Answers to Anonymous Referee #2

The manuscript presents a complex approach to optimized hillslope partitioning designed for landslide susceptibility modeling. One of the innovative contributions of this work is the optimization of parameters - many geoprocessing methods require tuning empirical parameters so even highly automated procedures can become time consuming and subjective - this paper tries to address this issue using an iterative process along with robust statistics. The proposed approach can be adapted to other types of landform units mapping so the paper could potentially have broader impact.

The most serious issue with the paper is unclear validation or evaluation of the results. In the fig. 7 the derived hillslope units appear to have little relation to the observed landslides - I assume that landslide model would have to be applied to each unit to find out whether there would be a landslide but many observed landslides cross the hilslope unit boundaries and the size of units is not very consistent with the distribution and size of the landslides.

In this work, we did not take into account validation of the modelling results *i.e.,* no evaluation of the performance of the landslide susceptibility model was performed using e.g., a different set of landslides (temporal validation) or applying the model to a different location (spatial validation). The proposed optimization procedure is executed only in the model calibration phase of the logistic regression model, for at least two reasons. The first reason is that validation of the results would have added additional (and unnecessary for the scope of this work) complexity to the analysis; whereas the focus has to be on the slope-unit delineation and the optimization procedure. The second reason is that, from a conceptual point of view, the very purpose of the procedure "*is not to evaluate the performance of the LS classification but to help determining an optimal terrain subdivision for LS modeling, and thus before any LS model is available for proper validation*", as we write in Section 5 of our work.

Coming to the specific comment of the Referee, we stress that r.slopeunits does not require landslide information. The software only accepts numerical input and produces a well-defined output. A landslide inventory is used only in the susceptibility model. It is the optimization procedure's task to choose input parameters for r.slopeunits such that the aspect direction map is consistently segmented into domains facing well-defined directions, and that the landslide susceptibility model training stage performs well. In other words, the requirement that the final slope-unit based map contains polygons crossing landslide deposits is not implemented explicitly. We may only expect that landslide occurred within well-defined aspect domains (i.e., well-defined hillslopes), and that a good performance of the susceptibility model is associated to a slope-unit delineation that, on average, does not cut landslide deposits. The specific examples shown in Figure 7 illustrates precisely this point. The three *sample* slope-unit delineations were obtained with three different combinations of the parameters, providing slope-unit polygons of different shapes and sizes. The selection of the best delineation is not yet performed at this stage.

The paper does not show the details of how the final optimized results look like when overlayed with the observed landslides. It would be useful to at least include reference to the figure 10 center as the optimal partitioning, if I understand the text correctly. Is there any way to measure the improvement achieved by the proposed approach versus using simpler approaches such as the half-basins derived from r.watershed with optimized threshold or a raster based landslide model?

It is true that showing, or providing the optimal slope-unit based map would add value to our paper, and we have added the map to the revised version. Also, it is correct that the center box in Figure 10 turned out to be the optimal slope-unit partition, and we now acknowledge this in the Discussion section.

The Referee's comment concerning the improvement achieved by the proposed approach versus simpler approaches is pertinent only if one assumes that the half-basins produced by r.watershed can be considered mapping units fulfilling the requirements of maximization of internal homogeneity and external non-homogeneity. The partition of the terrain obtained using r.watershed depends on the accumulation threshold selected for the delineation of the first order streams. This modelling parameter is the same for the *whole area* under examination. Use of small values of the accumulation threshold results in an over-segmentation of hillslopes internally homogeneous. This can be overcome using r.slopeunits, which determines different accumulation thresholds in different regions of the study area, according to local terrain variability, quantified by aspect homogeneity. A susceptibility model based on half-basins with a single accumulation threshold would be of little conceptual meaning. For this reason, it was not calculated.

Few minor comments and questions: - the term slope units could be misleading (e.g. it could be interpreted as classes of slope steepness) - perhaps hillslope units may be better, but leave the slope units if this term is used by this journal

We decided to stick to the slope-unit name and definition, which is well known in the literature as it can be seen checking the list of references.

- the landslides were from the years 1954-77, were the contours from which the DEM was interpolated from the same time period? Were at least some of the landslides captured by the contours?

The contour lines used to prepare the DEM are more recent than the landslides. The DEM captures the morphology of the large and very large mass movements.

Implementation as a module in open source GIS makes reproducing the results easier, the authors should also provide their data set to ensure full reproducibility and comparison with other methods

We stress here that the reproducibility invoked throughout the manuscript refers to the procedure we have developed and tested, and not to the final result pertaining to the particular example presented in this work to illustrate the procedure. We claim reproducibility of the results because, unlike many existing approaches in the literature, every step is parametrized and nothing is left to expert judgement. As a result, for a given dataset and values of the input parameters, the result is uniquely determined if the r.slopeunits software is used to delineate the slope-units. We consider this a significant advantage and an advancement towards standardization.

The datasets used in this work consists of (i) a 25 x 25m DEM, (ii) a reclassified lithological map, (iii) a reclassified land use map, and (iv) a detailed landslide inventory. The datasets were obtained in collaboration with third parties, who should be consulted before the data are disclosed by any means or used for purposes different from obtaining results for a scientific publication. The mentioned data may be available upon motivated request, but adding datasets to this work would only slow down the publication process, and will not add to the content and meaning of the paper. We have added the final result of our procedure applied to our test area, the optimal slope-unit delineation, in Figure 14. The vector layer is also available upon request, should anyone be interested in comparing results obtained with other models or approaches. The new figure is attached to this comment as Figure 1.