Dear Editor,

Thank you for coordinating two useful and comprehensive reviews of our study. We appreciate the insight provided by both reviewers and the commenter, and below we address each of the comments in turn. In line with the guidelines, we have provided the outline of the planned changes to the paper but have not submitted a revised version. We look forward to hearing back and to further feedback.

Yours faithfully,

Robert Emberson, on behalf of all authors

R1:

This is an interesting and highly timely manuscript that will make a strong contribution to our understanding of rainfall-triggered landslide occurrence. It builds logically on prior work by the authors and others on large-scale analysis of the growing body of landslide inventories, and also adds to those inventories by providing some additional datasets to the community. I strongly recommend publication after fairly minor revisions. Most of my comments are aimed at clarifying aspects of what the authors have done. The writing is mostly clear, but in places there are some minor inconsistencies or elements of the explanation that feel less complete or a little rushed. I've made some comments and suggestions on the annotated PDF; I won't repeat all of those here, but instead I've included the more substantive comments below, tied to line number.

We thank the reviewer for their supportive comments and appreciate their effort to review our study. We feel that the comments made are both fair and constructive, and we will in revision work to incorporate their input. Below, we provide detailed responses to the individual comments.

Line 80: I agree with all of the statements in this introduction, but at the same time I feel like part of the argument is missing. The previous paragraphs make the case for why inventories are important and why landslide location data are useful, but there isn't really a problem statement - what is the gap that the authors are trying to fill? The abstract makes the point that prior work (like that of Milledge et al. NHESS, and also work by Jeandet and Gallen and colleagues) has looked at the characteristic locations of coseismic landslides to try to derive some underlying patterns or rules that can be useful for understanding exposure. It's logical to extend that work to look at rainfall-triggered landslides, for all of the reasons listed above, but that point isn't made explicitly here. I think the paper would be strengthened by making that case in the intro, to help convince the reader of why this is needed. This need not be a lengthy addition, but a few extra sentences could make a big difference.

This is a great point. We will include an additional sentence near the close of the introduction:

"Finally, recent studies have sought to derive underlying simple topographic rules to understand hazard associated with earthquake-triggered landslides [e.g. Milledge et al. 2019], and it is important that we extend this kind of analysis to rainfall triggered events to provide comparative data.

83-84: 'We suggest that...' - I agree, but for the reasons outlined above I think this also undersells what you are doing. If there really are patterns or rules that can be gleaned from this kind of analysis, then that is incredibly valuable for hazard assessment in areas where more detailed models or investigation have not or cannot take place. Similarly, if there are no such patterns, that's also valuable. Milledge et al. 2019 NHESS made this point explicitly and I think you could do the same here.

Again, we appreciate the reviewer pushing us to amplify the value of our study. We will add an additional sentence to the closing paragraph of the introduction:

"Moreover, with a set of simplified rules for landslide hazard, researchers can support hazard assessment in areas where more detailed models may be unavailable."

119: This point about image resolution is a tricky one, because it depends upon the definition of completeness - even 1 m imagery will not catch every event, especially in areas of either sparse vegetation or very dense vegetation (where for example small events are still hidden under the canopy, or contained within channels). I mostly agree with this statement, but it is an oversimplification to say that higher resolution = 'better' in all cases. It's also true that high resolution imagery suffers more from issues with rectification (and sometimes thus georeferencing), which can have an impact on inventory creation and analysis - again, see Williams et al. 2018 NHESS for an example of this.

We will add a caveat statement to the end of this paragraph to clarify that higher resolution imagery does not always lead to better quality inventories:

"It is important to note that although high resolution imagery can provide more accurate mapping in some cases, it can also be more challenging to ortho-rectify, which can limit the quality of landslide inventories generated [Williams et al. 2018]"

138: 'We therefore consider...' - I'm sure you're right, but this is a pretty broad-brush statement. Quality in terms of landslide location, or information on size and geometry, or both? Does it matter for 'quality' that two of your new inventories were produced with Sentinel-2 and the others with Planet imagery?

This is a fair comment, and we will remove the statement in revision.

140: Does the 'Date' column in Table 1 represent the dates of the triggering rainfall? What were the time windows over which images were collected (given that those are almost certainly longer)? This has implications for the relationship between rainfall data (which themselves are aggregated or simplified compared to what actually hit the ground) and landslide occurrence – put simply, for many inventories we cannot be certain of when landslides occurred unless the images very closely bracket the dates of the storm. This is relevant because of the discussion of the potential differences between extreme and persistent rainfall in triggering landslides in different parts of the landscape (lines 505-510).

The date in Table 1 does indeed refer to the date of triggering rainfall. For the newer inventories, Planet data allows us to closely bracket the rainfall (within a few days) and many of these inventories are the result of rainfall that exceeds annual averages by a significant margin, suggesting that the bulk of landslides are generated during short intense bursts of rain that we consider. However, the reviewer is correct that this issue may be important, and so we will include the following statement to highlight this potential issue:

"It is important to note that the date of the triggering rainfall is not identical to the dates the imagery used to map the landslides was obtained. Although we have selected events where the triggering rainfall significantly exceeds historical peak rainfall (and therefore is likely to be the dominant trigger for landslides) some events may have occurred as a result of lesser rainfall before or after. While the new inventories generated for this study that utilise Planet imagery that closely brackets the rainfall events (within 1 week either side), the older inventories may be more subject to this challenge."

235: Does this mean that you have rasterised all of your inventories? Are you assuming some kind of majority rule to go from landslide polygons (in your inventories) to pixels (for comparison to the continuous raster variables)? And is the resolution of those rasters 1 arcsec? Are you therefore censoring the smaller events? I think this needs to be clarified. Further down in the ms it emerges that in fact the inventories have been rasterised and with a 'presence' rather than 'majority' rule, but to address the questions that I had at this point in the ms, I think that information needs to come sooner.

We will move the paragraph explaining the GLM methodology to later in the text to ensure that the description of landslide pixelation occurs prior to where it is explained in the GLM context.

253-254: 'We utilize the method...' - I think it would be useful to give a very brief description of how this is done, so that the reader does not need to go to the Marc et al. (2018) paper to understand this.

We will add the following explanatory text to describe the method:

"We utilize the method of Marc et al. [2018] to extract the scar areas, which uses the perimeter and area (A) of landslide polygons to calculate aspect ratio of an equivalent ellipse, K, (Marc and Hovius 2015) and the associated width (W) , according to the formula $W \cong \sqrt{(4A/\pi K)}$ (Marc et al., 2018). The scar area is defined as ~$1.5W^2$ based on a global database of scar aspect ratio (Domej et al 2017)."

Related to this: I think you use 'scar' and 'headscarp' interchangeably throughout the ms, and I would suggest focusing on one or the other. To me, the headscarp is a particular part of the scar area - they are not synonymous

We will replace all uses of 'headscarp' with 'scar'; thank you for flagging this.

406: 'While we have attempted…' - this does raise the question, though, of how we 'ground truth' landslide inventories in a meaningful and rational way. I think that is an issue that's beyond the scope of this ms, but at the same time there is an implicit assumption in this kind of phrasing that inventories can be 'corrected' by hand and presumably brought closer to a true representation of landslide occurrence. Whether that's the case or not and what we mean by 'better' are both open to discussion. I'm not suggesting that you address these points in any detail, but some recognition that 'correction' is challenging might be useful here.

The reviewer raises a good point here. We agree that hand-correcting is not necessarily the gold standard, with subjectivity an important problem. We will rephrase the highlighted sentence as follows:

"Although we have used hand-corrections to reduce the impact of polygon amalgamation from algorithmic mapping methods, some inconsistencies may still exist."

450-453: 'Characterizing these parameters…' - this is a little confusing, because you HAVE used a globally available rainfall product as well as a globally available forest cover and loss product. So I don't see how the final sentence is correct. Or am I misunderstanding what you mean?

We acknowledge the potential for confusion here and will rephrase this sentence as follows:

"Although global data for rainfall, soil type and geological parameters exists, the resolution of these datasets is too low to allow for consistent comparison of landslide and non-landslide areas at the scale of the analysis described here (~100 m)."

506-510: 'Nevertheless, we also suggest…' - I agree that this would be a really instructive comparison, and it's great to point that out here. But this also raises the issue that I flagged above, of the difference between (1) the time window over which high-intensity rainfall occurred in these areas and (2) the time window over which landslides were mapped. If the latter is much greater than the former, then I think you need to be a little careful about the inferences you draw from the landslide patterns. I don't think this invalidates what you have done, but I think it's important enough to warrant a mention.

We thank the reviewer for raising this important point. We will add the following clarifying sentence to the end of the paragraph to ensure this point is communicated effectively:

"In any future comparative study of low-intensity and high-intensity rainfall events, it will be necessary to carefully select landslide inventories where the imagery used to generate them closely brackets the start and end of the rainfall events, to ensure only landslides triggered by an individual event are analysed."

Figures

Despite the fact that Figs 3-8 all show essentially the same thing, you use variable labels for both x- and y-axes. I suggest choosing a consistent naming convention and sticking to it, to make things easier for the reader. Otherwise, it's necessary to repeatedly work out what is being shown.

The reviewer raises a good point here, and we will revise the figures to ensure consistency.

Also, the color scheme used in these figures for the different inventories is really hard to distinguish. Maybe this doesn't matter, if the point is to show that all datasets show similar performance, but picking out an individual dataset and distinguishing it from the others (which is required to evaluate some of the statements in the text) is really tough. Perhaps use a progressive colour scheme instead (e.g., blue to white to red)?

Although we appreciate the point the reviewer is raising, we have tried multiple different colour schemes and still prefer the current system we are using. Progressive colour schemes with so many different inventories make it extremely challenging to pick out which dataset is which.

Fig 3: the x axis labels are not very clear or informative on panels a and b. I think these could usefully be made more precise.

We will revise these labels to ensure clarity in revision.

Figs 4-5: it’s not clear why the x axes are reported to different levels of precision here (integers on the one hand, real numbers on the other).

We will revise these figures to ensure that there is a consistent choice for data – we will use real numbers for all data.

Fig 11: this figure isn't really explained in the text, and the axis labels here are identical to those for Figs 6 and 9. The reader could therefore be forgiven for not recognising what you are showing, and not really understanding how this figure is distinct from those earlier figures. I think it would be good to provide a more clear explanation of what this figure shows and how it is distinct from the other figs. The caption is factual but confusingly worded, and as a result I don't think this is very effective.

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R2:

This work by Emberson et al. adds to the existing rainfall induced landslides datasets and explores different topographic parameters associated with these landslides. These parameters are used to charactering landslide locations and differentiate scarps from the rest of the landslide body. While I think the overall analysis is well done, much work is needed in the writing to better communicate the methods and results. As it stands, these sections lack an appropriate level of organization and clarity required for publication. With that said, I do believe with some rewriting this paper will be a significant contribution to the field of landslide hazard mapping and should be published. Below I provide a few general comments and then provide some specific edits.

We greatly appreciate the effort and detailed comments from the reviewer. Their feedback has served to improve the clarity of our study and we further acknowledge the supportive comments from the reviewer about the value of our study. We broadly agree with all of the comments from the reviewer and feel that there are no major issues to revising our study to incorporate them. Below, we detail the individual responses to each of the comments.

General Comments:

The Paragraph formatting seemed a bit off. Probably just a pdf artifact, but double check to make sure the paragraphs are clearly delineated as you want them. As it stands, the apparent paragraph splits made it hard to follow in spots.

We will fix this issue when revising the paper; thank you for flagging it.

Supplemental Figure naming is inconsistent.

This will be resolved in revision.

The methods really need a lot of work. It was difficult to follow what you were talking about and that made the results also difficult to follow. I've included a few suggestions, but I believe they largely need to be rewritten to improve the flow and organization. There was a lot of jumping back and forth between ideas in the paragraphs. Another thing that would help the reader is using consistent and clearly defined terminology throughout the methods and results.

The reviewer provided a number of extremely helpful detailed comments below to help guide the revision of this section. We acknowledge that more clarity is needed and we will revise this section extensively to resolve this. See detailed comments below.

More references to your great figures need to be included in the text. There were several spots where I wanted to know which figure to look at to see what you the writers were describing but there was none.

Further figure references will be added.

There are a few instances of using round brackets for citations.

These will be resolved in revision.

Grammar could use a little work. I pointed it out in a few locations, but rereading it with only grammar in mind would help.

We will conduct a full grammatical check when revising our study.

Specific Comments:

Line 8: "4" should be superscript

We will fix this.

Line 30: This second sentence seemingly contradicts the third. Is your point that some landslides not induced by rainfall or seismic activity are more localized while those that are can have a broader footprint? Or that small localized events can occur within a broad area? Please clarify.

We recognise the potential for confusion here and will rephrase the third sentence:
" Although the impacts of individual landslides may be localized, large triggering events such as intense rainfall or seismic activity can cause large numbers of landslides across a wide region, the extent of which often mirrors the extent of the intense rainfall and seismic shaking"

Line 46: I would add Mirus et al., 2020

We will add this reference.

Line 59: Omit "for instance". It muddles the sentence.

We will remove 'for instance'.

Section 2.2: Paragraphs in this section were really distracting. If this is not a pdf error, please rework paragraphs to make the flow better.

We thank the reviewer for highlighting these issues. We will, in revision, work to fix the formatting issues and ensure the text flows appropriately.

Line 133: Is this supposed to be a new paragraph? If so, it's not a great introductory sentence since it only includes half of the new data described in the paragraph.

This was not intended to be a new paragraph, and we will amend this mistake to improve flow.

Line 134: Reference Table 1

We will reference Table 1.

Line 137: Why did you use two different approaches for making the inventories? Was it a question of data availability? Should be addressed.

We will clarify the reason with the following sentence:

"The algorithmic method was used to reduce the overall time spent mapping some of the larger new inventories."

Line 152: 'landslide' should be 'landslides'

We will change to the suggested ‘landslides’

Line 152: I don't follow why debris flows were omitted. Is it just so you can estimate debris volume? That can't be done with debris flows? Or is there another reason I missed?

This is an important point; we remove debris flows to focus solely on the process of landsliding, rather than the characteristics of preferential runout paths. It also allows us to provide consistent datasets that can be used in future studies to assess landslide volumes that can then be combined into more general runout assessments. We will add the following final clause to the sentence in question:

“and permits a focus solely on the topographic characteristics of landslide source regions, rather than the characteristics of preferential runout paths.”

Line 153: Please specify volume of what.

We will add the clarifying statement: “of mobilised landslide material”

Line 156: Omit comma after ‘interval’

We will remove the comma.

Line 160: Should this be a new paragraph?

We will adjust the formatting here to ensure that the paragraphs are more clearly defined.

Line 163: Marc et al., [2018]

We will change this to the suggested edit.

Line 182: Consider showing a figure of this. It’rs hard to tell from the table.

While we acknowledge the reviewer’s point here, we prefer to avoid including any figures describing the rainfall – landslide density relationship since it would potentially confuse the more focused points we are making regarding topographic relationships. The rainfall-landslide density relationship deserves significant time and research focus and our belief is that it would be preferable to do so in a more expansive way, with another research paper focused on this in the future.

Line 191: What characteristics? How does this prevent inconsistencies? I found it hard to see the context of this paragraph. More detail I think may help.

We will rephrase this sentence to the following to avoid confusion:

“We have analyzed the topographic characteristics of landslide locations for the event inventories, using global satellite datasets to ensure consistency across each site.”

Table 3: “Analysis datasets. Explanation of each of the variables in found is the accompanying text.”

We are not clear what the reviewer means by flagging this text.

Line 196: Direct comparison of what?

We will rephrase this sentence to the following:

“this means we do not have to resample either dataset when conducting a raster-based analysis at this scale”

Line 205: Insert “(TPI)”

We will insert ‘(TPI)’ as suggested.

Line 211: An equation describing this parameter (and TPI) or a more precise definition would be helpful. Why do you call CTI the 'wettness index' later on? I would delete that unless you mention it here.

We will add definitions for both of these parameters. We will continue to include the clarification that CTI is sometimes referred to as wetness index, since that provides some context for readers who may have seen one or other of the terms.

Line 215: You never describe TRI. Please do so.

Thank you for flagging this mistake! We will add the following description:

“We also calculate Topographic Ruggedness Index (TRI), a measure of the local surface roughness. It is defined as the root mean squared difference in elevation between a central pixel and each of its eight neighbouring pixels.”

Line 218: The difference between Figure S1 and S2 is not clear. Please provide more descriptive captions.

We will add the following sentences to the caption for S1 and S2 to provide clarification:

“The difference between figure S1 and S2 is that figure S1 shows the data solely for the pixels containing landslides, whereas S2 shows the data for all pixels in the landscape. This allows comparison of the differences between areas containing landslides and the more general characteristics of the landscape.”

Line 223: How? Are you referring to the relative ratio analysis or the LASSO? Better flow and organization would help readers follow this great work. Consider outlining this portion of the methods in an introductory paragraph. Something needs to be done to help keep the reader oriented in this section.

We acknowledge that the lack of sign posts to the relevant sections may confuse the reader. We will add in references to the relevant sections (bivariate analysis in section 3.1 for the analysis of single parameters, and multivariate analysis in section 3.2 for the LASSO work) as well as clarify that this is the bivariate analysis (see comment for line 285, below).

Line 226: Following up on the previous comment, the remainder of the methods is very difficult to follow. I believe this is largely due to poor organization and unclear language.

We acknowledge that the flow and clarity likely needed improvement. In revision, and thanks to the comments of the reviewer, we intend to make significant changes to resolve these issues. These are detailed in the responses below.

For example, what is meant by topography in this context? I think I figured it out by reading Marc et al., 2018 (which I shouldn't have to do to understand this paper) but it's not clear where the bounds are of the topography you're considering. The whole island, within 10 m of the landslide?

This is a really important point that we did not adequately explain. We will add the following text to clarify:

"The topography values are calculated for all pixels within the area in which landslides were mapped. Since we lack data on the mapped areas for all of the inventories, we assume that the convex hull (minimum bounding polygon) for the landslide polygons represents the mapping area"

Why do you start this paragraph talking about the methods for figure 3, then go into the GLM, then later back to talk about the methods for figure 3 in more detail. Maybe I'm completely missing the mark here. If so, all the more reason to rework this section.

The reviewer raises a good point here. The flow of this section was not working well, so in revision we will move the entire GLM / LASSO discussion to after the bivariate methodology discussion, and add references to where the results are found.

Line 232: Cite figure.

We will add a reference to Figure 3.

Line 235: Please add specific description of your choice of priors.

Thank you so much for this notification. We fully agree with the reviewer comment that the choice of the priors need to be specified in Bayesian analysis. However, in the final version of the manuscript we did not use Bayesian GLM but binomial GLM. This is to say that thanks to your comment we realized that we forgot revising the text. We intended to use Bayesian GLM in the very initial draft of the manuscript but then we switched back to its binomial counterpart. We will revise the text and remove the line mentioning Bayesian GLM, which was obviously used by mistake. We apologize if we have caused confusion during the reading/reviewing process.

The section will read as follows:

"We also apply a feature selection algorithm to identify the significant and irrelevant variables to feed the GLM. For this purpose, we use Least Absolute Shrinkage and Selection Operation (LASSO) technique (Tibshirani, 1996). This method is particularly suggested for landslide susceptibility assessment to reduce the large number of highly correlated predictors without losing parameter interpretability (e.g., Camilo, et al., 2017). GLM fitting with a LASSO implementation is carried out by using the R (Team R, 2013) "glmnet" library, which was made available by Friedman et al. (2009). We apply this method and couple it with the 10-fold cross-validation to remove non-informative covariates and to assess the modeling performance based on the area Under the Receiver Operating Characteristic curve (AUC) calculated for each landslide inventory (Hosmer and Lemeshow, 2000). From each model we built, we store the information related to the regression coefficients. Before fitting the regression model, we apply a mean zero and unit variance normalization to all variables (e.g., Lombardo et al., 2018), which are expressed in different ranges and scales. This normalization allows us to better examine the modelling results in terms of contribution of each variable. In this scheme, larger absolute values of the regression coefficients refer to relatively large contribution of variables."

Line 251: comma after secondly.

We will correct accordingly.

Line 256: “than” should be “as”

We will adjust as suggested.

Line 273: Median of which dataset? the topography, landslide? In the figure you say "landscape" is that just topography, or a combination of topography and landslide. Are the two mutually exclusive?

We will clarify this with the following edits to the statement:

“we calculate the median slope value for all pixels within the mapped area for each inventory “

Line 285: When you describe the bivariate analysis in the methods I would call it by that to better orient the reader.

We will describe the bivariate analysis appropriate above (see response to comment about line 223).

Line 285: In this paragraph, and really throughout the paper, more references to the figures your discussing need to be included.

We thank the reviewer for pointing this out. We will add references to the figures more effectively throughout this section.

Line 288: What figure(s) are you see this in?

We will clarify that this is seen in Figures 4, 5, and 7.

Line 292: I would explicitly say that this is where the y-axis = x-axis = 1. I would also add lines to the plots showing this.

We thank the reviewer for pointing this out. We will add a sentence to clarify that this is case (see below) and also add the lines to the plots.

“This can be observed in Figure 4-8, where the probability ratio of 1 for almost all inventories occurs at approximately the median value of the parameter for the entire landscape.”

Line 297-298: I think this needs to be explained better using the figure as a reference. What are you looking at to pull this out?

We will add a sentence to clarify that this can be observed in Figure 5.

Line 312: Should this be a new paragraph?

Thank you for highlighting this, it should not be a new paragraph. We will correct this formatting issue.

Line 325: I think drawing lines between the points of a given location would help the reader see this better. It would also make the plots generally more interpretable. I suggest doing it to all these plots.

We have tested including a line to connect the points in the figures but we feel that it is not appropriate both for clarity (since the large number of lines become somewhat hard to distinguish) and because it may suggest a more clear relationship particularly at higher values when one does not exist.

Line 338: Please reference the appropriate figure.

We will remove the discussion of the long-wavelength TPI values since the data themselves show no discernible pattern and does not add to the discussion within the paper. This section will be been removed.

Line 444: That the caption for Figure S5 says 'difference' is a bit misleading. Maybe substitute 'comparison'?

We will change this to the suggested ‘comparison’

Line 491: Explain why the evolution of the regolith matters.

We will clarify in revision that the hillslope regolith is an important control on the susceptibility to landslides.

The new text will read: “The Evolution of the hillslopes’ regolith state, which acts as an important control on landslide susceptibility, under climatic forcing is predicted by geomorphological models of hillslope stability coupled with stochastic rainfall forcing (Dietrich et al., 1995, Iida 1999, 2004).”

Line 501: I suggest omitting “Anyway”

We will omit this.

Figure 3: Is landscape slope the same as topography slope?

We will adjust Figure 3 in line with comments from reviewer 1, above, and this will explain that it is the slope of the entire topography. We apologise for the confusion.

This is just an example of how the other figures are made, correct? If so, why do you label the axes differently in the other figures?

Both reviewers highlighted this oversight and we will fix it in revision.

Figure 5: Here and in other figures does "vs Inventory Median" mean the same thing as "/Inventory Median"? If it does, please change it to be consistent. If it doesn't, this needs to be clarified in the text.

As mentioned by reviewer 1, there is a some inconsistency in the figure naming. This will be fixed in revision.

Figure 6: In this figure you don't use the acronym, in others you do. Be consistent.

This will be fixed in revision, thank you for flagging.

Figure 7: Make sure you define what TRI is in the text.

We will add this.

Citation: https://doi.org/10.5194/nhess-2021-250-RC2

Milledge Comment:

This is a really nice paper that compiles an impressive set of inventories and draws several useful and thought provoking conclusions. The most interesting findings each prompted a question for me that I felt it would be helpful (but not essential) if the authors could comment further on.

We thank the commenter for the supportive words. We appreciate their input and the influence their own study had on this research.

First, landslide likelihood exhibits consistent continuous increase with slope across the range of slopes for which there is sufficient data to resolve a likelihood. To me this appears to contradict other recent findings that there is a threshold slope above which landslide likelihood flat-lines or even declines (Marc et al. (2018), for a subset of the inventories examined here, and Prancevic et al (2020), for shallow landslides). Why do you think these studies find such different behaviour?

First one caveat to keep in mind is that it is actually not so straightforward to compare these results to Marc et al 2018 (plotted as a function of S-Sm and not S/Sm) nor Prancevic who plotted S/S10 (the minimal slopes above which 90% landslides occurred).

But in any case we agree these two studies suggested saturation (ie hazard scaling with respect to slope rapidly stopping) was the dominant behaviour in their inventories. What we observe is more a broadening view than a contradiction : Indeed we do find again several cases with such saturation and then a decay (often not in the high significance but still) : Morakot, Kii, Blumenau, Dominica, Hiroshima (etc). In contrast some cases do seems to have increasing hazard until 2-3 times the median slope (Thrissur, West Pokot, Burundi). We can only speculate on the reason for why these cases behave differently: perhaps they are located in landscapes out of equilibrium (Africa, Indian Escarpment) where the median slope is low but steep slopes favoring landslides are still common. Perhaps it relates to the rainfall triggering mechanism which seems not limited over steep slopes (as for one other case in Marc et al 2018). We do not think the main differences lie in the source material (bedrock or soil) as decline occurs for bedrock cases (Morakot, Kii) and increase occurs for likely soil cases (Africa). So, it clearly seems that multiple processes could lead to specific dependence with slope over the steepest slopes (>2median) of a landscape, and that detailed investigations of specific cases should be pursued.

Second, normalising by median slope works well at collapsing the data. This is consistent with the findings of Marc et al 2019 and Prancevic et al 2020 who both collapse the data in a similar way. The connection that you draw to landscape scale strength controls on the slope-likelihood relationship, is really exciting. How do you think this relates to the idea of threshold hillslopes (e.g. Burbank et al., 1996)?

Another excellent question! First we must recall that our work is about instantaneous landscape response to a forcing event, while the threshold hillslope concept is about the emergence (over geomorphic timescales) of a dominant hillslope angle, likely linked to bedrock strength. Clearly since Burbank 1996 and even more now with high resolution datasets we observe sometimes large portions of the landscapes are above the threshold. Simply over long timescales they should be preferentially eroded but it does not mean that every landslide events must focus on these zones. It may depend on the landslide trigger, or on other preparatory factors (weathering, fracturing) not instantaneous but fast relative to landscape geomorphic timescales.

While this certainly warrants further analysis and observation, it may be that the threshold hillslope model can be generalised to consider erosional behaviour below the threshold, the rate of which depends on the distance from median values in a given landscape. As mentioned above, the decay of any relationship significantly above the typical 'threshold' slopes observed in our inventories suggests that the limited parts of the landscape that persist above the threshold are subject to specific conditions (lack of fracturing, fortuitously aligned bedding planes, etc) that prevent the landslide-driven erosion from reducing these areas to the threshold values.

Third, the compound topographic index is not a good predictor of landslide initiation likelihood even in a multiple regression. I don't have a question here but for me this is a very interesting result and your discussion of the implications of this for topographic controls on pore pressure are helpful.

Certainly, we agree here. It's perhaps surprising and we note that looking at landslides triggered perhaps under less intense rain regimes may be appropriate to establish how consistent this relationship is.

Fourth, drainage area is reported as a good predictor of the entire landslide footprint and therefore of landslide hazard. You draw a parallel to Milledge et al. (2019) and I agree in that: 1) both studies highlight the importance of runout for landslide hazard; and 2) drainage area identifies areas of flow concentration. However, Milledge et al. (2019) found that 'hazard area' (which incorporated a slope inclination weighting) rather than simply drainage area was a good predictor of landslide hazard. Unweighted drainage area actually performed fairly poorly in that study. I wonder what you think the reason for this difference might be?

We would suggest that perhaps the difference lies in the different triggering processes associated with the inventories analysed by the respective studies. The runout zones seem to track flow-paths more closely for the rainfall-triggered events than the earthquake triggered ones; however, this remains somewhat hard to explain given we observe unweighted flow accumulation is a poor predictor for the landslide scar areas. An interesting conundrum for future study.

One caveat in flow area interpretation is that the landscape probability decay very strongly from hillslopes (most of the landscape with small to moderate drainage) to channels (very small fraction of landscape with very large drainage area) as it is well shown in Fig 3C of Milledge et al 2019.

For scars (not studied by Milledge et al., 2019), we found a hazard near 1 for flow accumulation near the median: it makes sense as the median is dominated by hillslopes where all scars initiate. Then there is a decay for almost all cases but most bins are not statistically significant (because scars overlapping for channels are rare (or even erroneous))

Flow accumulation for whole landslide is after normalization much more compact than what was found by Milledge 2019, and show a strong decay for area below the median (our cases rather agree with Gorkha and Haiti EQ). This is likely due to the fact that many slides runout until channel areas and thus upslope area (scars) are a small proportion of the whole landslides and are compared to most of the landscape.

It seems to be the case for some EQ and not for others (like Finisterre or ChiChi). Earthquakes that do not display this decay may have more landslides entirely (scar + runout/deposit) distributed in the hillslope domain, because they initiate higher on slopes (Meunier 2008) and/or have less long runout.

In contrast the elevated hazard for large normalized area is roughly consistent for cases with high statistical confidence, but much more diverse and showing reversal (ie decay with increasing drainage area) for many inventories when looking at the less statistically significant portion. Morakot or Zimbabwe show the start of the reversal in their significant part. Still it is true that some event show statistically significant increase of hazard with drainage until very large normalize drainage (like Kii, Thrissur, Burundi) which may relate to a large proportion of long runout landslides (clearly the case for Kii where in the south some landslides where debris flow like)

Finally, one very minor point on the presentation of the results. I wasn't clear what was represented by the landslide likelihood ratio (Figures 3-9). Is this a likelihood ratio, which I understand to be the ratio of likelihoods or is it a ratio that results in a likelihood?

To clarify: this is the ratio of probabilities. We will clarify this in revision.

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