

### 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 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 will be the following: “Combination of statistical and physically-based methods to assess shallow slides susceptibility at the basin scale”. In addition, any reference to “empirically-based” will be 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 and all specific comments on figures, tables and manuscript text will be answering after this our general comment on a point by point basis. With respect to manuscript text corrections suggestions marked on the attached pdf file we agree with all suggestions and we will change 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 and the above mentioned text and the English were verified in this new version of the manuscript. In the new version of the manuscript the phrase where this text section is included will be changed to “The evaluation of landslide susceptibility has been made worldwide sustained on three fundamental principles:”

**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 revised accordingly. In the new version of the manuscript the phrase will change to “That is, unlike landslide susceptibility models based on statistical methods, landslide inventories are not used to access landslide susceptibility with deterministic methods. However, landslide inventories still remain essential in the validation process of the obtained landslide susceptibility maps.”

**Reviewer comment (Page 5, Lines 5-8)** “Final IV scores ( $L_{xi}$ ) for each terrain unit ( $j$ ) was obtained using Eq. (2).” Do you mean  $L_{xi}$ ? Check symbols. Explain what is  $L_j$  (quote in the text, not only in the formula). What is  $l_i$ ?

**Authors reply:** We acknowledge and understand the reviewer doubt. In order to be clearer we replaced  $L_{xi}$  by  $l_i$  in this new version of the manuscript. Additionally all symbols were verified and we believe that in the new version of the manuscript it will

be clear the meaning of  $I_i$  and  $I_j$ . In this new version of the manuscript Eq. 1 and Eq. 2 will be presented and described as follows:

$$I_i = \log \frac{S_i/N_i}{S/N} , \quad (\text{Eq. 1})$$

“where:  $I_i$  is the Information Value of class  $X_i$  belonging to an independent variable (landslide predisposing factor);  $S_i$  is the number of pixels with shallow slides belonging to the training group and the presence of the variable class  $X_i$ ;  $N_i$  is the number of pixels with variable class  $X_i$ ;  $S$  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  $I_i$  is not calculated when  $S_i = 0$ . In these cases  $I_i$  was determined as the lowest information value considering the complete data set of landslide predisposing factors. The final IV scores ( $I_j$ ) for each terrain unit ( $j$ ) was obtained using Eq. (2).

$$I_j = \sum_{i=1}^m X_{ij} I_i , \quad (\text{Eq. 2})$$

where:  $m$  is the total number of variable classes; and  $X_{ij}$  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 will change to: “The most popular formulations of the Infinite Slope method consider a subsurface flow/water table level parallel to the topographic surface, which 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 will change

to: "...to consider that 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,  $\eta$  and  $\psi-1$  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 explained to become clearer. In the new version of the manuscript the phrase will change to: "The three parameters C,  $\eta$  and  $\psi-1$  were expressed in a scale ranging between 0 and 1. For each parameter, the value 1 was given to the maximum observed value, whereas the value 0 was given to the minimum observed value. Intermediate values were given proportionally between 0 and 1 by linear normalization."

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

**Authors reply:** It is our understanding that the reviewer asks to insert the bibliographic reference of the national digital soil map at 1: 25,000 scale: Therefore the following reference will be added to reference list in the new version of the manuscript: DGADR (1999) Cartas dos Solos de Portugal - Cartas Complementares, Escala 1:25 000, Folha 389. Elaborado por: SROA/CNROA/IEADR/IHERA/IDRHa/DGADR.

**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 will change 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  $FS \leq 1$ . However, Fig. 8B shows a small fraction of the study area classified with Very high susceptibility ( $FS \leq 1$ , 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  $FS \leq 1$  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  $FS \leq 1$ ) corresponds to only 1.3% of the total study area and validates only 8.1% of the landslides belonging to the training group. This information will be added to the new version of the manuscript.

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 weigh (mean values)t			Cohesion (kPa)*	Internal friction angle (°)*
	Saturated soil (kN/m <sup>3</sup> )	Natural soil (kN/m <sup>3</sup> )	Submerged soil (kN/m <sup>3</sup> )		
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	25	16.2	50.0 (50.0)	35 (35)

\* Cohesion and Internal friction angle between parentheses guarantee  $FS > 1$  when  $m=0$ .

Additionally, in the end of section 4.2 (Physically based landslide susceptibility assessment) the following text will be 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  $FS \leq 1$ ) 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  $FS \leq 1$  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), will be 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 will be listed in the figure caption and reference to Table 4 will be included.

**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 will be changed by "Ratio h/z" in figure 5.

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