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Interactive comment on "Automatic delineation of geomorphological slope-units and their optimization for landslide susceptibility modelling" *by* Massimiliano Alvioli et al.

Anonymous Referee #1

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The manuscript introduces a new and apparently user-friendly software, called r.slopeunits, for the automatic subdivision of the terrain into terrain units (i.e. slope-units; SU). From a geomorphological perspective, the parameters "a" (minimum size of slope-units) and "c" (circular variance of slope aspect) represent the crucial parameters to control the size and orientation (i.e. aspect) of the SU. An optimal subdivision of the terrain into SU (measured with the introduced segmentation metric F(a,c)) is characterized by a high internal homogeneity (i.e. low local aspect variance within a SU) and a high external heterogeneity (i.e. high variability between SU) of SU. The authors also propose an approach to identify the "best" combination of "a" and "c" to generate SU for statistical landslide susceptibility modelling. This procedure is based on the

previously mentioned SU segmentation metric and the fitting performance of the generated landslide susceptibility models (measured with the metric AUROC). The optimal terrain partitioning for landslide susceptibility modelling is then based on a combination of high segmentation performance of SU (F(a,c)) and a high fitting performance (i.e. high AUROC) and measured via the introduced function S(a,c). Software and optimization approach were tested in Central Italy, where landslide susceptibility was modelled using logistic regression and a large set of potential predictors. The quality of the present discussion paper is – for its most part - high and contains very useful graphs. From my perspective, the application of r.slopeunits appears to be very useful for a variety of purposes, but especially for empirically-based landslide modelling (e.g. landslide susceptibility modelling, probabilistic hazard modelling, etc.). The presented model is able to account for multiple important details (e.g. minimum area, removing odd-shaped units) and seems to be sophisticated from a technical perspective. I think that the usability/limitations of SU-based approaches, as well as the presented optimization-approach, should be discussed more thoroughly (see comments below). I recommend a moderate revision.

I believe that the paper would improve by addressing the following issues:

(i) Adding some discussion on advantages and limitations of the presented model and optimization-approach. E.g. As a potential user of the software, I am very interested on why I should favour SU (e.g. over more easily applicable grid-based approaches) for the purpose of landslide susceptibility modelling. What are potential advantages and disadvantages of the presented software and SU-based landslide susceptibility models in general? (ii) Shifting some text parts to other sections respectively slightly restructuring the paper (see comments below) (iii) Reducing some redundancies within the text (see comments below) (iv) Modifying some figures (see comments below)

Specific comments:

1. From my experience, the boundaries of SU do not directly correspond to the units

used by spatial planners (i.e. spatial planning-units are not equal to geomorphic units which is somehow addressed also in p. 11 line 21f)). For me, grid-based landslide susceptibility models appear more flexible in this respect as they can be (with some limitations) regularly and easily adapted to such boundaries (e.g. aggregating pixel values etc.). Discussion of such issues would shed more light on the usability of the presented software.

2. SU are related to hydrological and geomorphological conditions and therefore well suited for landslide susceptibility modelling (cf. p.1, line 20f). The delineation of SU does not consider any quantity other than terrain aspect. Thus, I assume that a single SU, which is represented by similar slope aspects, may as well exhibit a high intervariability of other landslide-influencing topographical properties (e.g. high slope angles in the upper part and low slope angels in the lower part). Thus, there might be as well a high intervariability of landslide susceptibility within a single SU, because the inclination of a slope is directly related to shear stresses. This might be one drawback of SU within the context of statistical landslide susceptibility modelling and should be discussed within the paper. Furthermore, I wonder if a consideration of the variability of slope angles (or a combination of slope angels and terrain aspect) would be possible or reasonable when dividing the terrain into homogeneous areas for the purpose of landslide susceptibility modelling?

3. If I am right, SU affected by only one landslide are treated equally as those units affected by e.g. 10 landslides (= landslide-presence unit). In other words, SU-based approaches do not differentiate between slope units slightly (e.g. 3% of the area) or highly affected (e.g. 80% of the area) by landsliding. From my perspective, such tendencies might also affect the final susceptibility modelling results. This argument further highlights that a detailed discussion of advantages and disadvantages of SU (in the context of landslide susceptibility modelling) may be highly beneficial for potential users of r.slopeunits.

4. The authors mention that “a detailed terrain partitioning, with many small SU, is

required to capture the complex morphology of badlands, or to model the susceptibility to small and very small landslides (i.e. soil slips)” (p. 15, line 2f). Please provide clarification if or how the proposed optimization approach accounts for terrain complexity (i.e. are there other parameters than terrain aspect?). For instance, does the optimization approach favour small SU in the case of complex terrain morphology or very small landslides or should the users decide by themselves on those parameters (which would be in contradiction to the proposed optimization approach)? Is the portion of SU considered as presences (= affected by landsliding) higher in the case the respective units are larger? If yes, does such a change in the ratio between landslide-presences to absences systematically change subsequent modelling results? E.g. Figure 10 suggests that the total areal extent of susceptible areas increases with an increasing SU-size. Could this also influence the apparent fitting performance of the model? Please discuss.

5. The findings demonstrate that the number of significant variables generally increased with a decreasing size of slope-units. Predictors related to local terrain settings (e.g. slope angle) were frequently neglected by the models while especially predictors related to lithology controlled the final landslide susceptibility modelling results. From the text, I deduce that the dominance of lithological parameters is related to an increasing SU-size. The authors finally infer that a logistic regression model generated for an area represented by very large SU “can be replaced by a simple heuristic analysis of the lithological map” even though high AUROC values might be achieved (p .13, line 20f). I think that this observation further indicates that a purely quantitative optimization of a SU-partitioning might (in some cases) not be sufficient to produce high qualitative SU-based landslide susceptibility maps. Please discuss.

6. The proposed combination of the aspect segmentation metric and the AUROC metric appears logical from a purely quantitative perspective. The subsequent final metric S(a,c) is then based on a multiplication of both previously mentioned and normalized metrics. It would be interesting to know why the authors chose to multiply the respec-

tive metrics (instead of using an average) to get S(a,c). Is it because a very low value of one metric (e.g. 0.1) prevents an “acceptable” S(a,c)-value in the case the other metric is relatively high (e.g. 0.8) (e.g. multiplication: 0.08 vs. average: 0.45)?

7. The usage of S(a,c) appears to be based also on the assumption that a high AUROC value is directly related to a higher quality/usability of the underlying logistic regression based model. In this context, I do not see a problem to use the calibration set to measure this metric since logistic regression models are relatively robust and do not tend to (strongly) overfit on training data. However, several studies outline potential limitations of AUROC-based measures for spatial distribution models (Frattini et al., 2010; Lobo et al., 2008; Steger et al., 2016). Therefore, I assume that there might be a risk that the conducted direct deductions (i.e. by interpreting solely the metrics) can lead to misleading conclusions. I think that a discussion of potential drawbacks of the proposed metrics (S(a,c)) would also be valuable.

Frattini P, Crosta G, Carrara A (2010) Techniques for evaluating the performance of landslide susceptibility models. Engineering Geology 111:62–72. Lobo JM, Jiménez-Valverde A, Real R (2008) AUC: a misleading measure of the performance of predictive distribution models. Global Ecology and Biogeography 17:145–151. Steger S, Brenning A, Bell R, Petschko H, Glade T (2016) Exploring discrepancies between quantitative validation results and the geomorphic plausibility of statistical landslide susceptibility maps. Geomorphology 262:8–23.

8. I also suggest a minor restructuring of some text parts to enhance readability and to reduce redundancies:

From my perspective, several “non-discussion-parts” of the manuscript already contain useful discussions (e.g. p8 line 28-30; p. 9 line 28f; p. 10 line 11f etc.). As a reader, I would prefer to read those text segments within a well-structured discussion, which summarizes those issues (e.g. one part may relate to the SU-segmentation, another to advances/limitations of the metrics, another to the conducted susceptibility modelling

etc.). I propose to restructure and expand the discussion part (e.g. including subsections, separating discussion and conclusion).

p. 2, line 20-23: I suggest to shift these lines to section 5 (“Landslide susceptibility modelling”)

p. 13f (section 8.4): I propose to address the calculation of the combined metric S(a,c) within the methods section.

p. 14 line 15 to 20: Those sentences are similar to the text passages in p. 1 line 16-19. I recommend explaining the concept of SU solely within the introduction (p.1)

9. Figure 5: I would prefer a more intuitive colour selection for the land cover map (e.g. dark green for forests, light green for pastures, brown for arable land, blue for water etc.). Figure 10: I think that inserts of corresponding metrics (F(a,c), R(a,c), S(a,c), fraction of significant predictors) within each of those nine maps would further enhance traceability (i.e. interrelations) of the results. Maybe you can also present the susceptibility map produced by the optimal parameter combination (i.e. “a” and “c”).

10. The title clearly reflects the content of the paper. The guidelines of the journal (GMD) state that “the model name and number should be included in papers that deal with only one model.” Maybe the authors can add the name of the software “r.slopeunits” to the title?

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