REVIEW

A data-driven evaluation of post-fire landslide susceptibility

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REMARKS FOR THE AUTHOR

The paper focus on the differences between the occurrence of rainfall-triggered mass movements at unburned and unburned locations working at small scale (i.e. over large areas) using landslide occurrence from a global catalogue and rainfall data. Despite the authors did a consistent set of analyses, there are many uncertainties on what they found with many aspects of the analysis, results and conclusion to be clarified, modified or avoided. In many parts the authors attempt to speculate on specific process differences but without proper evidences, that probably could not be solved at this scale, unless specific process data are provided and analysed. Please find in the following the specific comments to the manuscript.

COMMENTS AND SUGGESTION TO THE AUTHOR

ABSTRACT

Page 1 **Line 1** Landslide and debris flows are just a part of the geo-hydrological phenomena that can be impacted be fires. Please mention also the other phenomena or clarify that these are only some of them.

Page 1 **Line 4** Please specify why GLC should facilitate regional inter-comparison?

Page 1 **Line 8** The authors here speculate on the seasonality of "mass movement-triggering storms" but actually this should be read as the seasonality of mass movements triggered by rainfall. In addition, please specify what "other rainfall-triggered mass movements" means; here is too generic.

Page 1 **Line 12** "… characteristics of rainfall-triggered mass movements …".in general, or only fire-related?

TEXT

Page 1 **Line 15** Please specify what the author intend for "path"? Propagation, runout path?

Page 1 **Line 24** "sediment-laden floods" or "sediment-laden flows", since the authors use the term mass- movements, I assume they refer to the second.

Page 1 Line 29 Since this is a generic/general statement should be referred not necessarily to US recent works. In addition, I suggest to avoid defining "meteorology" or "length of time since the most recent fire" as factors, since those may be just indirect ways to refer to proper landslide conditioning factors.

Page 2 Line 36 This sentence is really cryptic. What is the "relative magnitude of triggering precipitation events", why this should be a proxy for the susceptibility? Is this a result of the study or is an initial assumption?

Page 4 Line 90 Here use the term "widely recognized relationship" in place of "statistically significant positive relationship", since you are mostly referring to literature and not to specific statistical tests results.

Page 4 Line 100 "is a universal phenomenon" seems a bit ambitious here.

Page 5 Line 124 "precision" or "accuracy", here and at line 132 you are using the wo worlds but they do not refer to the same problem. In addition, completeness may not be necessarily a problem when using inferential statistics.

Page 5 Line 145 Here and in the rest of the text, the readers have the impression that the authors just considered part (mostly US) of the literature. I'm not a specific expert of fire related mass movements, but just a quick search on the main scientific literature search engines revealed also specific studies in other part of the world. Since the authors use this lack of local studies as one if not the main justification for the work, this problem is relevant. Hence, please account also the other studies in different countries and modify the text accordingly.

Page 5 Line 151 What is the rationale behind the choice of "the seven-day running total precipitation depth percentile for the 30 days surrounding the day of the year"? Which percentile do the authors refer to? Which day of the year is used? Need to clarify why these should be used as a "proxy for mass movement susceptibility". The rest of the paragraph till the beginning of section 2.1 give a series of details that are just confusing the reader. This is really cryptic, for this reason I suggest the authors to simplify this part, identifying the methodology with "understandable" steps and demanding the specificities and explanations of methods to the dedicated sections.

Page 6 Line 162 It will be more correct defining this a "sample" and not a "large sample".

Page 6 Line 174 What does "recorded locations" refer to? Fire location?

Page 6 Line 180 Please explain how the "hierarchical clustering algorithm" based solely on latitude and longitude is able to highlight/account for climate differences. This will be an important information to complete the description of the procedure.

Page 9 Line 205 Event the opposite case is possible. Please comment in the text.

Page 9 Line 218 What does "significantly significant differences" mean?

Page 10 Line 231   It is not clear how a "7-day running average of antecedent precipitation" is able to highlight "storms of different lengths and intensities"? Please specify.

Page 10 Line 233   Please specify what type of "7-day antecedent rainfall indices".

Page 10 Line 234   "more equal comparison of mass movement triggers which fall within throughout this spectrum of storm intensity"? This is really cryptic. Please specify. In addition, "is less sensitive to small errors in precipitation" is really tautologic, since this is an aggregated measures; better removing it. It is unclear why being "less sensitive to mass movement date accuracy" is an advantage for this type of analysis. Maybe this hides important causal relations between rainfall and post-fire landslide occurrences, in line with what you have mentioned before about the importance of runoff related phenomena compared to the infiltration related once.

Page 10 Line 239   The sentence is not clear! Please rephrase!

Page 11 Line 256   How do you exactly normalized these value (i.e. which kind of calculation did you do)? Please also explain exactly why the normalization you are performing should "facilitate the comparison of mass movement-triggering events across a variety of seasons and climates". Is this based on previous study? Please also justify why "this statistic controls for geographic and seasonal differences across mass movement events". Which is the rationale behind that?

Page 11 Line 265   Again, here and hereafter, it's unclear why the percentile should serve as a proxy for relative mass movement susceptibility?

Page 11 Line 270   Mann–Whitney test does compare ranks and not median directly, or at least it may compare medians under certain circumstances and distribution assumptions (i.e. the two samples should have the same shape). Please check.

Page 11 Line 272   The authors do not provide any evidence on the fact that data are uniformly distributed, and is unclear how Sect 2.4 should guarantee this. In addition, Mann–Whitney test does require any uniform distribution. Instead it requires that the two analysed samples should be measured on a ordinal or continuous scale and should not normally distributed.

Page 11 Line 280   It is unclear what " …with the actual sample number adjusted by region so that all sites were selected evenly" mean.

Page 11 Line 283   What do you intend whit "lead time"? Is this the time of occurrence of the mass movement?

Page 11 Line 283   If I well understood you "control dataset" include those rainfall cumulative values in the selected accumulation period (i.e. the 7 day period) in a period of 15 days before and after the date of landslide. This assume a pretty constant rainfall characteristics over time and in particular over the same 30 days period across all

the years. Given the general rainfall variability I'm not sure this 30-days period length is large enough to considered all the possible climatic variability in the selected area, and I believe it should be better to consider a seasonal period to estimate the "control" references. Alternatively, the authors could show the influence of the selection of the length of the "control" period on their results. This comment is someway related to the previous comment "Page 10 Line 231", please consider them related (the larger is this accumulation period, the larger should be the control period).

Page 11 Line 295 What do the authors intend for seasonality? Please give a definition or refer to a reference one.

Page 12 Line 301 "frequency" of what?

Page 12 Line 301 "These persistent differences between burned and unburned sites were removed by subtracting the mean precipitation frequency for both the burned and unburned groups": is the intent of the authors to perform a variable scaling? Why don't they do a full variable standardization dividing by the standard deviation? Please explain better how this should be useful in the analysis?

Page 13 Line 319 From here to the end of the paragraph: so basically, you got confirmation on the effect or fires only in the area in US with previous studies, while your main hypothesis is to test this possible effect worldwide. Please comment

Page 13 Line 326 Figure 4 and figure 5 are only showing the p-value. Without boxplots it is impossible to check whether a p-value greater than 0.05 correspond to a precipitation percentile in burned areas lower than the unburned ones. This is a relevant information for the analyses. I suggest to realize plots or multiple plots similar to that in fig 3 or modifying the plot in a way to show such information. Please homogenize the p-value threshold descriptions in the figure captions.

Page 17 Line 343 I assume the figure 6 refers to the output of Mann–Whitney test even if in they axis is specified "Wilcox" test, which actually should read a "Wilcoxon". Maybe this is just a refuse since the Mann-Whitney test is also called Wilcoxon rank-sum test, but there is a completely different Wilcoxon test called "Wilcoxon signed-rank test" which test something different. Since some of the interpretation of p-value done by the authors make me think on the use of this last, I ask them to check. Indeed, it seems (i.e. al least observing plots styles) that these test have been done in R which uses the same function (i.e. *wilcox.test()* fuction) to perform different types of Wilcoxon tests. Plase highlight the meaning of the different colour tones in the caption/tex.t

Page 17 Line 349 "with lower values": similarly, to comment Page 13 Line 326, p-value may just highlight some difference but not their positive or negative sing. This information in figure 6 can be only appreciated in panels (h)-(u) but only for the case of the day-of-landslide precipitation.

Page 17 Line 357   "with implications for potentially region-specific physical processes associated with mass movement triggers": the analysis of timing of rainfall/storms in different regions cannot say anything in this regard, these may only say something on the rainfall characteristics leading to landslides which presumably depends from regional climatic difference.

Page 17 Line 358   "in the Himalayas and Southeast Asia (Fig. 6 panels (f) and (g)) precipitation rises at a similar rate for each group": It is really difficult to appreciate this from the figure! Indeed, there seems to be substantial differences between burned and unburned areas. In addition, from here to the end of the paragraph all the speculations/comments of the rainfall intensity do not find support from the analysis of the figure, or at least the authors do not provide all the information to appreciate this (only panels (h)-(u) may provide such information). For instance, why "In the Pacific Northwest and California (Fig. 6 panels (d) and (b)), the burned sites exhibit shorter but more intense storms than the unburned sites in the week preceding the mass movement"?

Page 19 Line 371   This does not seems the case for all the panels, but is almost impossible to appreciate correctly. Please add 0.9 p-value line or at least their ticks in Figure.

Page 19 Line 378   Fig.5 should probably read as Fig. 7.

Page 19 Line 381   4854%? Please check.

Page 19 Line 384   Maybe hereafter, it will be worthwhile to mention the rainfall season. This is the really relevant information to speculate on landslide occurrence (e.g. in the Himalaya the monsoon period is generally in June and bring almost the entire yearly rainfall leading to landslides, but this is not the same for other regions characterized by different seasonal rainfall regimes and distributions). How the analysis on seasonality account this?

Page 19 Line 388   Please see previous comments on this type of plots (Figure 4 and 5), and on their possible interpretation regarding rainfall.

Page 19 Line 393   Why? p-value > 0.05 only for "less then 1 year"

Page 19 Line 395   Why? The p-values are always > 0.05 for all the lines. Here the authors seem using a p-value interpretation opposite to what in Fig 4 and 5 which should be the correct one. Is something missing here for the interpretation?

Page 19 Line 415   This is certainly visible for California, but not for Himalayas!

Page 19 Line 427   In the view of the comments above, the discussion may be revised carefully and have significant changes. In the section some points are really too much speculative with no support from the data and results described in the manuscript: please revise these parts and indicate appropriate references to support them.

Page 19 Line 505   In the view of the comments above, also the conclusion may be revised carefully and have significant changes. Please maintain only the relevant findings, avoiding really speculative conclusions not well supported by data and analyses' results