Earth Surf. Dynam. Discuss.,
https://doi.org/10.5194/esurf-2018-20-RC2, 2018

Interactive comment on "Towards a global database of rainfall-induced landslide inventories: first insights from past and new events" *by* Odin Marc et al.

Anonymous Referee #2

Received and published: 16 May 2018

General Comments

In this manuscript, the authors Marc et al. seek to understand what governs the spatial and geometric characteristics of rainfall-induced landslides that result from single storm events. Toward that end, they compile a database of landslide inventories from single storm events spanning the past twenty years. The database is well considered and thorough, and the strengths and limitations of each are discussed (e.g., availability of local rain gauge data, etc.). With this dataset, the authors then compare landslide spatial characteristics (number of landslides, area affected by landslides, landslide density) and geometric characteristics (landslide total area, landslide scar area)

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with precipitation characteristics (rainfall duration, storm intensity, total rainfall). In this analysis, the authors make a number of interesting findings. For example, they find that the longer-duration storms result in an increasing number of lower-gradient landslides. Additionally, they show that while landslide volume and spatial density vary as a nonlinear function of storm total rainfall, other landslide parameters do not appear to depend on storm characteristics such as rainfall intensity. From this analysis the authors conclude that their global inventory of single-event rainfall-induced landslides can be queried to answer fundamental questions about the spatiotemporal evolution of landslides in response to hydrologic forcing.

Understanding at a broad scale how a landscape may respond to a given storm event is fundamentally useful for both geomorphic and hazard assessment applications, and this topic should appeal to the readership of ESurf. The rigorous dataset compilation by the authors provides a sound basis to start better quantifying these relationships, and the statistical methods applied to the dataset are well founded and consistent with those used by the community and should be readily reproducible by other researchers (assuming the database will be available online). The observed correlations between storm magnitude and landslide area draw an interesting parallel to empirical studies of coseismic landslides (e.g., Keefer, 1994), and the data support the authors' conclusions both that rainfall magnitude (expressed as total storm rainfall) is a good indicator of landslide hazard potential and that the increase in lower-sloped landslides occurring over longer duration storms may reflect timescales for water to infiltrate lower portions of the landscape. Although the idea of rainfall-induced landslides occurring in lower-sloping sections of a hillslope is well established (e.g., Reid and Iverson, 1992; Densmore and Hovius, 2000), this work shows that the prominence of this effect potentially depends on the storm magnitude and duration.

In terms of the general aspects of the manuscript presentation and layout, I find that the paper needs some fine-tuning and clarification, but overall it is close to being a finished product. The abstract provides a clear summary of the work, and the overall structure and segmentation of the manuscript is easy to follow. The title largely makes sense, although I find the last section stating “first insights from past and new events” confusing since all datasets are within the past twenty years and the youngest event occurred in 2015, and I’m not sure what that phrase adds to the description of the research. There are a number of places where language needs to be altered slightly, and I’ve tried to provide examples below in the technical comments section of the review. Mathematical formulae appear to be largely correct, but abbreviations for the landslide inventories (although intuitive) are not defined before they are used. Additionally, I found that Figure 2 should be modified, as it is very difficult to see the pink landslide polygons draped over the red and green topography. I imagine this would be especially difficult for people who are red-green colorblind. The supplementary material complements the manuscript well, and I have a few comments regarding supplementary figures below. Although I have a few additional concerns related to content and clarification, overall I think this paper will make an interesting contribution to ESurf.

Specific Comments

Page 2

1st Paragraph: I’m not sure I agree with the statement that the goal of constraining quantitative relationships between landslide occurrence and rainfall is out of reach. The authors cite examples of this in the same paragraph. I do agree that there is certainly room for improvement in his area, which I think is the implied sentiment here.

Equation 1. I appreciate that the authors’ goal here is to try to bridge the gap between purely deterministic models and purely statistical models, but I think that there needs to be a little more clarity. At the end of Paragraph 1, for example, you state that certain parameters such as permeability and cohesion that are required for deterministic approaches make a landscape-scale approach in data-poor regions inapplicable, yet you specifically include those parameters in your idealized semi-deterministic Equation 1. Why then is a deterministic approach not appropriate? I think that a bit more discussion might clarify these discrepancies.

Page 4

Lines 31-34: I'm confused by this sentence. When specifically are data from 2010-2012 used? When May-June 2009 data are not available for a specific location in the landslide-affected area?

Section 2.2 overall.

I'm also confused with the general methodology here. You map landslides on 30 m Landsat imagery, as well as on higher resolution imagery within Google Earth, but only in areas where a negative change in NDVI was observed at the 30 m scale. You then say that field mapping in the area reports twice as many landslides than was observed via remote sensing, but that the missing landslides must be smaller than ∼1 m resolution. Could the missing landslides not just be in areas that didn't result in a negative NDVI shift in the landsat imagery? For example, a small translation or slump in a forested area may not affect a 30 m pixel.

Page 11

Lines 23-27: In your discussion of peculiar landslide frequency distributions, you focus on deviations from the (perhaps) expected Inverse-Gamma distributions at the large end of the distributions. What about deviations on the smaller end? For example, in the Total Area distributions, TW9, B11, C99, and J11 deviation from the maximum likelihood estimations pretty substantially for small landslide areas. Converting total landslides area to landslide scar area (As), the TW9 distribution especially deviates quite far from the expected P values. Is there a known reason for these deviations? I am far from an expert on landslide frequency distributions, but it seems worth discussing since it is quite apparent on Figure 3!

Figure 3: Similarly, I don't believe it is mentioned why the authors choose to break up their landslide populations into two groups. Is this just to more easily visualize? Or is it

based on the quality of datasets?

Page 15

Lines 11-12: Is there a plot that shows the relationship described on these lines? I couldn't find one. Maybe it would be worth including these in the supplemental material.

Page 16

Lines 8-9: Very cool.

Page 18

Lines 12-15: If there is a continuous forcing of heavy rainfall over an extended period of time, it is not clear to me why a monsoon would not fit in with the scaling relationships derived in this paper. Would that not be an end-member condition for considering the role of water infiltration in setting the spatial distribution of landslides on lower slopes? If not, then why not? I imagine other people not as familiar with monsoon dynamics like myself might ask the same question.

Page 19

Line 14: Does the proportion of flat ground affect the slide aspect ratios as well, since the flat ground may provide more accommodation space for runout?

Line 22: This sentence cannot be true, as Figs. 6 and 7b all show a relationship between storm metrics on landslide scar areas. Do you mean other storm metrics outside of storm total rainfall?

Page 20

Line 20: This is almost certainly true, especially for the smaller-area landslides that depend on local slope smaller than what a 30 m pixel can resolve.

Technical Corrections

Page 1

Line 3: "..we have very few datasets of rainfall-induced landslides." I think this should be clarified that this is the case only for single-event inventories.

Line 6: should be "orders of magnitude"

Line 8: "The non-linear scaling with total rainfall." Two notes: 1) "nonlinear" is one word; 2) the variable that is being scaled with total rainfall should be specified.

Line 11: "contrarily" should be "contrary"

Line 18: "..itself expected to increase with global change." I find this sentence slightly confusing. Consider replacing "itself" with "which are"

Page 2

Line 26: should be "This progress has been possible"

Page 3

Line 5: Should be "is needed"

Line 7: Sentence fragment ". . .affected by the storm, and thus . "

Lines 13-18: Nice overview!

Line 20: should be "datasets" (plural)

Line 25: should be "The rainfall was"

Line 30: "details" should be singular

Sections 2.1 and 2.2 – Introduce acronyms (e.g., B08) as you introduce each dataset.

Page 4

Line 29: "(30m)" I think a space usually goes between the value and the units, e.g., "30 m". This should be done consistently throughout the manuscript.

Figure 1. It would be very helpful to make the landslide inventory polygons and rain

gauge stations contrast more with the background. The topography could easily be represented as a hillshade since the absolute elevations are not the focus of the figure. Also, the panels are not labeled with letters as described in the caption.

In the caption, “Landslides inventory” should be “Landslide inventories”

Page 7

Line 2: “sub-parts” could be “subsets” perhaps?

Page 8

Lines 2-3: The values and units appear italicized in one instance and standard font in the other. Should be consistently reported.

Lines 5-6: I think “rain gauge” is the correct spelling

Lines 13-14: Is the correlation between D and Rt and I3 and Rt shown anywhere? May be good for the Supplemental.

Line 21: Probably good to put a reference here where the increased runout for larger volume slides is discussed (e.g., Legros, 2002).

Page 10

Line 8: “However, for subset with less” should be “However, for subsets with fewer”

Line 10: “uncertainties” should be “uncertainty”

Page 11

Line 1: Sentence could be simplified here, e.g., “Landslide inventories typically exhibit heavy-tailed, power law frequency size distributions”

Line 14: “Fitted” should be “Fit”

Page 12

Line 9: the term “a slope gradient units” is confusing.

Line 10: before the word “oversampling”, “and” should be “an”

Line 30: “create” should be “creates”

Line 34: Should there be a figure reference here?

Page 13

Line 7: Should reference Fig. 5d here.

Figure 5: In Figure 5b, you could define the axis value more clearly on the axis itself. “Landslide sampling on steep slopes” does paint a clear image of what the axis value represents.

Page 14

Unit labels are italicized and inconsistent with other unit labels throughout the text.

Page 16

Line 4: remove “S??”

Page 17

Line 3: Could reference Fig 5b specifically.

References

Densmore, A. L. & Hovius, N. Topographic fingerprints of bedrock landslides. Geology 28, 371–374 (2000). Keefer, D. K. The importance of earthquake-induced landslides to long-term slope erosion and slope-failure hazards in seismically active regions. Geomorphology 10, 265–284 (1994). Legros, F. The mobility of long-runout landslides. Eng. Geol. 63, 301–331 (2002). Reid, M. E. & Iverson, R. M. Gravity-Driven Groundwater Flow and Slope Failure Potential. Water Resour. Res. 28, 939–950 (1992).

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