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Interactive
comment

Interactive comment on "Seismic and geologic controls on spatial clustering of landslides in three large earthquakes" *by* C. Rault et al.

Anonymous Referee #1

In this paper, the authors explore ridge and toe clustering of earthquake-triggered landslides compared to rainfall-triggered landslides and seek explanations for different patterns, particularly related to geology. While, if taken at face value, their results are compelling and relevant, the problem is the paper and especially its supplement lack sufficient clarity for me to determine if their methods are scientifically sound and unbiased.

It was very difficult to follow the methods description, which is mainly detailed in the supplement, because of English usage problems and misspellings, but also lack of clarity and logical gaps that require more explanation. This may be partially because I don't specialize in probability and statistics, so it may be worth having someone with more expertise in that area review this instead of me if revised or resubmitted. But

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even so, most readers are not going to be specialists in probability and statistics either yet they should still be able to follow the steps and logic of what was done. Therefore, I think the authors need to do a substantial rewrite of the methods section and supplement for clarity and have the supplement reviewed for English usage before this is given to another reviewer.

Without having fully understood the methods used by the authors to measure clustering, I do still have a few general concerns. The authors state several times that they confirmed rainfall-induced landslides cluster at toes and earthquake-induced slides often cluster at crests in general in their analysis, but this statement is made based on comparing against just one rainfall inventory. That is not enough evidence to make such a strong, generalized statement. I'm quite sure there are other rainfall induced landslide inventories out there, adding one or two more to the analysis would provide more support to the general statements made.

The implementation of the topographic amplification estimation method needs to be explained in much greater detail, for it's not clear what they actually did to compute MAF, especially given that the method is frequency-dependent but they make no mention of assumptions of S-wave velocities or how they dealt with the frequency-dependence or how they chose the wavelength range they mention. But from what I can understand, I'm not sure their exploration of MAF as an explanatory factor for ridge clustering is not really telling us much, given the huge uncertainties in estimating topographic amplification, that they average the results over the very large macrocells, and the fact that the method they use is based on numerical models and has not really been proven effective with real data yet. Note, there is an alternative empirical method that is not mentioned in the paper that might be worth comparing because in contrast to the Maufroy method, it is based on data. The downside is its specific to California data. Rai et al., 2016 https://doi.org/10.1193/113014EQS202M

The authors are also clearly aware that using the entire polygon of a landslide biases their results and they explore this. They do a small analysis of the centroids of the

source areas for the Chi-chi earthquake and state that it shows the same pattern, concluding that they can disregard the potential bias, but I'm not convinced they did a thorough job of ruling out bias because they derived the source areas from the original polygons automatically based on a simple assumption of typical aspect ratio and only looked at one inventory. I would be more convinced if they instead used one of the several inventories that did map source areas and deposits separately [e.g., Gorkha (Roback et al., 2017), Mid-Niigata (GSI of Japan, 2005), Kaikoura, (Massey et al., 2018)] to show whether considering entire polygons biases the results.

Line-specific comments

L13 – The word "confirms" is a little strong for a conclusion based on comparison against one rainfall inventory

L14-15 – Stating that seismic ground parameters have little bearing on observed patterns is pretty problematic because the landslides wouldn't have happened without the ground motion. The greater likelihood is that we don't have the means to accurately estimate the relevant ground motion parameters at the site where landslides are often triggered. Perhaps rather than saying "have little bearing" one could instead say ground motion parameters from ShakeMap do not seem to exert a primary control on observed clustering patterns.

L16-17 By major faults, do the authors mean faults involved in the earthquake that triggered the landslides or all faults?

L20-21 I don't think anyone is suggesting that landslide clustering be used as an indicator of seismic parameters. . .

L61 – give reference/source of reported PGA's here and elsewhere. Use commas for thousands in English, dots for decimals.

L95 – What does a random draw of landslide positions with no external forcing even mean? Pretty much every landslide occurs due to external forcing. Perhaps this section

needs to be rewritten for clarity?

L121 – How is the Maufroy method actually implemented? Not nearly enough detail is given. The method is frequency-specific, but the relevant frequency depends on the scale of the feature and the wavelength depends on the shear wave velocity. Also how is this applied to the ground motions, none of the ShakeMap outputs are frequency-specific except the spectral accelerations, but those are single degree of freedom oscillators with a specific natural frequency, which is not the same thing as ground motion of a specific frequency content. This method is also based on modeling results and to my knowledge, hasn't yet really been validated against real data so I'm a little skeptical that this analysis is telling us much. It's not clear how the Paolucci method is used in the study.

L130 – Provide some information about the scale of mapping for each of these maps.

L143-144 – It is problematic to make such a general statement about all landslides based on an analysis of three watersheds in one location.

L154 – Crest-clustering is not dominant for Chi chi either, I'd estimate that more than half of the cells are blue.

L163 – Also true for Fig 3

L182-185 Clarify if these values were adjusted somehow for topographic amplification, as described earlier, or if this is just showing the values directly reported by ShakeMap

L189 – At what DEM scale is the MAF computed? This definitely could benefit from more clarity earlier on how the MAF was actually computed, (i.e., at what scale) and then presumably averaged over macrocells. Wouldn't averaging it over such large areas tend to remove any possible correlations?

L230 – What is a dissolution figure?

L249 – Do the authors mean the landslides occurred when slopes were parallel to the

stratigraphic dip? That is what is implied by the cited figure.

Supplement

It was so frustrating to try to follow the supplement given its lack of clarity and poor English usage that I did not even try to provide comprehensive comments for it. The whole thing needs to be rewritten for clarity before it can be reviewed for scientific content. Some of the figures were hard to follow as well, especially Figure S8, which looks like a headless stick figure. It needs something for reference.

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