazard area metric remains the most skillful predictor of hazard across grid resolutions from 10 m to 60 m, and thus that the rule even when applied over length scales as small as 10 m or as large as 60 m will continue to perform 'well' relative to the alternatives. A description of this test has been added to the Discussion.

- There are cases in which several inventories are available for the same study area (e.g. Wenchuan). These inventories are sometimes quite different from each other. Among others, we discussed this in a recent submission, still under review (see the revised manuscript in the discussion at https://www.earth-syst-sci-data-discuss.net/essd-2018-105/) and we found substantial areal mismatches (up to 67%) between inventories in the Wenchuan, and rather low pixel-based correlations (R-squared as low as 0.35). We showed that this translates in quite some differences in landslide-size probability distributions and hence in landslide volume estimations. This might condition some types of hazard assessments based on volume-runout correlations. However, we did not go deeper into the topic, as it was out of the scope of our manuscript, and we did not investigate how this mismatch between inventories translates into statistics of controlling factors (e.g. slope, upstream contributing area, etc.). It would be interesting if you could estimate to what extent choosing a different inventory for the same study area would affect your assessment.

We have tested the impact of different landslide inventories for the Wenchuan earthquake and now report the results in the discussion. We find that the change of inventory has no impact on the rank order of performance of the metrics; and a very minor impact on both the AUC values and the hazard curves. As above, we now provide a description of this test in the Discussion

- Also, again about the Wenchuan case, you only chose a subset of the inventory by Li et al. (2014) containing about 1/3 of the landslides. It would be good to explain whether this subset can be thought as representative of the entire study area (e.g. in terms of landslide metrics, topography, lithology, distance from epicentre and fault rupture, etc.) so that one would be confident that the results you obtain have more general validity and are not biased by your choice, which was only due to a data availability issue. What you report in the conclusion (see my point below), that is that the site-specific and averaged rules perform similarly, is comforting in this sense, but what if it is just a coincidence?

Subsetting was necessary because gaps in the SRTM would result in incorrect computations for our topographic metrics, particularly upslope contributing area and hazard area. The subset of landslides that we use run in a swath from north to south. The area extends from the footwall to the hanging wall of the fault crossing the surface expression of the fault and thus spans almost the full range of shaking intensities, lithologies, and topographic settings. Thus, while we cannot rule out the possibility that the site-specific rule for Wenchuan would be different with the full data set, we see no reason why that should be the case. The fact that site-specific and averaged values for hazard area are essentially equivalent also suggests that we are looking at general patterns rather than coincidental relationships. We now include a series of study area maps in the supplementary information showing the study areas and the mapped landslides superimposed on the DEMs. For Wenchuan we show both the full set of landslides mapped by Li et al. (2014) and the subset that we use.

- From your analyses you obtained a set of simple and easily understandable rules to minimise the exposure, and you wrote that the hazard area calculated with averaged parameters performs only slightly worse than hazard area calculated with site-specific parameters. This is encouraging and, as you wrote, it suggests that the average parameters can be applied to other inventories (or subsets of inventories). Thus, it would be very interesting to see these averaged parameters being applied to other inventories, across a variety of landscapes, climates and seismic characteristics. Also, it would be interesting to apply your rules to a highly seismic region in which no recent earthquake has occurred, and relate it to the current distribution of population and exposed goods (but I recognise the latter is out of the scope of this work, so it is just an idea).

These are both very interesting ideas, though we feel that they are out of scope for this work as you say. We are keen to examine these rules in different contexts to establish the range of conditions under which they apply, but felt that the six cases used here make a useful initial contribution. We have taken an approach similar to your second idea to provide an indication of the spatial distribution of co-seismic landslides that might be expected in a scenario earthquake for the specific case of an earthquake on the Weinan-Jinyang fault near Xian, China (covered in a separate manuscript submitted to IJDRR).