Review of Parker et al., "Spatial predictions of earthquake-induced landslide probability"

Summary of research:

Parker et al. use logistic regression analysis of 9 coseismic landslide inventories from different locations and combine these results with a Monte-Carlo routine to estimate absolute spatial probabilities and probability uncertainties of earthquake triggered landsliding for shallow continental earthquakes. Through the initial regression analysis the authors demonstrate that slope and peak ground accelerations (PGA) yield the simplest and most generally applicable fits to the global data set and note that subduction zone events appear to behave differently than other events. The model is tested using out-of-sample data from each of the nine landslide inventories used in the study and preforms reasonably well given its simplicity. The value of the study and the newly proposed model is that it provides a baseline to study unexplained variability in coseismic landslide distributions (e.g. rainfall patterns, structural geology, material strength, etc.) and it allows for rapid assessment of coseismic landslide hazards quickly after an earthquake using freely available DEMs and USGS Shakemap products.

General reviewer assessment:

This manuscript is clear, well-written and should be published in NHESS. The methods used are sound and the model derived will be useful for coseismic landslide hazard assessment due to its simplicity. I was happy to see the authors use multiple landslide inventories for the regression analysis and work to find the most relevant and easily implemented parameters (slope and PGA) into a hazard model. The summary of previous research is thorough and even handed, and in most aspects the discussion of the results is detailed and exhaustive. Furthermore, Parker et al. do not try to over sell this new and useful model, but clearly state weaknesses, offer meaningful suggestions for future research, and how explain how their model might be used to address other questions in natural hazards and geomorphology.

I have three "major" comments that I would like the authors to address. I put major in quotations because I think that these comments can be easily addressed by the authors, and thus recommend this manuscript be accepted pending minor revisions.

(1) The Wenchuan earthquake comprises nearly half of the landslides used in this study based on the inventory by Li et al., 2014 (Note: that a more complete inventory by Xu et al., 2014 does exists with nearly 200,000 mapped landslide, but perhaps the authors had difficulty getting access to this data set). I am curious as to how much the Wenchuan event effects the model output. In other words, if this event were excluded from the analysis, how different would the results be? At the very least I would like to see a discussion on the potential influence of Wenchuan on the combine model results because I it might have a strong effect on final results.

(2) I would like to see an assessment of the performance of the model in a discrete spatial sense. The benefit of being able to predict spatial probabilities is that it will provide insight on the factors that contribute to the spatial distribution of landsliding, but it is not entirely clear how well this model preformed in a spatial sense. The figures showing how well the model preformed in event-by-event are good, but a figure demonstrating more quantitatively (e.g. beyond the spatial comparisons shown in the supplement) how well the model preformed spatially for a given event would be great.

(3) The discussion on the influence of material strength is generally lacking in the final section in the discussion "further investigating sources of unconstrained variability". At least from theoretical considerations, material strength is as important to seismic slope stability as topographic slope, while

peak ground accelerations play a secondary role. I understand the difficulty in constraining material strength parameters that are relevant to landsliding on regional scales, which is why Parker et al. and many other research choose to ignore material strength. The manuscript does note that rock strength is likely accounted for in some way with the regression analysis, and that based on comparisons of different events lithology seems to play a role. However, I am curious if the approach employed in this study might be exploited to better understand the spatial patterns of material strength for a single event, even if in a relative sense. It would be nice to see some discussion on this point as it is important for both natural hazards and landscape evolution and I think it would broaden the discussion.

I also have one suggestion that I think could help elevate the profile of the manuscript: test the model against the Gorkha (Nepal) earthquake, which is not used in this study. My colleagues and I produced a landslide inventory for the Gorkha earthquake that includes GIS polygon files of source areas and full landslide extents that is freely available from the USGS (Roback et al., 2017a,b; https://www.sciencebase.gov/catalog/item/582c74fbe4b04d580bd377e8). Such a comparison would help to demonstrate the applicability of the model presented in this study. However, such a comparison is only a recommendation that the authors may choose to ignore.

Some minor comments and technical corrections are offered below.

Please let me know if you have any questions.

Kind regards,

- Sean Gallen

Specific reviewer comments (line-by-line):

p.2, l.25-28: qualify what is meant by censoring here. This usually has to do with the resolution of the imagery used to map (e.g. resolution bias).

P2-3., l.33-34, 1-2: I appreciate the citations here, but I'm not sure that this characterizes our work accurately. I would cite our 2016 paper in the final sentence of the paragraph, as it was sort of a negative results paper that emphasizes the importance of material strength on the slope stability equations we used. Our 2015 work uses these physically-based models in conjunction with topographic slope from DEMs, estimates of PGA from ShakeMap, and a coseismic landslide inventory to invert for material strength parameters at large spatial scales. To validate the results we compare a forward model using calculated material strength to the observed landslide distribution, so I guess this reference loosely fits in with the second-to-last sentence in the paragraph.

P.4: some additional references for the case study column: Seismic wave attributes, Godt et al., 2008; Orientation of hillslope relative to seismic source, Harp et al. 2014; Bedrock lithology, Dreyfus et al., 2013.

p.5, l.15: why is McFadden et al., 2005 cited here? This study has to do with physical weathering of boulders and cobbles due to diurnal temperature changes.

p.10, l.18-25: I find this discussion and rationalization confusing. Is this simply to justify the low levels of completeness of some inventories?

P9-10, l.1-30, 1-25: After reading this section, it is not clear to me how estimates of completeness are used. It isn't until page 13 that this information is divulged. I would suggest adding a few sentences in this section describing how these estimates of (in)completeness are relevant and will be used in subsequent modeling.

p.12, l.18-33: Is the model derived in this study only applicable to SRTM 1-ArcSecond (~30m) data? I would assume that this is the case because it is calibrated to slope derived from a DEM of that resolution and slope is highly dependent on DEM resolution (e.g. Larsen et al., 2014). Please state explicitly that this model is calibrated to a specific DEM resolution and that it should not be applied using DEMs of different resolution.

Technical corrections (line-by-line):

p.5, l. 31: space between elevation and (Parker et al., 2015).

p.6, l.3: add a period to the end of the sentence.

p.10, l.12-13: I do not understand what is being said in this last sentence. Please clarify.

p.13, l.1: 9 instead of 10?

p.13, l.8-10: I am struggling with this sentence. "we" instead of "when"? Is this number 180,747 or 180 or 747? How does this number related to the Laudian earthquake with 1024 landslides?

p.13, l.14: Pedregosa et al., 2011 is not in the references.

p.17, l.8: "built" instead of "developed"?

References:

Correct the journal titles to the Copernicus format.

p.25, l.14: journal, volume, page numbers?

p.26, l.36: volume, page numbers?

p.26, l.40: was this published in 2008 or 2009?

p.26, l.49: earthquaker in journal title.

p.27, l.56: volume?

p.28, l.11: page numbers.

p.29, l.10: check journal title.

References cited in review that are not in the manuscript:

Dreyfus, D., Rathje, E. M., and Jibson, R. W., 2013, The influence of different simplified sliding-block models and input parameters on regional predictions of seismic landslides triggered by the Northridge earthquake: Engineering Geology, v. 163, p. 41-54.

Harp, E. L., Hartzell, S. H., Jibson, R. W., Ramirez-Guzman, L., and Schmitt, R. G., 2014, Relation of Landslides Triggered by the Kiholo Bay Earthquake to Modeled Ground Motion: Bulletin of the Seismological Society of America, v. 104, no. 5, p. 2529-2540.

Larsen, I. J., Montgomery, D. R., and Greenberg, H. M., 2014, The contribution of mountains to global denudation: Geology, v. 42, no. 6, p. 527-530.

Roback, K., Clark, M. K., West, A. J., Zekkos, D., Li, G., Gallen, S. F., Chamlagain, D., and Godt, J. W., 2017a, The size, distribution, and mobility of landslides caused by the 2015 Mw7.8 Gorkha earthquake, Nepal: Geomorphology.

Roback, Kevin, Clark, M.K., West, A.J., Zekkos, Dimitrios, Li, Gen, Gallen, S.F., Champlain, Deepak, and Godt, J.W., 2017b, Map data of landslides triggered by the 25 April 2015 Mw 7.8 Gorkha, Nepal earthquake: U.S. Geological Survey data release, https://doi.org/10.5066/F7DZ06F9.

Xu, C., Xu, X., Yao, X., and Dai, F., 2014, Three (nearly) complete inventories of landslides triggered by the May 12, 2008 Wenchuan Mw 7.9 earthquake of China and their spatial distribution statistical analysis: Landslides, v. 11, no. 3, p. 441-461.