Reviewer #3 comment (29-Oct-2025)	**Author response**
General: Stepping late and as an additional referee into this review process I think the authors did reply well to some of the original reviewer concerns, and the quality of the manuscript improved accordingly. However, I made some additional observations and think that this manuscript needs further revisions to become up to standards for possible publication:	Many thanks for your willingness to jump into the middle of a review process, your insights are much appreciated!
Section 3.6: If I correctly understand, the authors measured 405 joint orientations and then tested each DEM pixel with each of this data for topple failure to express the terrain pixel susceptibility as the percentage from the 405 orientations fulfilling the kinematic criteria at this location? I am sorry, but I think this approach is inadmissible because you cannot use a failure analysis on individual joints (from which you suppose they are present in exactly the same orientation and	• We've considered this helpful review comment very carefully and thus were delayed in revising and resubmitting our manuscript. Thanks for the opportunity to dive more deeply into the methodology. • Field-based characterization of bedrock joints has a long history in structural geology, and remains useful in field settings where vegetation, steep terrain, and other obstacles prohibit extensive collection of SfM or lidar data. We employed a modification of the standard "scanline" method, where we measured joints along a paced transect to capture a representative sample of joint orientations. (Traditionally this would be accomplished by measuring every joint on a tape transect across an outcrop, but Ian scaled this up to collect data across a wide area in rough and vegetated terrain.) Recorder bias is always present in field measurements (the reviewer correctly notes "there are always more joints to sample"), but it is effective for efficiently capturing a sample of structural data that approximates the pervading structural fabric. In this way, our sample set of 405 joints provides a representative distribution of joints in the Skagway valley. Kinematic analysis of each of these joints is NOT based on an assumption that all joint orientations occur in each pixel, but that the sample of 405 measurements characterizes the overall population of joint orientations in the study area. This is a well-established practice for probabilistic kinematic analysis. This does rely on the assumption that our sample is representative of the structural fabric of the study area (see the note below on sample site selection). While it is possible that our field observations are an imperfect sample, such is true of any structural observation sample, whether it's measured in the field or from topographic expression. For the purposes of assessing relative susceptibility, we are confident that our sample approximates the overall population of joint orientations closely enough to provide useful estimates. ○ Conference paper describing the scanline technique: Https://link.springer.com/chapter/10.1007/978-3-319-09060-3_61

number all over the area) for spatially distributed analysis. I would highly recommend to calculate (or at least estimate from the density plot) mean orientations of the two orthogonal joint sets together with a confidential cone (can be done with all stereonet softwares) and then do the testing with the DEM data using the mean orientations and their variance defined by the confidential cone. However, this would only be a global analysis neglecting that mean joint orientations (and sometimes also the number and presence of joint sets!) often vary throughout a study area. For a valid spatial analysis, a sufficient number of orientations of any joint set present at a particular location should be measured and mean orientations should be calculated at each sampling location. The spatial distribution of the mean orientations can then be used for kinematic testing. In this context, I wonder why the authors did not test for wedge toppling	○ Book with a short section about scanline methodology: https://www.sciencedirect.com/science/chapter/edited-volume/abs/pii/B9780120839803500085?via=ihub ○ We could state our assumption that our sample is representative of joint orientations throughout the Skagway valley. Local variability in joint distribution may impact rockfall susceptibility, but we are confident that our dataset provides a useful approximation of the rock discontinuities in our study area, and is appropriate for use in a relative susceptibility index. Particularly in applications of public awareness and resource planning, this is an appropriate approximation. • In this sense our approach provides exactly what the reviewer is looking for—identification of potential failures along the most common joint orientations—while also approximating a small probability for potential failures on joint orientations at the tail ends of the observed distribution. As the reviewer suggests, kinematic analysis of average joint orientation can be done (commonly referred to as deterministic kinematic analysis), and it may be possible to calculate a confidence interval based on distribution parameters. However, we find that our approach provides a more refined result, as it estimates a probabilistic susceptibility based on the overall distribution of joint orientations observed in the study area, rather than condensing this distribution down to a single average value. This is strongly supported in the literature, as multiple studies conclude that using mean joint set orientations have the potential to miss many potential failure geometries (see examples below). We also prefer our approach for its simplicity and ease of communication about relative rockfall susceptibility to a non-technical audience. • The approach of using a distribution of joint orientations for kinematic analysis using topographic data is outlined in this 2023 paper by Kundu et al, which is already cited in the manuscript. Below are several other examples cited by Kundu et al that used a Bayesian or probabilistic approach to kinematic analysis with some sample or distribution of joint orientations. According to these sources that use Bayesian and Monte Carlo tools, we think the general approach we use holds up. ○ Xin Zhou et al 2016 -- they measured a bunch of discontinuities from a handful of sample pits and then used all possible pairs of joint orientations for very detailed kinematic analysis to characterize potential failures. Importantly, they find that their probabilistic approach identifies many potential failure geometries that would **not** be identified if they just used mean values of joint sets. ○ Admassu and Shakoor 2013 --This is software that can be fed lots of joint orientations for kinematic analysis. It does set a precedent that large samples of joint orientations are appropriate input for automated kinematic analysis.

(as described in Günther et al. 2012) since it can be supposed that when having steep, orthogonal discontinuities, this mechanism may also be identified for some locations. Last, I think at least some justification of the applied residual friction coefficients (expressed as friction angles) of the joint sets for kinematic testing must be given. This could be done using literature values, or by applying some appropriate field analyses	○ Obregon and Mitri 2019 -- some cool Monte Carlo stuff shows that if you just use a single joint value ("deterministic kinematic analysis") you might not capture susceptibility, because maybe only some joints in your distribution are susceptible…. **4.1.2. Probabilistic kinematic analysis** A closer look at Fig. 2e–g reveals that if a conventional (deterministic) kinematic analyses were to be performed, no potential failure would be reported. This is simply because all mean orientations of the joint sets lie outside their corresponding critical zones. Nevertheless, a careful observation of the density contours shows that in effect part of the joint orientation distribution lies within this zone (shaded in grey in Fig. 2e–g). This means that there are in fact some combinations of dip and dip direction angles leading to potential failure. A probabilistic kinematic approach comes in handy in these cases where the joint orientation distribution partially lies within the critical unstable zone of the stereoplot. ○ Jiahnua Yan et al 2021 -- helpfully also re-hashes the fact that using a single average value is not appropriate. They compare both a deterministic (mean values) and probabilistic (all discontinuities) approach, and find that the deterministic strategy misses potential failures. ○ Notably, Kundu et al. 2023 recognize that slope stability analysis requires a good dataset of rock discontinuities. They suggest using remotely sensed orientation data, but I think we have already argued that in our veggie-rich and very steep study area, field observations are more representative. Here's a snippet from Kundu et al: for the rapid automated mapping of geometrical and kinematical slope properties [50]. Ghosh et al. (2010) have used this suite for rock slope assessment of rock slopes in a large region of Darjeeling Himalaya, India [51,52]. Unfortunately, RSS-GIS is not sold or supported anymore by ESRI, hence making it difficult for public availability. Ji et al. discussed in detail the possible impacts of degrading yield acceleration in Newmark computing frameworks for seismic slope stability analysis [53]. Regional rock slope stability analysis is difficult without adequate information on the spatial distribution of discontinuity orientations at a higher density. The denser the geostructural information per unit area, the better the analysis results that can be obtained. The spatial availability of joint information is usually poor in the geological record due to the inaccessibility of areas in mountainous regions and the high cost of data collection. To the rescue are several techniques developed to determine discontinuity orientations on a regional scale by utilising remotely sensed images in combination with GIS techniques. A semi-automatic GIS model was proposed in [54] for extraction of the orientation of planar structural elements through spaceborne thematic images and the Digital Terrain Model (DTM). In [25], a method was developed for the detection and quantification of a general trend of bedding planes using stereoscopic aerial photographs and the subsequent use of TOBIA to evaluate the dependence of landslide types on local structural settings. There are instances of assessing the rock slope susceptibility utilising laser scanning or UAV point clouds [55–57]. A few algorithms have recently been developed to perform three-dimensional kinematic analysis on point clouds [58,59]. ○ We also note that using a number of discontinuities that are failure-prone to quantify hazard isn't precisely what Kundu et al recommend to assess hazard (see snippet below, entitled "1.3. Kinematic Susceptibility"), because they argue that one joint is all it takes to make a bad rockfall. However, I think we're using this approach slightly differently, because we're assuming that *if* the bad joint is present, it is capable of triggering rockfall, and we're trying to estimate a relative likelihood of occurrence in different pixels, not the magnitude of rockfall that's possible….

	1.3. Kinematic Susceptibility
	
	It is well known that the maximum principal stress (σ_1) acts nearly parallel to a mountain slope surface [60]. Therefore, a joint plane or intersection line, which shows more parallelism with the slope surface, has a higher chance of failure due to shear release. In other words, a joint or intersection inclined at a higher angle is generally more susceptible to triggering factors, provided it is on the slope's surface. The conventional kinematic criteria alone cannot estimate the intensity/degree of the failure potential; therefore, a different method is needed to quantify the degree of vulnerability of a failure element. The existing quantitative methods are often based on the number of discontinuities, which undergo Markland's test to give results of either zero or one [30,36–38,61]. The susceptibility estimation in these methods is based on the manipulation of the number of zeros or ones obtained out of numerous failure elements. However, these types of quantitative estimation are important as they provide practical solutions to include the effect of variability in discontinuity orientation. In our view, the presence of a few unfavourably positioned discontinuities, if penetrative, can give rise to massive failures. Therefore, the assignment of the susceptibility value should be based on the magnitude of unfavourability of a single element rather than the number of discontinuities that satisfies Markland's test. We have adopted the unfavourability criteria of slope mass rating (SMR) to implement this concept on the mountain slope stability analysis. The adjustment factors of SMR have proven their potential in estimating the probability of unfavourable discontinuity arrangements since its introduction [62]. The rock mass classification system is used worldwide
	
	• **Finally, we note that it's also very common practice to characterize joint sets in sample areas within a study region, based on site access, rock outcrop locations, and efficiency.** This is an incredibly important approach for characterizing variability (or continuity) across a larger region. As already noted in the manuscript, we see similar joint set orientations across the study area, indicating a consistent structural fabric in this relatively local area (it's only a few square kilometers, so regional tectonic constraints are going to be consistent). The reviewer highlights the intensive nature of field-based structural data collection. Other examples where this approach is applied in structural research and hazards analysis:
	  o This project and other structural geologic studies informed some of our discussions and our approach was more methodical. https://pubs.geoscienceworld.org/uwyo/rmg/article-abstract/54/1/1/570836/Laramide-shortening-and-the-influence-of?redirectedFrom=fulltext
	  o This aligns with the approach described here by Arlegui and Simon, 2001, who also cite a book by Davis, 1984. That citation is "Structural Geology of Rocks and Regions," a widely accepted textbook that presents pretty foundational structural methodology. See the screenshot from their methodology below. https://www.sciencedirect.com/science/article/pii/S0191814100000973
	
	Fig. 1. Geological map of the NE Iberian Peninsula showing the location of the study area (Fig. 2).
	
	1.2. Methodology
	
	Not all joints in a structural domain may be examined. Thus, standard practice (Davis, 1984, p. 342) is to evaluate jointing through detailed analysis at selected sites. A total of 290 high-quality outcrops were studied in this work, where orientation and geometrical data about joints were collected. Data sites are located across the central Ebro basin, in

	• The reviewer argues that a susceptibility index is "inadmissible" unless we measure all joints in the study region, because we can't otherwise characterize the structure of a rock mass. This is a daunting task indeed and directly contradicts a large body of structural geology literature where structural fabric is estimated using a sample of measurements. Using a sample of observations to estimate characteristics of a larger population is standard scientific and statistical practice. In our view, this suggests that the reviewer has a particular grasp of structural analysis and susceptibility that contradicts much of the literature. Relative metrics like our susceptibility index are extremely useful tools to evaluate a hazard or risk that cannot be fully constrained. • The susceptibility index that we utilize in the analysis is just that, an index or proxy that's imperfect but a vast improvement on random or purely slope-derived approaches. • In order to address these comments, we've emphasized in the text that our joint dataset is a sample and can not represent the entire joint distribution. In addition, we add information supporting the approach use here.
Section 3.7: I think some illustration of the data used for RAMMS block size parameterization should be given, even though cited. I am not sure if talus fragments are suitable for estimating rockfall trajectories, this must be justified.	We've summarized statistics of the blocks in this section and added a histogram to the supplemental information. In addition, we cited justification for this approach.
Section 4.1: Was the inventory only exploited for rockfall triggering conditions? Couldn´t it be used for parametrization of RAMMS when doing some frequency/size statistics to estimate block sizes? Why is the inventory lacking information along the railway line (except for one location)?	This inventory does provide information on rockfall location but unfortunately the accuracy is insufficient to be used for parameterization. Perhaps more importantly, the inventory is collected along the highway system and our RAMMS simulations are performed above the railroad and township, which is not included in the inventory. The inventory is generated by Alaska department of transport and they do not manage the railroad, which is a private entity. We contacted the railroad on numerous occasions for rockfall information but sadly that information was not forthcoming.
Section 4.3: This is not convincing, as also stressed above. In addition to the above: how did you identify the	As discussed above we are confident that the joint dataset used for our kinematic analysis reflects a representative sample. In particular, the remarkable similarity of the dominant joint orientation across the study area implies that our approach is defensible and that the structural controls on rockfall initiation are consistent across a >20 km^2 area. The three sub plots (b,c,d) do not represent ALL of the joints but instead are intended to show that the orientations in the far extremes of the study area are quite similar.

clusters? Why are some shallow dipping and moderately dipping discontinuities only present in the synoptic diagram? Why did you exclude sample locations from the fabric analyses (as in Fig. 8 some sectors are plotted but many sample locations are outside of those)? What is the rationale behind the "heat map" (as these only shows how many data was measured and you may always measure more)? Please also plot the traces of major faults in your maps since it is often the case that joint spacing reduces and persistence increases in the vicinity of fault zones, enhancing rockfall susceptibility.	To be clear, we do not exclude samples from the fabric analyses as stated above. The analysis in 8e includes all of the data. The heatmap does indicate the number measurements and we did this to show the spatial extent of our sampling campaign that spans much of the study area. And yes, we can always measure more but the remarkable consistency of the data across the study area was compelling to suggest that we acquired a representative sample. Unfortunately, there are no known fault traces in our study area to include on this map. We've attempted to modify the text in order to clarify our rationale for presenting the data in this fashion.
Section 4.4: I think this is inadmissible. You cannot define a susceptibility index by calculating percentages of some measured orientations fulfilling failure criteria. To do so, you have to measure ALL joint orientations (at least at a specific observation scale and for a defined moving window where you assume bulk rock mass homogeneity)at	See above comment for details. The index we use is a measure of relative rockfall susceptibility as a function of local slope geometry and bedrock structure, which is represented by our sample rather than the all of the joint orientations, which is intractable as detailed above. The additional data that the reviewer proposes, such as roughness, persistence, fillings, etc would be an excellent approach for a specific slope but untenable for our entire study area. In this sense, our approach is regional and slope-specific. We've attempted to clarify this key point throughout the manuscript and we appreciate the review comments highlighting the need to clarify this point.

many outcrops, this would result in an enormous workload. Alternatively (and statistically admissible) you may measure a number of orientations of each joint set present at a particular location and compute mean orientations and their scattering using some vector statistics available in common stereonet softwares. Then you can do some kinematic testing. In this context, I wonder why the authors did not collect data on discontinuity persistence, spacing, roughness, fillings etc. This would be the data important for failure susceptibility evaluations.	
Section 4.5.: I think it would be good to have some idea on the influence of the settings of the different RAMMS parameters. Please give some insights into the parameter spaces. Additionally, why are the RAMMS models not even capable for reproducing the only documented rockfall event along the railway line? I think the presentation is missing some (even if only qualitative)	The text includes description of the RAMMS parameters used, many of which are measured directly or based on field observations. The RAMMS models are indeed capable of providing a clear pathway that reflects the observed area of activity in 2022. Those rockfalls seldom reach the docks because of retaining structures that are included in the lidar data and mitigate the vast majority of small rockfalls in that area. We've attempted to clarify this further in the text. Thanks for the helpful insights and observations!

evaluations of the modelled rockfall trajectories.	
To conclude, I think the paper needs additional work and data to become up to standards for publication in NHESS, or at least some major improvements of the applied analyses. However, I am aware that stepping into a running review process as a third referee is problematic, and the original referees have been more optimistic. Consequently, I would leave it up to the editor to decide if this manuscript has the potential for publication in a revised version. Günther, A., Wienhöfer, J., Konietzky, H. (2012): Automated mapping of rock slope geometry, kinematics and stability with RSS-GIS. Natural Hazards, 61:29-49, DOI10.1007/s11069-011-9771-2	The comments are much appreciated and the changes have helped improve the clarity of the text! The reviewer has raised some very helpful comments and we hope the responses and changes to the text provide an improvement that distinguishes our approach with its regional focus with one that's site-specific.

Reviewer #1 comment (13-Sept-2025)	Author response
General: The manuscript presents an interesting and promising approach for an integrated analysis of rockfall hazards. But it requires revisions regarding a more rigorous coupling of the kinematic model with the runout model as well as a more schematic workflow and consequent structure. The coupling of the models by linking the susceptibilities for failure and runout is a crucial but innovative for hazard assessment and the improved workflow allows to handle the complexity of the approach in an appropriate way.	Many thanks for the comments. We've attempted to clarify the nature of the coupling of the models. We do see this not as a methodological paper, however, and rather a geomorphic-geotechnical one that highlights the importance of hillslope morphology for determining the spatial pattern of rockfall activity.
Figure 6: This figure adds great value to the analysis of the rockfall inventory. But the number of rockfall events is over a period of 18 years while all other information are referred to one year. Consequently, the rockfall activity in terms of number of events per year should be shown.	This is a helpful comment. Done!
Figure 8: This figure is now more clear since the rainbow colors from the DEM are removed. However, the heat map illustration for the joint measurements makes it less clear. The measurement points were more clear and if a heat map is shown, it should have a legend for the values.	The points were problematic because they fail to distinguish relative density of samples. We prefer the heat map and values are now shown as per the suggestion.
Figure 10: This figure improved regarding the mapping of the runout and with the assignment of the locations. But it is now showing only two of four runout scenarios. The heat map should be removed because it covers the runout and all runout scenarios as well as the roughness should be shown.	We've added the roughness map as suggested. Otherwise, we're not including the additional runout maps. The two maps shown bracket the end-member scenarios and the additional maps were distracting to our community partners and didn't add additional key insights. We've opted to keep the two end-member scenarios for clarity because they can be combined with other useful maps in Figure 10. The heat maps are retained because they only contain runout for the two relevant scenarios and not all of the scenarios as suggested by the review comment. We've clarified the text accordingly.
Line 67: The term „relative likelihood" is correctly introduced in this sentence. But it is not linked to the term „susceptibility", which is crucial in the study. This sentence would offer an opportunity to introduce „susceptibility" and define it as „relative likelihood"	Good idea! We've added that definition as a parenthetical
Line 100: Indeed runout is often simulated and there are gaps in quantifying why and where rockfall occurs. But these gaps are rather related to the susceptivity for failure than to the susceptibility for runout in terms of propagation. It is therefore important to differentiate between these two aspects of the overall susceptibility and to highlight the need for better quantification of rockfall sources and their connection to rockfall runout.	This is an interesting point. While we agree that source areas affect the potential for propagation, one of the key points of our contribution is that slope morphology plays a key role in propagation beyond source area density. We've made a note to clarify.
Line 273: The identification of joint clusters by contouring is an established an reasonable approach. Nevertheless, the methodological details for a reproduction are missing. It must be described which tools and which parameters are used to separate the joint clusters.	We added the relevant software and methodology.

Line 345: The governing forces considered in RAMMS::Rockfall are now properly mentioned. However, its strength is the complex momentum balance that goes further than a simple energy balance. Even though this includes also an energy balance, its strength of the momentum balance should be highlighted.	We've made this additional suggestion and added text relevant for momentum balance.
Line 351: The results later show that block toppling is way more frequent than planar sliding. Nevertheless, it is not justified in the methods why the source areas for the runout simulations are only based on the block toppling. This should either be already mentioned or being kept general in the methods and justified later in the results.	We have mentioned the focus on block toppling.
Line 397: The cumulative number of rockfall passages is a good start to quantify the susceptibility for runout. But it should be normalized to be comparable to the susceptibility for failure. Therefore, some type of reach probability should be defined, in the best case by incorporating the susceptibility for failure.	In the results section we explain that we've normalized the results by susceptibility. These details are less useful at this point and the explanation is better suited for the results section.
Line 563: The rockfall source density can be an interesting variable to consider in the analysis. But nowhere its exact definition is described. A proper definition should be placed in the methods where also the definition of the roughness is described.	We added the definition, good point!
Line 624: Indeed a more advanced treatment of these factors would improve the accuracy of the simulations. But these factors influence the runout while this paragraph is about the failure and the most crucial issue of how about to apply this method with more scattered joint systems remains open. The paragraph should focus on the kinematic analysis and address the issue of how the method could be applied for more scattered joint systems for example with interpolating joint orientations from measurement points.	This is a helpful comment and we've shifted the sentence about advanced modeling to elsewhere in the discussion section.
Line 689: Conclusions about which factors influence the rockfall hazard are crucial since it is the central topic. But this should be done more systematic and also the use of an integrated analysis could be better highlighted. Therefore, it should be argued with the geologic and climatic factors investigated in this study, how they are implemented in a integrated analysis and how other concepts like frequency-magnitude relationships could be implemented too.	We've added text to emphasize the integrated approach we use here. The other approaches are perhaps not as relevant for the conclusion section, but these are good points.
Ms: The manuscript has been clearly improved regarding many small aspect. But the integrated analysis is not rigorously implemented yet, there are still many quantitative analyses and the structure has still some issues. For a publication, at least the integrated analysis must be improved by assigning the susceptibility from the kinematic analysis to the trajectories of the runout modeling and a schematic workflow must be presented and followed, while the qualitative analyses can be kept but as siding.	We have clarified the workflow in a few places in the text. We opted against an additional schematic and instead added a new paragraph in the methods section that outlines the linkages more clearly and uses workflow language.
Section 3: The methods applied are promising and now better described. However, there are still many qualitative approaches and the proper coupling of the kinematic analysis with the runout modeling is missing. While the qualitative approaches are acceptable as a side contribution specific to the site, the model coupling is crucial for an	We added a paragraph to section 3.7 that clearly describes the coupling of the two models.

integrated analysis as the main contribution with a more general significance.	
Section 5: The structure of the discussion has been improved with a more clear narrative. Nevertheless, it is missing critical appraisals and several aspects that are open before but not closed yet. It must be discussed why the rockfall inventory, the geomorphic mapping and runout factors are only analyzed qualitatively, new aspect like the slope and the roughness should be discussed as well and it should systematically address geologic and climatic factor that influence the rockfall hazard.	We've attempted to bolster the discussion section with some additional verbiage that addresses some of these comments.
Section 6: The conclusion has been improved in the sense that some conclusion are now drawn. But it is still missing a systematic answer to the question, which geologic and climatic factor influence rockfall hazard and which questions are still open and require more research for being answered.	We've rearranged the conclusion for clarity. We unfortunately, however, do not have a systematic answer but have identified clear spatial and temporal patterns in rockfall activity that vastly expand our knowledge base.
Sec 3.1. The overview has been improved by a better description of what is done in the study. However, this overview is still not sufficient regarding that the analysis is rather complex including different aspects. A figure with a schematic workflow would be very helpful for the reader but also as a basis to improve the structure of the study.	We've added a paragraph to accomplish the integration as requested.
Sec 3.7. The coupling of RAMMS::Rockfall with the kinematic analysis is a promising approach. But a lot of potential is untapped by not incorporating the exact value for the susceptibility for failure and the trajectories of the runout. Since the model provides trajectories as output, the susceptibility for failure of the source could be assigned and by dividing by the number of trajectories per start point, an overall susceptibility for rockfall could be mapped.	We've clarified the specific values needed to accomplish the coupling. In the results, we describe and perform the next steps of normalizing the model results as suggested here.
Sec 4.1. The analysis of the rockfall inventory has been greatly improved. Nevertheless, it includes interpretations in the last paragraph that would also require citations. The description of the findings based on the figure should be kept in the results but their interpretation should be placed in the discussion.	We've modified the text and added citations.
Sec 4.4. The presentation of the findings from the kinematic analysis has been clearly improved. But it includes interpretations and could be better structured. The interpretation in the context of the failure criteria equations should be placed in the discussion and the part about the combination with the talus deposits could be in a separate paragraph.	We've broken this section in two paragraphs to improve the readability although the equations are key for introducing these results and reminding readers how they were generated. So, we've retained the equations here.
Sec 4.5. The analysis of the runout modeling has been improved regarding the focus on the runout patterns. However, it is now limited to the two extreme scenarios, which is not understandable and a loss. All four scenarios should be included into the analysis for properly analyzing the influence of the block size and the forest cover.	We've opted to include the two end-member scenarios in order to enable readers to better compare the spatial patterns with the maps and slope and roughness data.