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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Shape and dimension estimations of landslide rupture zones via correlations of characteristic parameters" *by* Gisela Domej et al.

Anonymous Referee #2

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1) General comments

In this paper, the authors re-used a dataset published in a previous article to analyse the statistical behaviour of landslide dimension parameters and shape parameters (angles, or dimension parameter ratios). The preliminary study consisted in grouping the landslides as a function on their volume and infer the mean parameters. This new study consists in plotting parameters, or parameter ratios, as a function of individual volumes. The presented results strengthen the observations of Domej et al., 2017 that mean dimension parameters increase with landslide volume and that shape parameters are constant regarding landslide volume. The main implication of those results

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Discussion paper

is the possibility to infer landslide volume, from measurable other parameters such as landslide width.

Generally speaking, I found the article well written. The presented dataset leads to interesting results and implications, especially regarding landslide volume assessment. However, I noticed four points that should be addressed in a revised version of the manuscript:

General comment #1:

The motivations of this new paper and differences with Domej et al., 2017 should be clearer. If the main findings of the 2017 paper are introduced and summed up very clearly, the difference (in term of methods and result implications) with the present article is often vague (“a refinement” line 62, “a more detailed exploration” line 354, “refine and improve” line 361 . . .). The motivations of this study should be clearly stated in the introduction: why those results need to be “refined”? What still not well constrained from the previous article and what tool do you use here to answer it?

General comment #2

Generally, references to previous studies are missing, especially in the discussion (part 3) and conclusion/perspectives (part 4). What do you bring compared to previous work on landslide geometric properties?

General comment #3:

Those results are based on correlations between landslide volume and dimension parameters. But landslide volume is computed itself with dimension parameters L, D and W. Even if proper independent estimations of multiple landslide volumes are not easy to obtain so far, it would be interesting to discuss this method a little bit more.

General comment #4

What about the distinction bedrock/soil landslide? The two types do not correspond to

the same mechanics and display different scaling (Larsen et al., 2010). If you choose to study one of the two types, it should be stated (is this what you mean by "classic" landslides line 129?) If you studied the two types of landslides, how do you interpret/explain the similarity of dimension and shape parameters?

2) Specific comments

Introduction: The introduction partially motivates this study, and does not explain the novelty of this paper and the difference from the previous one. Several ideas should be clearly stated in the introduction: 1) Why do we want to assess landslide dimensions and shape for hazard assessment? For example, the small and frequent, and large but rare landslides do not represent the same danger, and large landslides are especially devastating; that makes the estimation of landslide volume crucial in hazard assessment.

2) Landslide dimensions and shape can be indirect ways of extrapolating landslide volumes, which is a difficult data to obtain from mapping or field measurements.

3) What do we already know about landslide geometric parameters? There is already some literature, for example about relationships between landslide volume and area (e.g., Larsen et al., 2010, Guzetti el al., 2009...). You have to precise that you are not starting from scratch. What new results will your study bring compared to those previous studies?

4) The 2017 study brought some insights on the shape and dimensions that could be useful to infer landslide volumes but the method employed in this previous paper does not allow such extrapolations. Here you use a new statistic method that leads to increase sample size and add more robustness to the study, in order to propose an application to landslide volume estimations, but this should be clearly stated here.

- Lines 43-44: "the expansion to new living endangered environments": do you mean that there are more and more populated areas prone to landsliding? You could also

mention that climate change is expected to increase the frequency and intensity of extreme meteorological events that can trigger numerous landslides.

- Lines 45: “phenomenon”: can you be more specific? What did those studies already try to understand about landslides (size distribution, rupture mechanics, localisation, frequency. . .) ?

- Line 45-58: here you should precise what is measured in those inventories. Is it area, length, volume, other parameters? You should state here that landslide volume is very important to assess landslide hazard, but that this data is also the most difficult to measure.

- Between 58 and 59: a link is missing between those two parts: 1) landslides are an important danger and many inventories exist; 2) to manage landslide danger, you are going to focus on their dimensions and shape. But why is it important to assess landslide dimensions and shape properly and what insights those inventories already brought?

- Line 59: “assessing landslide”: what do you want to assess about landslides? Volume, localisation, frequency. . .?

- Line 64: Is it field measurement, mapping from remote sensing?

- Line 65: Ok, but why landslide shape is so important to assess?

Statistical analyses:

- Line 76-79: How those inventories have been made (remote sensing mapping, field measurements)? Even if it is explained in details in the 2017 paper, a brief recap of the methods would be useful here.

- Line 104 – why is this remarkable? This should be emphasized from the introduction, on the basis of pre-existing literature

- Line 105: what are the consequences of this drawback?

- Line 110: What additional results/strength of the results can bring this new method?

- Line 113: precise in the text how is the volume Vequ calculated (the equation appears only in the figures) and justify this choice

- Line 114-116: this justification should be appearing in the introduction: why are we interested in linking landslide volume to other measurable parameters?

- Line 129: what do you mean by “classic” landslide? Do you take into account soil landslide, bedrock landslides, or both mass movement?

- Line 131 and table 2: why choosing to separate rotational, translational and roto-translational landslides? Adding a definition of those three landslide types would be also useful.

- Line 137 : precise that you exclude the following parameters from the analyse because they contain not enough samples

- Line 142-147 (and 233-234): Ok but this also applies for other parameters such as L, for example, if the deposit overlaps the scar. Why excluding specifically landslide area?

- Line 148 – 150: not clear - do you mean that your results could help retrieving the area of a landslide from other measured parameters?

- Line 229: what do you mean by “more sophisticated filtering”?

- Line 264: “extremely wide but short rupture zones”: what do you mean by “short”: small length? And if those rupture are extremely wide, the ratio d/w should be small, and not a highest point in the diagram, no?

- Line 269 – 271: To strengthen it, it could be useful to also show in the tables the median values.

Discussion of results of all six sets

- Line 284: why this is of interest? Do you expect from previous literature that earthquake-triggered landslides, for example, would have different proportions?

- Lines 295-297: The comparison is not obvious. How do you compare all regressions? Is it based on the whole correlation, or only on the grey area in figure 5?

- Line 297: Generally, landslide rupture zones are much higher than deep: references?

- Lines 300-301: I do not see this difference (cf comments on figure 5). Moreover, the figure 4 shows that most of the data have a ratio wav5/L <1, so are longer than wide. How to explain such difference with your statement?

- Lines 306 – 311: What are the implications/interest of this finding (the maximum overlap)? Also, you could make it quantitative (for example, define a minimum distance between the logs of the regression?).

- Lines 335-336: Why is the match between the mean ratios obtained in the 2017 paper and in this study “surprisingly” good?

Conclusion and perspectives

- Line 354-359: As said previously, you could be more precise (“a more detailed exploration”, “refine”, “improve”).

- Line 367: “much greater details”: be more specific – why does it allows a broader spectrum of use?

- One other important implication that could be mentioned here is the size distribution of landslides, as it depends on rupture propagation (Stark and Guzzetti, 2009) and then on the 3D landslide geometry (Jeandet et al., 2019).

- The results (based on the use of the equation of Cruden & Varnes) could be discussed here, as I guess this equation is less valid for landslides that are not approximatively half-ellipsoid (such as those sliding on a planar surface).

3) Technical corrections

Figure 1)

- How are defined points 1, 2 and 3 (axes for the transversal cross section?) It seems that it is the four fourths of the projected length at horizontal, but this should be precised in the legend or with a symbol in the picture

- Precise also how is chosen the axe for the longitudinal cross-section?

- Why the depths d1, d2, d3. . . are vertical whereas the maximum depth D is orthogonal to the slope?

- How can d0 be different from zero?

Figure 2)

- I like this figure; it makes the differences between the two methods very clear. However, in figure 2a), it would be useful to plot also an example of ratio as a function of volume group (W/L) in purple as in figure 2b). In the present figure, computing ratios seems to be a new result of this study, but what is new it to plot them as a function of individual volume (the paper of 2017 already shows ratios of dimension parameters).

Figure 5)

- The comparison between the different diagrams is difficult to make visually. First, the step used to label the y axis is too big. You could also add an inset showing the juxtaposition of the four means (dashed lines) to make direct comparison easier.

Figure 6)

- Put the legend also in the figure, not only in the caption

- Precise in the caption that this is not plotted as a function on volume (it looks like those values are constant with the volume, but sometimes they differ significantly from one volume group to another, for example wav5/L).

Main text:

- Line 51: cut the sentence in two parts "... this publication. Some examples are..."

- Line 131-133 - I find this sentence a bit cumbersome – you could rephrase it more simply

- In figure 3, label the y axis more precisely (less space between two labels)

- Generally, figures should be more called in the main text. In figures 3, 4 and 5, the sub-figures should be labelled with a, b, c, and d. For example line 213: Fig 3b); line 261: Fig 4d)...

References:

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Stark, C. P., & Guzzetti, F. (2009). Landslide rupture and the probability distribution of mobilized debris volumes. Journal of Geophysical Research: Earth Surface, 114(F2).

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