Reply to Anonymous Referee #1

The paper deals with an interesting topic, zoning of shallow landslide susceptibility at basin scale. The original contribution of the paper is the combined use of two different methodologies (in the paper called empirically-based and physically-based methods) to evaluate and map landslide susceptibility over a catchment. The paper is well structured. The proposed procedure, which is based on a "set of integration rules defined by the cross-tabulation of the susceptibility classes of both maps and analysis of the corresponding contingency tables", is clearly described in the paper. The application of the procedure in the test site (a study area in Portugal) effectively demonstrates the effectiveness of the proposal. The methods chosen in the application (i.e. the bivariate statistical information value method and the infinite slope method) adequately serve the purpose of the research.

Authors reply: We acknowledge and appreciate the global positive evaluation made by Reviewer #1.

My only major comment is the following. The name chosen to define the first adopted methodology, called by the Authors "empirically-based methods" (also used in the title of the paper), does not adequately represent the class of methods the Authors refer to, i.e. the statistical methods. Although the adjective "empirical" is sometimes used to include both heuristic and statistical analyses (e.g. Goetz et al. 2011), most commonly the methods used to compute landslide susceptibility are differentiated in three classes; Fell et al. (2008a, 2008b) call them basic, intermediate and advanced methods. Within this framework subjective heuristic analyses should be considered basic methods while data-driven statistical analyses clearly belong to the second of these classes. If the Authors, as it appears, want to refer to intermediate methods only, I suggest they change the term "empirically-based methods" with "statistical methods" throughout the paper. However, if the Authors want to refer to both heuristic and statistical analyses, they should make it clear it to the reader, by stating it explicitly in the paper.

Authors reply: Although the term "empirically–based" had been suggested in the initial manuscript revision by the journal editor before acceptance for discussion in NHESS we agree with this suggestion. Therefore, the title of the new version of the manuscript is "Combination of statistical and physically-based methods to assess shallow slides susceptibility at the basin scale". In addition, any reference to "empirically-based" was replaced by "statistical" along the text.

See Attached PDF file for specific comments on Figures, Tables and Manuscript corrections.

Authors reply: We appreciate the Reviewer comments. All specific comments on figures, tables and manuscript text are answered after this general comment on a point by point basis. With respect to manuscript text corrections and suggestions marked on the attached pdf file we agree with all suggestions and we changed the text accordingly.

Specific comments on Figures, Tables and Manuscript corrections.

Reviewer comment (Page 1, Line 27) "have been made worldwide supported" check English.

Authors reply: We agree with the reviewer comment. The above mentioned text was grammar and style revised. In the new version of the manuscript the phrase where this text section is included change to "The evaluation of landslide susceptibility has been carried out worldwide based on three fundamental principles (Varnes et al., 1984; Carrara et al., 1991; Hutchinson, 1995; Guzzetti, 2005):"

Reviewer comment (Page 2, Lines 9-11) "That is, unlike what happens with statistical methods, deterministic methods are applicable not accounting the landslide inventory, which, however, is still essential to validate the obtained landslide susceptibility results." Improve English.

Authors reply: Again we agree with the reviewer comment and the above mentioned text was grammar and style revised. In the new version of the manuscript the phrase was rewritten as "Unlike landslide susceptibility models based on statistical methods, landslide inventories are not used to assess landslide susceptibility with deterministic methods. However, landslide inventories still remain essential to validate the obtained landslide susceptibility maps."

Reviewer comment (Page 5, Lines 5-8) "Final IV scores (Lxi) for each terrain unit (j) was obtained using Eq. (2)." Do you mean Lxi?. Check symbols. Explain what is Lj (quote in the text, not only in the formula). What is Ii?

Authors reply: We acknowledge and understand the reviewer doubt. In order to be clearer we replaced *Lxi* by *Ii* in this new version of the manuscript. Additionally all symbols were verified in the new version of the manuscript and we believe that now is

clear the meaning of *Ii* and *Ij*. In this new version of the manuscript Eq. 1 and Eq. 2 are shown and described as follows:

$$Ii = log \frac{Si/Ni}{S/N} \ , \qquad \text{(Eq. 1)}$$

"where: *Ii* is the Information Value of class *Xi* belonging to an independent variable (landslide predisposing factor); *Si* is the number of pixels with shallow slides belonging to the training group and the presence of the variable class *Xi*; *Ni* is the number of pixels with variable class *Xi*; *S* is the total number pixels with shallow slides belonging to the training group; and *N* is the total number of pixels of the study area. Due to the logarithmic normalization *Ii* is not calculated when *Si* = 0. In these cases *Ii* was determined as the lowest information value considering the complete data set of landslide predisposing factors. The final IV scores (*Ij*) for each terrain unit (j) was obtained using Eq. (2).

$$\mathrm{Ij} = \sum_{\mathrm{i=1}}^{\mathrm{m}} \mathrm{Xij\ Ii} \ , \qquad \text{(Eq. 2)}$$

where: *m* is the total number of variable classes; and *Xij* is either 0 if the variable class is not present in the pixel *j*, or 1 if the variable class is present."

Reviewer comment (Page 6, Line 4) "introducing that way, the saturated soil thickness factor." Not clear

Authors reply: We agree with the reviewer comment and the abovementioned text was revised accordingly. In the new version of the manuscript the phrase was rewritten as: "The most popular formulations of the Infinite Slope method consider a subsurface flow/water table level parallel to the topographic surface, whose maximum depth is equivalent to the maximum thickness of the saturated soil."

Reviewer comment (Page 6, Line 18) "tier according FS values being more susceptible the terrain unit as lower the FS value." Check English

Authors reply: We agree with the reviewer comment and the abovementioned text was revised accordingly. In the new version of the manuscript the phrase change to: "In a broader interpretation the FS results are compared with results obtained using

the statistical approach; in other words each terrain unit within a study area can be ranked according to its FS value, where the lowest FS value indicates the highest landslide susceptibility."

Reviewer comment (Page 7, Lines 1-2) "The three parameters C, η and ψ-1 were expressed by linear normalization into a dimensionless index with values ranging between 0 and 1." Not clear. Explain differently

Authors reply: As suggested by reviewer the abovementioned text was differently explained to become clearer. In the new version of the manuscript the phrase will change to: "The three parameters C, η and ψ-1 were expressed in a scale ranging between 0 and 1. For each parameter, the value 1 was assigned to the maximum observed value, 0 to the minimum observed value and the remaining observed values were assigned numbers between 0 and 1 by linear normalization."

Reviewer comment (Page 8, Lines 1-2) "Insert blibliographic reference

Authors reply: It was our understanding that the reviewer asked to insert a bibliographic reference related with the national digital soil map at 1: 25,000 scale. Therefore the following reference (DGADR, 1999) was used and included on the reference list in the new version of the manuscript: DGADR: Cartas dos Solos de Portugal - Cartas Complementares, Folha 389, Escala 1:25000. SROA/CNROA/IEADR/IDRHA/DGADR, 1999.

Reviewer comment (Page 10, Line 18) "is detached to the upper left corner of ROC space" meaning not clear

We agree with the reviewer comment and the abovementioned text was revised accordingly. In the new version of the manuscript the phrase changed to: "…is closer to the upper left corner of the ROC curve graphic"

Reviewer comment (Page 10, Lines 26-31) "Given the assumed boundary conditions, it was expectable that model do not generate FS ≤ 1. However, Fig. 8B shows a small fraction of the study area classified with Very high susceptibility (FS ≤ 1, 2.25 % of study area) in a condition of absence of water into the soil, which is interpreted as an error of the IS model. It is worth mentioning that most of the model errors occur over the LU2 (Arranhó formation) indicating that corresponding resistance parameters

(cohesion, internal friction angle) may be underestimated." Why was this issue not tackled during the calibration phase? Explain

Authors reply: We acknowledge the reviewer comment. In fact, we have tested a susceptibility model considering the geotechnical parameters (cohesion and friction angle) that return as results no areas with FS ≤ 1 with no water into the soil. However, these parameters proved to be too high to correctly express the landslide susceptibility in the study area considering the existence of water into the soil: the area classified as unstable (with FS ≤ 1) corresponds to only 1.3% of the total study area and validates only 8.1% of the landslides belonging to the training group.

In the new version of the manuscript, the geotechnical resistance parameters of each lithological type (cohesion and angle of internal friction), which guarantee FS> 1 in the absence of soil water (m = 0) will be added to Table 3 as follows:

Table 3. Geotechnical parameters assigned to each lithological unit (LU). In brackets, cohesion and internal friction angle for each LU to guarantee FS>1 in the absence of water into the soil (m=0).

LU	Specific soil weight (mean values)			Cohesion	Internal friction angle
	Saturated soil (kN/m³)	Natural soil (kN/m³)	Submerged soil (kN/m³)	(kPa)	(°)
1	17.5	16.5	7.69	3.0 (3.0)	19 (19)
2	20.9	19.9	11.1	0.5 (1.0)	17 (27)
3	20.6	19.6	10.8	2.0 (4.0)	16 (22)
4	20.6	19.6	10.8	2.0 (4.0)	15 (19)
5	20.9	19.9	11.1	1.5 (3.0)	24 (24)
6	19.6	18.6	9.8	3.0 (3.0)	19 (21)
7	19.6	18.6	9.8	2.0 (4.0)	19 (22)
8	26.0	25.0	16.2	50.0 (50.0)	35 (35)

Therefore, in the end of section 4.2 (Physically based landslide susceptibility assessment) the following text was included in the new version of the manuscript:

"The cohesion and internal friction angle values that guarantee FS>1 for any LU in the absence of water into the soil (m = 0) are summarized in Table 3 (in brackets). These geotechnical parameters were tested in a new model (susceptibility map not showed) considering the existence of water into the soil and the obtained result is not reliable: the area classified as unstable (with FS ≤ 1) corresponds to only 1.3% of the total study area and validates only 8.1% of the landslides belonging to the training group. Therefore, we conclude that the geotechnical parameters that guarantee the absence of cells with FS ≤1 when m = 0 are too high to correctly express the landslide susceptibility in the study area."

Reviewer comment (Page 29, Figure 2) Use bidirectional arrows for lines connecting with the validation box (on both sides)

Authors reply: We agree with the reviewer suggestion and bidirectional arrows for lines connecting with validation box (on both sides) were added to figure 2 in the new version of the manuscript.

Reviewer comment (Page 30, Figure 3) List the variables in the figure caption. Insert reference to Table 4 in the caption.

Authors reply: As suggested by reviewer the variables were listed in the figure caption and reference to Table 4 was included. The caption of Figure 3 in the new version of the manuscript was rewritten as “Figure 3: Dataset of shallow slides predisposing factors. A) slope, B) aspect, C) profile slope curvature, D) topographic position index, E) slope over area ratio, F) land use. Lithology is shown in Figure 1 and the description for each class of landslide predisposing factor in Table 4.”

Reviewer comment (Page 32, Figure 5) Change Hydrological model with "Ratio h/z"

Authors reply: As suggested by reviewer in the new version of the manuscript Hydrological model was changed by “Ratio h/z” in figure 5.

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Reply to Referee #2 M. Mergili (Referee)

The authors analyse the susceptibility of a study area in Portugal to shallow landslides, comparing a statistical model and a physically-based model. Thereby they split their landslide inventory into two temporal subsets, one for building the statistical model and for calibrating the key parameters of the physically-based model, and a second one for model validation. They further compare the outcomes and combine the results in order to obtain a more informative landslide susceptibility map. The topic is highly relevant, the research is well described, and the manuscript is well structured and illustrated. As it is usual for discussion papers, there is some potential for improvement. I have identified a number of minor issues which should be addressed before I can finally recommend the manuscript for publication in NHESS. All in all, I suggest minor revisions

Authors reply: We acknowledge and appreciate the positive evaluation of the manuscript made by the Referee #2.

General comments:

Reviewer comment - I suggest to refer to "statistical" instead of "empirically-based" models.

Authors reply: To this respect, we acknowledge that Reviewer #2 makes the same suggestion as Reviewer #1. We agree with the suggestion. Therefore, the title of the new version of the manuscript is: "Combination of statistical and physically-based methods to assess shallow slides susceptibility at the basin scale". In addition, any reference to "empirically-based" was replaced by "statistical" along the text.

Reviewer comment - The illustrations are fine, but you might consider underlaying all the maps with a hillshade.

Authors reply: As suggested by the reviewer all maps in this new version of the manuscript were underlined with a hillshade model of the study area. The suggested changes were made in figure 1, figures 3 to 6 and in figures 8 to 10.

Reviewer comment - Even though the discussion paper is well written in general, some final polishing of grammar and style will be necessary.

Authors reply: As addressed by the reviewer English grammar and style verification through the new version of the manuscript was made. To accomplish that, full text

revision was made by a bilingual Portuguese and English translator hired to a specialized translation service.

Reviewer comment - A further reference that could be interesting: de Lima Neves Seefelder, C., Koide, S. & Mergili, M. (2016) Does parameterization influence the performance of slope stability model results? A case study in Rio de Janeiro, Brazil. Landslides, doi:10.1007/s10346-016-0783-6

Authors reply: We read the work suggested by the reviewer and taking into account the discussion that authors make on the geotechnical spatial parameterization associated to physically-based models and comparison with statistical models we decided to quote the suggested article in the introduction section in the new version of the manuscript.

Specific comments:

Reviewer comment - p7, l14 (and several other instances in the manuscript): I think that "hydrological model" should be replaced by "hydraulic model".

Authors reply: Although the term hydrological model had been generally used in literature considering rainfall infiltration conditions as steady state (e.g. Park, 2013; Teixeira, 2015) we agree with the review suggestion and we changed "hydrological model" by hydraulic model" along the new version of the manuscript.

References:

Park, H. J.; Lee, J. H.; Woo, I. (2013) Assessment of rainfall-induced shallow landslide susceptibility using a GIS-based probabilistic approach. Engineering Geology, 161: 1–15.

Teixeira, M.; Bateira, C.; Marques, F.; Vieira, B. (2015) Physically based shallow translational landslide susceptibility analysis in Tibo catchment, NW of Portugal. Landslides, 12: 455–468.

Reviewer comment - p8, l7: What do you mean with "social" areas – maybe "cultivated" areas?

Authors reply: "Social" areas are used in the official Portuguese digital soil map at 1: 25,000 scales to describe urbanized areas. Therefore in order to avoid misinterpretations we changed "social areas" by "urban areas".

Reference: DGADR: Cartas dos Solos de Portugal - Cartas Complementares, Escala 1:25000. SROA/CNROA/IEADR/IDRHA/DGADR, 1999. http://www.dgadr.pt/nota-explicativa

Reviewer comment - p8, 130ff: Maybe you should mention in an additional sentence (here or in the conclusions) that, through back-calculation, the geotechnical parameters loose their direct physical meaning, but are instead a product of statistics. Even though the infinite slope stability model clearly remains physically-based, in fact parameter calibration gives it a bit of a statistical touch.

Authors reply: We acknowledge the reviewer comment, and we think the observation is correct and pertinent. Therefore we included in the conclusion just after physically-based models drawbacks the following text: "Additionally, although the infinite slope stability model remains physically-based, the used geotechnical parameters lose, to some extent, their direct physical meaning since critical cohesion and internal friction angle combination were determined statistically assuming the highest effective ratio."

Reviewer comment - p10,l22: I would not consider a rainfall with a duration of 15 days a short duration rainfall event.

Authors reply: The interval 1 to 15 days has been used to express the rainfall conditions that typically trigger shallow landslides, which contrast with rainfall periods lasting several weeks that are associated with deep-seated landslides (e.g., Zêzere et al., 2005; Zêzere et al., 2015). We acknowledge that 15 days is in the borderline between the two groups. To avoid confusion, we kept out the reference of number of days in the new version of the manuscript.

References:

Zêzere JL, Trigo R, Trigo I (2005) Shallow and deep landslides induced by rainfall in the Lisbon region (Portugal): assessment of relationships with the North Atlantic Oscillation. Nat Hazards Earth Syst Sci 5:331–344. doi:10.5194/nhess-5-331-2005

Zezere, J.L.; Vaz, T.; Pereira, S.; Oliveira, S.C.; Marques, R.; Garcia, R. (2015) - Rainfall thresholds for landslide activity in Portugal. Environmental Earth Sciences, 73(6): 2917-2936. http://link.springer.com/article/10.1007%2Fs12665-014-3672-0

Reviewer comment - p14, l10f: It could also be possible to identify uncertain areas with one single model by varying some input assumptions and parameter

combinations tested and, e.g., analyzing the standard deviation of the results. Even though I agree that it is much better to compare different model approaches, you might consider to slightly reformulate your final statement.

Authors reply: We appreciate the suggestion of the reviewer and we think that the observation is correct and pertinent. Therefore we reformulated our final sentence as: "Although it was possible to identify uncertain areas with one single model by varying some input assumptions and parameter combinations, our work demonstrates that the combination of both methods allowed the identification of areas classified as uncertain regarding landslide susceptibility but with potential to be highly/very highly susceptible to shallow slides occurrence, which is not possible when using a single landslide susceptibility model."

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