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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Non-susceptible landslide areas in Italy and in the Mediterranean region" *by* I. Marchesini et al.

I. Marchesini et al.

ivan.marchesini@irpi.cnr.it

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Conventions used in this text:
Bold is used for the reviewer comments
Italic is used to cite parts of the manuscript
Plain text is used for the responses to the comments

The paper by Marchesini et al. presents a statistical approach to delineate terrains not susceptible to landslides over large areas using two morphometric attributes (slope gradient and relative relief) from SRTM digital elevation data together with exhaustive landslide inventory information for some areas in Italy

to establish linear, quantile-linear and quantile non-linear regression models to classify terrains as non-susceptible to landslides. It is an extension of the work done by Godt et al. (2012) to determine non-susceptible terrains over the conterminous United States using a linear model and employing identical morphometric attributes from SRTM data. The authors examined the performance of the obtained models using independent landslides information over Italy, employed Italian census data to determine the percentage of population located in non-susceptible terrains, and extended their terrain delineation over the landmasses surrounding the Mediterranean. From my opinion, the paper is interesting, well-structured and not far from being publishable in NHESS.

We thank the reviewer for his/her appreciation of our work.

I only have a very few remarks. One would be that I am missing some more information on the landslide inventories used to establish the models. The authors show that the best obtained model (the quantile non-linear model QNL) can be validated best for translational and rotational slides since the 13 inventories used for model construction mostly comprise these types of landslides. However this is not shown. What are the proportions of different types of landslide in the inventories, especially concerning their areal extent?

We concur with the referee that this is important information to provide to the reader. We are now adding this information in Table 1.

In this context, it would be also interesting to have an idea about general model robustness: How is the success of the QNL-model in the training areas, also considering landslide typology? Does the model perform significantly better here than using the validation data?

To respond to this request, we have performed a new analysis and prepared a new table (Table 5) that lists matching indexes for the different sources of landslides information used in the work, in all the 13 regional study areas. We have added language to the text to describe the results obtained. The text reads, *"Table 5 lists the matching indexes computed for the different sources of landslide information. Overall, the matching indexes are in the range 2.48% ≤ I ≤ 8.26%, with an average of I = 4.76% (std. dev. = 1.56%). Particularly small values were obtained in the Valcamonica (L in Fig. 2) (I = 2.48%) and the Imperia (M in Fig. 2) (I = 2.52%) study areas, and large values were found in the Collazzone (B in Fig. 2) (I = 8.26%) and in the Lecco (J in Fig. 2) (I = 8.13%) study areas. The average values of I change slightly for the different sources of landslide information. When considering all landslides shown in the regional inventories, I = 4.56% (std. dev. = 1.54%), which is marginally smaller than the prescribed 5%. This was expected, as the regional inventories were used to construct the non-susceptibility threshold model. Interestingly, also the average values of I obtained considering the landslides shown in the IFFI dataset are very close to the prescribed value of 5% (I = 5.15%) (std. dev. = 1.75%) for the subset with slides, earth flows, and complex and compound movements, and I = 4.57% (std. dev. = 1.42%) for the subset with slides and earth flows. We consider the reduced spread in the average value of the matching index I a measure of the uncertainty associated to our QNL threshold model."*

It would be interesting to have an estimate here. Since the authors cross binary information (susceptible/non-susceptible and landslide/non-landslide terrain information), the results of model evaluations might be very easily presented in contingency tables or using ROC graphs.

As explained answering to the comments raised by the first reviewer, we do not consider the use of contingency tables, related performance metrics and ROC analyses, appropriate for our work. The main reason is that contingency tables (CTs) are meaningful when the four "cells" in the CT are meaningful, and result from complete information. Our threshold model is meant do decide where landslides are not expected i.e., where landslide susceptibility is expected to be "negligible". The model does not say anything about the susceptibility of the other areas. The model identifies non-

susceptible areas within a 5% range of expected miss-classifications. In a recent work (under submission in the journal Geomorphology) Gariano et al. (working in our research team, http://geomorphology.irpi.cnr.it/) have shown that even a 1% difference in the values of the cells in a CT can have a large impact on some of the derived metrics of performance. Further, landslide inventory maps are known to be (necessarily) incomplete, and particularly inventories prepared for very large regions (e.g., the IFFI national inventory for Italy, see Trigila et al., 2010). Incompleteness of a "benchmark" dataset jeopardizes all attempts to measure model performance using CTs and related metrics. This is a largely underestimated problem in the landslide literature worth additional investigations. We maintain that – given all the uncertainties – it is more advisable to show indexes that measure the degree of match or mismatch between two maps (i.e., the non-susceptibility zonation and an inventory), without embarking in more sophisticated (but less meaningful) analyses.

Moreover, I am not sure if the linear Method 1 (based on the original attempt of Godt et al., 2012) should be presented since it is outperformed by the other, more convincing modeling attempts and seems not to have a real statistical significance.

We do not agree with the referee on this point. We maintain that showing the results of the application of the (modified) approach proposed by Godt et al. (2012) is useful. We did not apply our two models, and specifically our "best" QNL model to the Conterminous United States, where Godt et al. (2012) developed their model. Thus, a full comparison of the different modeling approaches cannot be made, fully. The "poor" performance of the model of Godt et al. (2012) in Italy may be the results of different reasons, including: (i) the reduced number of inventories used to construct the model (five) in a very large area (8,000,000 square kilometers) compared to the larger number of inventories (13) in an area of 300,000 square kilometers), (ii) the quality of the inventories used to calibrate the model, which we maintain was superior for the Italian regional landslide maps, and (iii) the fact that landslides were represented by a single point in the model of Godt et al. (2012) and by multiple points in our model(s).

Last, I am not sure if the extension of the method to the landmasses surrounding the Mediterranean should really be included since it cannot be convincingly validated at this stage.

The referee rises an interesting point, but uses a somewhat weak argument. The method proposed in our work is meant to exploit a – necessarily – reduced set of accurate landslide information covering a reduced area (8.9% of Italy) to determine where landslides are expected in a much larger area (all of Italy). Results in Italy are encouraging. We further validate the extended model produced for the Mediterranean region using independent information for three study areas in Spain. Results of the validation in Spain are also encouraging. Therefore, we do not understand why our model should not be "convincingly validated". We agree with the referee, that availability of additional inventory maps, of reasonable good quality, will improve the validation. To stress this point, and to respond to the comment of the referee, we make available our non-susceptibility zonation for the Mediterranean region through a dedicated Web Map Service (WMS). In this way, interested readers will be able to test the zonation using their own independent information. We have added language to the text to explain this opportunity. The text reads, *"Readers interested in using the map showing non-susceptible landslide areas in Italy, and in the Mediterranean Region, can find information on how to access the map through a specific WMS service (see http://geomorphology.irpi.cnr.it/tools/landslide-susceptibility-assessment/non-susceptible-landslide-areas/). We expect that this will contribute to validate the non-susceptibility zonation with additional independent landslide information."*. Further, in the Conclusions we write *"We expect that our synoptic-scale zonation for Italy and for the landmasses surrounding the Mediterranean Sea will be used for insurance and re-insurance purposes (Godt et al., 2012), for small-scale land planning, and in operational landslide warning systems (Brunetti et al., 2009; Rossi et al., 2012) to outline areas where landslides are*

not expected, regardless of their trigger. For the purpose, we provide a specific Web Map Service (see: http://geomorphology.irpi.cnr.it/tools/landslide-susceptibility-assessment/non-susceptible-landslide-areas/). Use of the WMS service will also help validating the non-susceptibility threshold model with independent landslide information."

Responses to specific comments:

P2816L3: Heading of Section 2: Why "preliminary"?

We have changed the heading of Section 2, which now reads "Available data"

P2817L12-13: Where all morphological landslide characteristics (e.g., depletion zone, transport zone, accumulation zone) rasterized, or only depletion zones?

We thank the referee for pointing out this important issue, which indeed was not clear in the text. In the text, we now clarify that: *"In the original inventories, for the deep-seated failures of the slide and/or complex types, the landslide source (depletion) area was mapped separately from the depositional area. The separation was not made for the shallow failures. When the landslide information was transformed to the grid format, the internal subdivisions were not maintained, and the resulting grid maps show the presence of landslides, encompassing the source and the deposition areas."*

P2818L12-P2819L10: Maybe the calculation of slope values from geographical coordinates can be moved to an appendix.

We think that proper computation of terrain slope is important in our work. All too often, information on how slope is (or other terrain variables are) calculated is neglected in published papers. When working in very large geographical areas, like in this work, proper calculation of the slope is even more important. Further, we are not aware of many papers that provide this important information, including the equations. Given the choice, we would like to keep the text in the many body of the paper. However, if the Editor prefers, we can move the text to an appendix.

Maybe the “matching index” can be better shown in terms of contingency tables and/or ROC graphs.

We responded previously to this comment.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 2813, 2014.