Reviewer1:

This is an exciting manuscript that tackles the difficult challenge of calculating landslide rupture depths at scale without prior knowledge of landslide geometries using a new morphometric model. The model calculates the relative stability for each pixel within a DEM and identifies a "daylight" point, indicative of the most unstable downslope rupture location. The model then clusters unstable pixels and considered neighbouring pixels with a shared "daylight" point to be a single landslide. The geometric properties (e.g., area, volume, depth) of the modelled landslides are then compared to traditional scaling laws and a case study for Taiwan. The authors find a more realistic fit between modelled and observed scaling relationships when amalgamating several of the individual landslides. The authors argue that this amalgamation is inherent in the manually mapped landslide inventories used for comparison. Finally, the authors argue that the modelled rupture depths can be used as an additional metric to identify landscape transience, with deeper ruptures corresponding to high channel steepness and the asymmetry of migrating divides.

I think this is a compelling and well-written manuscript that presents an exciting advance in our ability to anticipate landslide rupture depth at scale, with clear applications for landscape evolution and hazard. I would recommend that the authors clarify some aspects of their approach prior to publication, since the underlying model and calculations of Hmin, area, and depth are important to assess the comparisons between the modelled output to observed relationships. I also think it would be useful to see an overlay of the landslides triggered in Typhoon Morakot on top of the landslide geometries mapped in Fig. 5 to qualitatively assess the mapped landslide geometries. I appreciate that this is not the primary focus of the model but think it would help to support the approach since most landslide shapes do appear reasonable. Finally, I think an additional figure alongside Fig. 10 could better demonstrate the benefit of this approach for landscape transience. With some clarifications, this manuscript will certainly be of interest to the ESurf community.

We are very grateful to Reviewer1 for his work on our manuscript and for his thoughtful comments that have helped make our manuscript better. The manuscript has been significantly changed to account for these comments and those of the other reviewer. We below provide answers (in blue) to his comments (in black).

General clarifications

- I think it would be helpful to include a series of definitions early in the manuscript. For example, I found the use of *daylight* throughout the manuscript a bit confusing. Similarly, I think it would be good to specify how *geometry* is used throughout the manuscript, is this the area of the rupture or the depth? I think it may be used interchangeably.

We now define in the first paragraph of the introduction what geometry refers to: “We define the geometry of landslide surface rupture geometry as the 3D shape, extent and depth of this surface, characterized by geometrical metrics (e.g., landslide area, depth, volume, length-to width ratio). These geometrical metrics 1) generally follow some characteristic size distributions, and 2) are often related by some specific scaling relationships.”

We also define in the first paragraph of the Methods what daylight means (as this is mostly useful to understand the Methods).

- If the model extends the approach of Jeandet et al. (2019) does it also apply explicitly to bedrock landslides? This is not mentioned in the manuscript at the moment but seems important for the interpretation of Fig. 6.

The paper of Jeandet is not strictly applied to bedrock landslides and also offers a perspective on shallow landslides. The main difference is the initial strength (in particular cohesion), before destabilization, of the material considered. The current model does not differentiate between the two material types (even though it could be implemented theoretically) between soil and bedrock. We now mention this in the last paragraph of the introduction, and in the discussion (section 4.1 Model Limitations).

- The primary objectives of the model and how the approach builds on previous clustering approaches could be set out more clearly in lines 95 to 100.

We have significantly modified this part to make it clearer: 1) what are the limitations of previous approaches, 2) what are the objectives of this new approach, and 3) how it tackles previous limitations.

- From lines 155 to 170, I infer that the algorithm is run across all points in the DEM and that for all these points the algorithm searches for the optimal rupture depth based on the downstream locations. If there are no downstream locations that daylight to produce a Fmin < 1, the point is considered stable and not mapped as a rupture location. Therefore, must all points between the furthest upstream rupture point (R) and the daylight point (D) also fail (e.g., Fmin <1) for the points to be considered a landslide (e.g., j, k and l in Fig. 1)? Is this mathematically inevitable based on the approach that has been taken?

Good point. The algorithm does not require that all points (even stable ones), between the furthest upstream rupture point (R) and the daylight point (D), fail. Yet, we observe this (unforced) spatial continuity of rupture points associated with the same D point. This is one of the interests of this approach, as the structure of the topography allows this behavior to emerge, and not imposed geometrical rules. I have no mathematical arguments to demonstrate why this is the case, even if this likely induced by the distribution of slopes

along hillslopes: small scale patches of unstable slopes will lead to small shallow landslides, while large unstable chunks will lead to deeper instability which in turn affects a large scale.

- Is landslide volume calculated by summing the Hmin of each clustered landslide? Is depth the mean depth or maximum depth and, considering the variability in the schematic (Fig. 2), does this impact the relationships in Fig. 6.

This is now described accurately in the new section 2.5 of the Methods: "Landslide area A is determined as the total area covered by the pixels associated with rupture points (i.e., the daylight point is not accounted for). Landslide depth D is computed as the mean value of h_{min} over the same unstable pixels. Landslide volume is then simply $V = AD$."

- Can the authors clarify how the model can produce a minimum landslide area of 900 m^2, which is the area of one cell, when the landslides consist of at least a rupture and daylight point?

The daylight point D does not belong to the landslide. Because this was not explained in the manuscript, we now clarify this on Figure 2 and at the end of section 2.4: "The daylight point D is excluded from the landslide list of pixels because 1) it does not contribute to landslide volume, with a depth of zero, and 2) it prevents including pixels belonging to valleys."

- How long does the current model take to run?

We have added a small section (section 3.6) at the end of the Results section to detail model computational speed.

Comparison with manual landslide inventory

I appreciate that this approach intends to identify all/most potential failures by reducing the cohesion and therefore does not represent a specific event. However, showing the manually mapped inventory from Typhoon Morakot used to compare the scaling relationships would be very helpful to (1) visualise the importance of amalgamation – the light green landslide in 5b2 looks like a deep-seated landslide that would remain persistent for several years if triggered, it would be interesting to see if it was; (2) demonstrate the proportion of the landscape that did fail in a recent event relative to the proportion that could fail; and (3) explore how the spatial distribution of landslides triggered compares particularly when thinking about landscape transience.

The landslides simulated by the approach we have designed would not compare well to the landslides triggered by Morakot. Indeed, as investigated during the PhD of Lucas Pelascini (second author of this manuscript), the location of landslides triggered during Morakot are tightly linked to locations where water table saturation and increase in pore pressure

occurred (see pages 82-98 of his PhD manuscript https://ged.univ-rennes1.fr/nuxeo/site/esupversions/56384d66-eddd-4132-b035-3216e653377c?inline). Therefore, the current scaLr model cannot predict faithfully these landslides, without integrating water table dynamics and pore pressure variations during Morakot typhoon. We therefore consider that answering this request is beyond the scope of this manuscript. Similarly, tackling an earthquake driven landslide catalog would also look to some inconsistencies, due to the clustering of landslides near the crests and where topographic amplification occurs (Meunier et al. 2008).

We added a sentence in discussion to state that a direct comparison with catalogs of natural landslides was deemed unsuitable (In the first paragraph of 4.1 Model limitations) due to the lack of a direct formalism integrating seismic wave acceleration of pore pressure variation.

Links to landscape transience

I think the arguments presented by the authors seem reasonable, however I wonder if there is a clearer way to present the complex relationships in Fig. 10 to provide support for the strong conclusion in the abstract (lines 25 to 26).

A simple adjustment to Fig. 10 would be to add labels to highlight key features, such as the central belt of high channel steepness and the higher values of hmin.

Could the authors calculate the average or total depth of material hmin "draining" into each channel segment and plot the longitudinal profile or a scatter plot for the relationship between hmin and steepness. The asymmetric relationship χ is more challenging to represent but perhaps a ratio could be used to show the large differences either side of an actively migrating divide. The relationship for χ would also benefit from annotations on Fig. 10.

In Fig. 10, a lot of the landscape appears to have an assigned hmin – what proportion of the landscape does the model expect to fail? This could be added as another panel to Fig. 9.

We have largely modified this part of the discussion to account for these comments. This has resulted in separating section 4.4 and Figure 10 in two sections and two figures: one dedicated to the relationship between hmin and ksn along river steepened reaches; another one is focused on hmin and chi along divides. These two figures integrate new panels on quantitative tests on these relationships, which are also discussed in the main text. Overall, these new additions bring stronger quantitative support to the previous qualitative claim. Please note that we have not annotated the maps, despite the suggestion made by the reviewer.

Minor Comments

Unsure about the term emergence in the title.

Indeed, I have removed it from the title, which make it in turn a bit shorter “A physics-based morphometric model to predict landslide rupture geometry and identify hillslope transience”. I have also changed “explain” by “predict”, as the model results do not fully explain the observations (like the exponent of the power-law decay of pdf(A)). Overall, this makes the title more direct and more in line with the paper. We have also removed the word emergence from our paper.

Introduction

Lines 34-35: This sentence could be clearer - how are smaller landslides are affected by power-law scaling?

Rephrased

Line 38: what is meant by geometrical scaling here?

Rephrased

Lines 42 – 43: This comment feels important for framing the manuscript but could be clearer.

Rephrased

Line 48: “From these models it has been suggested…”? I would expect each of the numerical points to be referenced.

Done

Lines 74: define what is meant by geometry (e.g., area)

Geometry here is the correct word, as we refer to the geometry of the rupture surface and not to one of its attributes (e.g., area, length)

Line 75: expand on how it is not prescribed by modelling assumptions.

Done

line 80: new model **is** established

Corrected

line 80: space between show and the

Corrected

lines 80 to 82: sentence could be clearer.

I have tried to make it a bit clearer

Methods

The derivation of equations (1) to (6) and those in Appendix A seem reasonable to me, though hopefully another reviewer can critique the approach here more thoroughly.

We have checked the equations of the paper and have found a mistake in the equation of Appendix A, which has been corrected. Due to this correction, no analytical solution can be found to solve for the optimal depth when considering pore pressure under hydrostatic conditions.

I have primarily commented on where descriptions of the approach could be clearer.

Line 95: It would be helpful here to summarise the approach of Alvioli et al. 2014 and explicitly state the objectives of this approach.

We have modified this part of the manuscript to make it clearer and more detailed. We now state the limitations of previous approaches, and how we tackle them.

Line 98: It would be helpful to clarify use of the term “daylight”

Done

Lines 120 to 125: It may be helpful to explain that every point in a DEM is considered to be a possible rupture point earlier in the text – if this is the case.

We believe this is already the case (beginning of section 2) where we state “It extends previous modelling approaches by 1) automatically determining the most unstable depth (i.e., the optimal depth) for each point of a DEM”

Line 129 to 130: I found this sentence to be confusing.

Rephrased

Figure 1 – I really like this schematic. Apologies if this is a misunderstanding, but should rupture lines then also be drawn for points j, k and l to point m to produce a landslide?

Indeed, this is a misunderstanding, as only point i is assessed here in terms of stability and depth. We have clarified the caption.

Figure 2: This figure is very effective. Would these two landslides be combined based on the amalgamation approach here? If so, it would be interesting to add this to the caption. Is

there a specific reason why only one rupture point is shown in panel A? Another interesting panel would be the Fmin scores for each cell.

We now state the two landslides would be considered amalgamated. Panel A aims at enabling an easy understanding of the transition from one rupture point (panel A) to several rupture (other panels). As we focus on landslide identification and geometry, adding an additional panel of Fmin would make this figure heavier.

Line 173: “emergence of landslide” a bit confusing.

Rephrased

Line 175: circle

Done

Section 2.4 to 2.5: It would be helpful to add an additional section that clarifies how landslide area, volume and depth are calculated (e.g., is depth maximum or mean Hmin?, is the daylight point (900 m^2) considered in the area calculation?) since these are important to understand the scaling relationships proposed.

A new section (number 2.5) called “Landslide geometrical properties and landslide amalgamation” now explain how these different metrics are computed

Line 199: “To test the model abilities, we apply it to a DEM…”

Changed

Results

Figure 4: For the Fmin values over 1, does this mean a landslide will not happen within certain parts of the hillslope at this cohesion (100 kPa) or friction angle (40 degrees)? And that the cells would not be mapped as a landslide? I think this is covered in the text (such as lines 221) but wonder if it could be made clearer in the figure too – a dotted or dashed line for non-landslides for example?

We have added in the caption of that figure that even stable areas are shown with the line of optimal depth

Line 236: I appreciate that you are not modelling an individual event, but could you compare these landslide areal densities of 9% to the expected densities from different events? They seem high – which is not a reflection of performance necessarily but would highlight how the model looks for all potential unstable slopes.

We now discuss the resulting landslide spatial density in section 4.2 and compare it to the compilation made by Marc et al. (2018) about rainfall-triggered landslides inventories, and with the Wenchuan earthquake (Xu et al., 2014).

Figure 5: Add to the caption that the numbers correspond to Fig. 3

Done

Line 255 to 257: I'm a bit confused by this sentence; would it be possible to explain how the amalgamation affects the volume? This step may be worth clarifying in the methods.

Done (see new section 2.5). Two landslides that are amalgamated see the new amalgamated landslide having for area or volume the sum of the areas or volumes, respectively, of the initial landslides.

Line 299: What is considered a large landslide here?

This was implicitly defined by opposition to small landslides. We now state that this corresponds to where the power-law is observed.

Figure 8: I wondered if the pdf units should be m^{-1} and m^{-2} respectively? If it's not too messy, it could be interesting to add the underlying landslide area data to the plots as a histogram showing count for example on a secondary axis to see how the populations compare.

Indeed, well spotted. The units of the pdf were checked and corrected on Figure 7 and 8.

Discussion

Figure 9: Add more points to the colour bar of 9C.

Line 317: double check sentence, maybe **since** instead of **if**?

Corrected

Section 4.1:

The assumptions and explanations of these seem reasonable to me but hopefully another reviewer can provide greater feedback.

OK

I found the use of first, second, third to separate points twice in the same paragraph a bit confusing. Could these be separate paragraphs?

Done

I think a simple overlay of the landslides triggered in Typhoon Morakot from the author's paper in 2020 would be insightful and provide a first-order assessment for the limitation stated in lines 341 to 343.

See our answer to this comment in the Main comments.

Section 4.2

Additional points that it may be beneficial to discuss are

- How do you infer a realistic number of landslides with this approach when there is no trigger?

There is a trigger as we force apparent cohesion to be low. But the question of the number or the spatial density of landslides is important. We now discuss this point and compare our values with published ones from natural inventories in section 4.2.

- The average area of landslides triggered based on the different parameters.

Landslide average area is considered a relatively ill-defined metric due to the power-law nature of the landslide area distribution.

- The large scatter observed in Figure 6, with smaller landslides above the expected relationship for bedrock landslides. Does this tell us anything about the failure mechanisms that this model can best predict?

We are quite unsure about this particular point, as the modelled scatter is similar to the observed ones (see for instance in Larsen et al., 2010)

Section 4.4

I wonder if the authors can find a way to display these complex relationships more clearly, see above comments. This could include a series of longitudinal profiles with the asymmetry between hmin on the aggressor and victim hillslopes shown?

We have made some statistical tests to better illustrate this part (Figure 10 an 11 and section 4.4 and 4.5 that replace section 4.4), as already answered.

The relationships described here also rely on accurate lateral landslide predictions and so I think showing the manual inventory from a previous event will be helpful.

Could you overlay results/show the area with the landslides mapped by Chen et al. 2023?

As already answered, we are not convinced this is appropriate to compare the location of the two inventories (model vs data), due to the model excessive simplicity respective to the complexity of where rainfall-triggered landslides occur.

It is great that the authors have made the code available.

Thanks

Reviewer2:

General comments

Steer et al. presents a new method of landslide modeling by modifying a Mohr-Coulomb approach to predict the most unstable landslide clusters from digital topography, not only in plan view, but also in depth. The model named "scaLr" iteratively tests different failure geometries and depths to find the most unstable configuration. Adjacent modeled landslides can be amalgamated to simulate temporal limitations of landslide inventory mapping (i.e., human mapping of small, adjacent, and sequential landslides into one large slide). By artificially applying a low material cohesion, the authors simulate landslides in Central Taiwan and capture the bulk behavior of unstable topography after a destabilizing event such as a typhoon. Interestingly, this model could reproduce landslide area-volume and area-depth scaling constants relative to observed landslides. Where a decrease in observed small landslides was not reproduced by the model suggest the observed inventory amalgamated small landslides into large ones. Moreover, the authors make a correlation between thicker modeled landslide depths and more transient topography derived from the stream power law. This manuscript takes a complex problem and presents results and implications in an organized manner. In my opinion, the work here represents a significant contribution to landslide hazards and topographic evolution and should be published after minor to moderate revisions.

We are very grateful to Reviewer1 for his work on our manuscript, for his positive evaluation, and for his useful comments. The manuscript has been significantly changed to account for these comments and those of the other reviewer. We below provide answers (in blue) to his comments (in black).

Specific comments

I have two scientific points I would like to see addressed in the revision of this work.

1) I think that the differences between soil and bedrock landslides could be more explicitly discussed (e.g., in the rollover and cohesion differences, mixed soil+bedrock vs soil only landslide inventories, model ability to include layered heterogeneity). A reader unfamiliar with these concepts may not be able to pick up the importance of layered bedrock, weathered material, and soil in landslide scaling. This next suggestion may be beyond the scope of this study, but I wonder how adding a soil layer (either constant or variable thickness) would affect results (produce more small landslides that get amalgamated by

the model?). The background provided by the authors implies a test of this would support their hypothesis that rollover (at least for Central Taiwan), is caused in part by amalgamation of smaller landslides, smaller landslides that may become more abundant if modeled with a soil layer. The authors state that "the developed model is fully compatible with heterogeneous cohesion or friction values," so I wonder if this could be a doable sensitivity test to include (vary soil thickness, test constant vs variable soil thickness; e.g., Bellugi et al., 2015; Moon et al., 2026). Regardless of what the authors decide to do, I think a more explicit discussion of soil, bedrock, and other weathered layers for modeling landslides in the methods and/or discussion sections would better situate this work in the literature (e.g., why or why not used, model simplifications). For example, Section 4.2 may include a passage about why some exponents and coefficients fall within the range of established values despite not including weathered layers (Because real-world soil is thin in Central Taiwan and does not affect bulk properties? Does low cohesion and friction angle push the material to behave more like weathered material as a secondary effect of bulk simulating a typhoon-affected landscape?)

To partly answer this comment, we now 1) acknowledge explicitly these limitations in the Method section (2.1), and 2) discuss them more thoroughly in the Discussion section about limitations (4.1), in particular the positioning of our model respectively to the issue of soil or bedrock landslides.

2) I think the connection to landscape evolution is also very important. I was wondering if there is enough of a climate gradient that precipitation-adjusted ksn or χ (e.g., Adams et al., 2020; Gelwick et al., 2024; Yang et al., 2015) may address how, as the authors state, the "…consistency between these two indices is not systematic, and a more thorough investigation is needed."

There are definitely some climatic gradients at the Taiwan scale, but an east-west contrast (which could explain the chi-anomaly along the main divide) does not seem to be a dominant feature of the annual rainfall. Please note that I have not found a reference annual rainfall map over Taiwan. I have explored different rainfall sources

(1) Mean annual precipitation (mm yr^{-1}) over 1980–2019 from the TCCIP TReAD v1 dataset (2 km dynamical downscaling of ERA5 with WRF), smoothed with a Gaussian filter (σ = 6 km) to highlight the regional pattern

(2) Mean annual precipitation (mm yr^{-1}) from the WorldClim 2.1 climatology (1970–2000, 30 arc-second resolution; Fick and Hijmans, 2017)

The resulting rainfall maps show some spatial mismatches, in particular over the central range. Moreover, the question of integration through "deep-time" to account for the climate

of the past (relevant for landscape building) represents another clear limitation. I am therefore doubtful the analysis in our paper could go much further in the direction of including rainfall into the steepness or chi indexes, despite being clearly relevant additions, at least theoretically. We have rather included a sentence at the very end of the discussion to mention these ideas as a potential future work: "Future work could focus on integrating rainfall patterns into the χ and k_sn analysis, to decipher potential climatic controls on these identified transient landscape features (Adams et al., 2020; Gelwick et al., 2024)."

Related to this point, I think the manuscript is missing a background of regional climate, spatially variable climate, orographic effect, etc. in the section where the study site is introduced (along that line, also if there is an uplift gradient across the site).

We have expanded section 2.5 to describe the tectonic setting of the study, its geology and climate.

Next, I do believe that a quantitative approach would greatly improve this discussion and may remain a simple analysis to keep as a discussion point instead of as a main result. It would be great if the authors could make a scatter plot of ksn or χ against some metric of nearby hmin (mean, median within a search radius perhaps equal to mean hillslope length?). Just as the authors do for flow paths, it may be important to calculate mean/median hmin restricted to the basin they drain (i.e., make sure the mean/median is not using values on the opposite side of a divide, like catchment restricted relief from Winterberg and Willett, 2019).

As answered to the first reviewer, we have largely modified this part of the discussion to account for these comments and include a quantitative analysis of these metrics and their inter-dependency. This has resulted in separating section 4.4 and Figure 10 in two sections and two figures: one dedicated to the relationship between hmin and ksn along river steepened reaches; another one is focused on hmin and chi along divides. These two figures integrate new panels on quantitative tests on these relationships, which are also discussed in the main text. Overall, these new additions bring stronger quantitative support to the previous qualitative claim.

Technical comments

Line 2: Just a readability point, I don't think the second "to" is necessary in the title and could be removed.

Indeed, corrected

Line 7: Should be “landslides tend.”

Done

Line 13: Maybe insert the word “landslide” as in “This landslide geometry defines…”

Not done

Line 20: Suggest inserting the word “adjacent” as in “Amalgamating adjacent, modelled landslides…” just to be specific that landslides next to each other are being combined.

Done

Line 35: Slightly difficult to parse this sentence. Should it say “…even if the latter…”?

We have fully rephrased this sentence to make it clearer

Line 38 – 43: I think this passage states the problem very clearly.

Thanks

Line 48 – 53: What works are suggesting the “topographical and mechanical factors” in this list? Although possibly repetitive, I think this sentence is missing citations. Or, the authors could somehow indicate that this list was produced from the citations in the previous sentence (e.g., adding “These works suggest that the following topographical…”). I also think a reader unfamiliar with “rollover” would appreciate a definition of this term somewhere in this sentence or before it.
We have rephrased this part and included appropriate references. Rollover was already defined in the previous paragraph.

Line 64 – 66: This is sentence was hard to parse and may benefit from splitting into two sentences by the parenthetical. The correct phrase may also be “Jeandet et al. (2019) ‘and’ Medweff et al. (2020),” not ‘or’ if both citations claim what the authors are stating here.

Indeed. We have split the sentence in two and replaced the “or” by “and”

Line 73: Missing period.

Corrected

Line 80: “Once this model ‘is’ established…”. Also missing space between “showthe.”

Corrected

Line 95 – 96: Maybe put the definition of “scaLr” in quotation marks to indicate it better in the sentence, as “slope curvature analysis for Landslide rupture.”

Done

Line 104: As the other reviewer stated, the equations 1 – 6 and those in the appendix seem reasonable (or are acknowledged as unreasonable where it seems overly complex).

We have checked the equations of the paper and have found a mistake in the equation of Appendix A, which has been corrected. Due to this correction, no analytical solution can be found to solve for the optimal depth when considering pore pressure under hydrostatic conditions.

Line 110: I think this would be a good place to address my first point listed under “Specific Comments”. The authors discuss how destabilization is bulk considered by reducing cohesion. I believe that not including a soil or weathered bedrock layer and not distinguishing soil or bedrock landslides merit some discussion.

We now mention this limitation as a model choice and state that we discuss it in the Discussion section.

Line 111: I’m not sure if “pristine” is the correct descriptive word to use here. Maybe “unaffected”? “Unreduced”? “Maximum”? Same for line 321.

Corrected

Line 123: Should be an en-dash “Δx from – and a…”

Corrected

Line 123 – 125: Here, maybe a parenthetical can help readability and split up the sentence “…the tangent of the rupture angle tan θ can also be expressed as the ratio between Δz – h (the difference of elevation between the tip and the top of the rupture plane) and Δx (the horizontal distance), leading to…”. Also, using the word “tip” by itself seems un-intuitive to describing this bottom point. Consider using “bottom tip.”

Done

Line 126 – 130: This is another hard to follow passage that could be simplified for readability.

We have fully rephrased this paragraph to increase its readability.

Line 137: Replace “with” with “the” as in “…provides the analytical expressions…”

Done

Figure 1: It may be illustrative to indicate θ, Δx, and Δz in the figure because these variables are important for the equations.

Done

Line 163: “Point” should be plural “removing points.”

Done

Figure 2: Misspelling of “circle” in the caption. I also wonder if the second landslide indicated by yellow lines and points should be described in the caption as being cut off.

Done

Line 179 – 183: This sentence is also hard to parse. I think the “with” can be removed in “…does not provide with an identification of…”

We have rephrased this part to make it clearer.

Figure 3: Just some small points. It is kind of difficult to see the true color bar colors if it is transparent, maybe make it opaque. For consistency, consider making the slope pdf opaque and scale bar as well, if that corner of the map is not necessary to be somewhat visible.

Thanks for this comment that we understand, but we believe Figure 3 is sufficiently clear as it is.

Line 199: Consider something simpler like “We apply scaLr to a DEM of the southern part of Taiwan to test model abilities (Fig. 3).”

Done

Line 201: If the elevation of Yushan is important to mention in-text, consider also marking the location in Figure 3.

It is not really important.

Line 203 – 205: As stated in the second "Specific Comments", I think this background could benefit from a more in-depth description of the geology, tectonic gradient (e.g., mapped faults, if any), and climate gradient of the study area.

We have expanded section 2.5 to describe the tectonic setting of the study, its geology and climate.

Figure 4: Consider flipping the color bar so that more unstable slopes show as red. Perhaps also consider making the lines colored by F thicker to see those colors better.

We have tried making lines bigger, but it actually decreased figure readability (due to lines being too close to each other). As the red-blue is only a convention issue, we have not modified it. We have yet added stable or unstable on top of the colorbar to help the reader.

Line 222 – 223: I think this sentence is missing a citation to what the "previous modelling results" are referring.

Added and also explained why this is the case.

Figure 5: The caption describes "3 subsets," but should it just be "2" as stated earlier in that sentence? Also, callback cite to Fig 3 for the location boxes or add northing and easting ticks to indicate location. Consider moving the bracketed subset labels to the top right corner or outside the figure. Less important, if the individual landslides are shown as colors in a) and b), can there be a scale bar to show the range of number of landslides?

Corrected

Line 238 – 239: Hard to parse sentence, particularly around the phrase "…do consist in continuous…"

Rephrased

Figure 6: Hard to parse the list at the end of the caption. Consider moving "respectively" to the end of the sentence. The dot in the legend is hard to see which color goes with initial or

amalgamated. Maybe coloring the text to red or blue will make it easier to see? Same for the legend in Figure 7.

Done, and the figures now used colored text in the legend

Figure 8: Consider adding a vertical line to mark the break-off and a label for the cutoff point, just because these terms are used in the main text. A reader may benefit from this more certain visual clue.

Done

Line 320: I think this would be another good place to address my first point listed under "Specific Comments". To the best of my understanding, not only does this work not consider "potential lithological or structural heterogeneities," but it also does not consider weathered layers nor explicitly distinguish soil or bedrock landslides. I think this limitation is okay, but the authors should discuss it here regarding the effects of groundwater flow, low-cohesion layers, etc.

We now discuss these limitations.

Line 331 – 333: I think you can replace "consists in clustering" with just "clusters" and "way to determine" with "way we determine."

Done

Line 336 – 338: Difficult to parse sentence, consider deleting "probably."

Done

Line 339: The start and end of the two "first, second, third" lists is a little ambiguous to distinguish between. Consider making the distinction between both lists clearer (new paragraph as suggested by the other reviewer?).

Corrected

Line 351 – 374: If I am reading this correctly, I think the in-text citations for Figure 9 are missing here. Also, a citation to Figure 8 could be added to the sentence that ends on Line 372.

Done

Line 356: I think “description” may be the wrong word here. Consider “…keeping a good relation between V and A.”

Done

Line 401: The parenthetical is hard to parse. Consider something like “…(i.e., indicative of oversteepened channels respective to upstream drainage area)…”

Corrected

Figure 10: Panels a) and b) may benefit from a UTM grid/ticks and/or a north arrow.

Not done, as the location of the studied area is clearly defined on Figure 3 (in UTM coordinates) and as the classical mapping convention wants that the north is indicated upward on a Figure if no north arrow is represented.

Line 431: Consider replacing “aggressor” and “victim” with language that more directly indicates drainage area gain or loss.

We now define what victim or aggressor means in terms of area gain/lost

Line 466: Consider “This work opens…” or something to that affect.

Done. We have also added two sentences in the conclusion on the link between hmin and ksn or chi.

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