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Interactive Comment

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

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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

Marchesini et al. present a study that aims to identify areas in the Mediterranean region that are "non-susceptible" (minimally susceptible) to landslide activity. The authors use different methods that basically determine low percentiles of slope angles of observed landslides for different relative relief levels, and these

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percentiles are used as decision thresholds. While the approach of identifying stable areas based on quantile regression is novel and interesting in this context, I have some concerns regarding the limitations of this approach and its disadvantages compared to more widely used landslide susceptibility modeling approaches that utilize presence and absence information.

Our work is not meant to determine landslide susceptibility (for which a large number of methods and approaches exist), but rather to identify areas where landslide susceptibility is nil, or negligible. In our work, we used two morphometric variables, local terrain slope (S) and regional relative relief (R) to outline areas where landslide were not recognized in the past, and we make the geomorphological inference that they will not occur in the future. These are areas where landslides are expected "not to occur", with some degree of confidence. We clarify that our work – and our model – do not say anything concerning landslide susceptibility in the areas that are recognized as "non non-susceptible". We stress that the task of determining landslide susceptibility is conceptually and operationally different from the task of outlining non-susceptible landslide areas i.e., areas where are landslide susceptibility is expected to be negligible. We have added and changed the language in the text to explain the difference.

In the Introduction, we write *"In this work, we propose a new model to determine non-susceptible landslide areas, in Italy and in the Mediterranean region, at the synoptic scale. This is conceptually and operationally different than the determination of landslide susceptibility, using heuristic (ruled-based) or statistical approaches (Guzzetti, 2005)."* In the Discussion, we expand the argument, and we first write, *"The model exploits two morphometric variables, regional relative relief R and local terrain slope S, to outline areas where landslides were not shown in the available landslide inventory maps. Then, two geomorphological inferences are made. First, that in the areas where landslides were not shown in the inventory maps, landslides did not occur in the past. This is a strong inference, which proves reasonable only where the quality of the landslide map is good (Guzzetti et al., 2012). Second, that landslides will not occur in the* *future in the areas where they did not occur in the past. These two inferences are conceptually similar to the inferences made by geomorphologists when preparing landslide susceptibility models, and associated terrain zonations, using heuristic (rule-based) or statistical approaches (Chung and Fabbri, 1999; Fabbri et al., 2003; Guzzetti, 2005; Guzzetti et al., 1999, 2005)"*. Next, we write *"The model determines, within a given accuracy measured by the proportion of accepted miss classifications (i.e., 5%), where landslide susceptibility is expected to be "negligible" in the test regions (Figs. 7 and 9), and possibly in other regions. We clarify that the QNL threshold model does not rank the areas that were not classified as non-susceptible to landslides. This is different from the outcomes of landslide susceptibility models based on standard classification methods (Chung and Fabbri, 1999; Guzzetti et al., 1999, 2005; Guzzetti, 2005). Landslide susceptibility models that exploit classification methods (Chung and Fabbri, 1999; Guzzetti et al., 1999, 2005) rank a study area based on the probability that a terrain mapping unit (e.g., a grid cell, slope unit, unique condition unit) pertains (or does not pertain) to the group of terrain units that have (or do not have) landslides (Guzzetti et al., 1999, 2005). Where the probability to pertain to the group of the unstable units is large, the mapping unit is considered "susceptible" to landslides. Conversely, where the probability to pertain to the group of unstable units is reduced, the unit is considered "non susceptible" to landslides. Differently, when determining where landslides are not expected using a threshold-based modelling approach, like the one proposed by Godt et al. (2012) for the Conterminous United States, or our QNL model (see section 3), only the non-susceptible areas are outlined, and nothing can be said about the degree of susceptibility to landslides for the remaining territory. The difference has consequences on the measurement of the performance of the threshold model. When measuring the predictive performance of threshold-based non-susceptibility landslide models, correct predictions are areas predicted as "non-susceptible" where landslides were not observed (True Positive – TP), and areas predicted "non non-susceptible" where landslides were observed (True Negative – TN). Incorrect predictions are areas predicted as "non-susceptible" where landslides were observed (False Positive –*

FP), and areas predicted as "non non-susceptible" where landslides were not observed (False Negative – FN). This is the opposite to what is most commonly done to evaluate the performance of landslide susceptibility models (Rossi et al., 2010)".

Additional major concerns are related to the need to use performance measures that adequately summarize the models' ability to detect low-susceptibility areas (comments 11-13 below), the occurrence of a large percentage of fatal landslides in the identified "non-susceptible" areas (comments 16 and 19 below), and the limited substance of the Discussion (comment 18).

We respond to this comment in various places below.

The structure of the paper should also be improved substantially, and the Methods and Results would need to be rewritten considering suitable performance measures. Detailed comments are provided below.

We have considered this comment very thoroughly, and we have made substantial changes to the structure of the paper. The new structure is the following:

Abstract
1 Introduction
2 Available data
2.1 Terrain information
2.2 Landslide information
2.2.1 Regional landslide inventory maps
2.2.2 Italian national landslide inventory
2.2.3 Landslide information in Spain
3 Non-susceptibility landslide models
3.1 Linear regression model
3.2 Quantile regression models
4 Optimal non-susceptibility model for Italy
5 Applications

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5.1 Size of the population of Italy living in non-susceptible areas
5.2 Non-susceptible landslide areas in the Mediterranean region
6 Discussion
7 Conclusions
Acknowledgements
References

Critique of "Method II"

The approach used in this study utilizes presence-only data of landslide occurrences without considering information from areas that have remained unaffected by landslides.

This comment gives us the opportunity to clarify a misconception common in the landslide literature. It is accepted that landslide inventory map record the location and, where known, the date of occurrence and the types of mass movements that have left discernable traces in an area (PasÌŇek, 1975; Hansen, 1984a, 1984b; McCalpin, 1984; Wieczorek, 1984; Guzzetti et al., 2000; Guzzetti et al., 2012). What is less clear is that landslide inventory maps (with very few and partial exceptions) do not say anything for the areas where landslides are not shown in the maps. These are not landslide free areas, necessarily. They are areas where landslides were not recognized or mapped, for multiple reasons. In other words, a landslide inventory map is not a binary map with (1) showing landslides and (0) showing landslide free areas. The statement made by the referee that landslide maps contain information on "areas that have remained unaffected by landslides" is not correct, necessarily. Clearly, this has consequences for the subsequent modeling.

In the case of the authors' proposed "Method II", for example, thresholds t(r) are chosen in such a way that P(S<t(r) | R=r and L=1) = 0.05 for each relative relief value r (S: slope angle; R: relative relief; L: landslide presence(1) / absence(0)

), using linear and exponential quantile regression models to model t(r) parametrically. A grid cell with relative relief R=r is considered "non-susceptible" if its slope angle S=s is smaller than t(r). By referring to these areas as "non-susceptible", the authors suggest that $P(L=1 \mid R=r \text{ and } S<t(r))$ is negligible, or at least that $P(L=1 \mid S<T)$ is negligible, where $T = t(R)$. Clearly, since the controlled quantity $P(S<t(r) \mid R=r \text{ and } L=1)$ depends only on landslide presence data (as it is conditional on L=1), this procedure does not explicitly or directly ensure that $P(L=1 \mid S<T)$ is negligible since the latter also depends on information from grid cells without landslide presence.

The rephrasing of the model attempted by the referee in terms of conditional probabilities is not fully clear to us, particularly in view of the clarification on the content of a landslide inventory given before. We do not understand why T and t(r) are equivalent, and why using both of them at all. Even if one should accept the model framed in terms of conditional probabilities, we argue that the scope of our work is not to experiment methods/models to determine landslide susceptibility. The modeling approach is more limited, and aimed at outlining areas that can be considered "non susceptible" to landslides or, more precisely, areas where landslide susceptibility is expected to be "negligible", with some degree of confidence. Our modeling approach does not say anything concerning landslide susceptibility in the areas that are recognized as "non non-susceptible". We point out that determining landslide susceptibility is a conceptually and operationally different task than outlining non-susceptible landslide areas.

The formal relationship between these two quantities can be elaborated more formally using some probability calculus, and it would seem appropriate for the authors to elaborate on these theoretical relationships in this article instead of just applying a proposed methodology. The solution to this issue would be to use, for example, logistic regression models or other models that utilize landslide presence and absence information to predict p ^ (r,s) = $P(L=1 \mid R=r \text{ and } S=s)$, where p ^ is used to denote an estimator for the probability. In order to

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identify areas that are nearly unsusceptible to landslides, a probability threshold t can be used to construct a binary classifier. The threshold t has to be selected in such a way that a high negative predictive value NPV(t) = P(L=0|p ^ (r,s)<t) is achieved, e.g. NPV(t) = 0.99, which would require a very low probability threshold that can be determined empirically (see e.g. Goetz et al., 2011 in Geomorphology, and comment 11 below).

We have responded to this comment before. Here, we reiterate that (a) we are not interested in building a susceptibility model, and associated terrain zonations, (b) we intend to determine areas where landslide susceptibility is "negligible" using a threshold base approach, (c) we do not rank susceptibility in the areas identified as "non non-susceptible". For these reasons, which we maintain are legitimate, we do not use the existing, well known statistical classification methods commonly used to assess landslide susceptibility. Also, the suggested approach would be the opposite of the present, empirical approach that we intend to pursue.

Additional detailed comments

1. Contrary to the wording chosen by the authors ("non-susceptible"), the identified areas are clearly not completely unsusceptible to landslide activity as they contain (by construction of the classification procedure) 5% of the observed landslides. Reword: "low susceptibility"

We concur with the reviewer that the term "non susceptible" can be misleading, potentially. Early in the text we now specify what we intend for "non-susceptible". In the Introduction we write, *"In this work, we propose a new model to determine "non-susceptible" landslide areas, in Italy and in the Mediterranean region, at the synoptic scale. "Non-susceptible" areas are areas where susceptibility to landslides is expected to be "negligible". Our work is conceptually and operationally different than the determination of landslide susceptibility, using heuristic (ruled-based) or statistical approaches (Guzzetti, 2005)."*.

2. P2814L19 – "Results proved" – use wording that reflects the empirical nature of the evidence.

To respond to this request, we have changed the text. The new text reads, *"Results showed that the QNL model was capable of determining where landslide susceptibility is expected to be negligible in the validation areas in Spain"*.

3. P2816L23 – If aerial photographs with scales ranging from 1:5,000 to 1:75,000 were used to inventory the landslides, there is probably an important difference in the quality of these inventories, in particular the smallest detectable landslide size.

Indeed, the quality of the inventories is of paramount importance for our work. We concur with the referee that the scale of the aerial photographs conditions the quality of the inventory, which however depends on multiple factors. When discussing or referring to the quality of the inventory in the text, we cite Guzzetti, F., Mondini, A. C., Cardinali, M., Fiorucci, F., Santangelo, M., and Chang K.-T.: Landslide inventory maps: new tools for and old problem, Earth-Science Reviews, 112, 42-66, doi:10.1016/j.earscirev.2012.02.001, 2012, who have addressed the issue in details. In Table 1, we provide detailed information on the different inventories. We also provide the references to the single works that describe the inventories. We think this sufficient information. Also, inspection of Table 1 reveals that for the sites where small-scale aerial photography was used (e.g., Umbria, 1:75,000, and Staffora, 1:40,000), other aerial photography flown at a larger scale were used to compile the inventories. This limits greatly the concern expressed by the referee.

Additional explanation seems to be needed to support the idea that all inventories are "consistent" (P2817L32; omit the claim that the sample is "significant").

We have changed the structure of the paper considerably. The description of the different sources of landslide information is now all in a single sub-section (2.2 Landslide information). We have also changed the specific text questioned by the referee. The

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new text reads, *"We maintain that the collection of the 13 inventories represents a high-quality (Guzzetti et al., 2012) and consistent landslide dataset to investigate terrain characteristics prone (or not prone) to landslides, in Italy. We base the statement on the accuracy and completeness of the single inventories, and on the fact that the inventories were prepared by the same team of geomorphologists, who have adopted the same (or very similar) photo-interpretation criteria, and have used the same (or comparable) tools to prepare the landslide maps, including the type of stereoscopes and the methods used to transfer the landslide information from the aerial photographs to the digital landslide databases. This has limited the uncertainty associated to the landslide information obtained from the inventory maps."*.

4. P2819 Eq. (3)-(5) – If I understand correctly this is a local planar approximation of topography. If this is correct, please say so, or provide other suitable verbal explanation of the procedure.

We have added a sentence to further explain the issue. The new sentence refers to the partial derivatives given by eq. (4) and eq. (5), and reads, *"These are the partial derivatives of the polynomial that approximates topography locally (Horn, 1981)"*.

5. P2819L18-19 – This description of the algorithm is difficult to follow since it is rather imprecise. What exactly is the "10% cumulative frequency of both topographic slope and relief", i.e. the 10th percentile of their bivariate distribution?

The sentence refers to the method originally proposed by Godt et al. (2012). Godt et al. (2012) did not use the bivariate distribution of R and S. In our modified version of their method, we did not use the bivariate distribution of R and S. We never stated in the text that we used such bivariate distribution. Since this part of the text was not sufficiently clear, we have rephrased it entirely. The new text reads *"To prepare our first "non-susceptible" landslide model, we modified the approach proposed by Godt et al. (2012), who were first to propose a threshold model for the zonation of "non-susceptible" landslide areas based on the use of terrain variables obtained from digital* *elevation data. Their approach is based on the empirical observation that topography conditions landslide susceptibility. Godt et al. (2012) used an aggregate of 16,000 landslide point locations obtained from inventories prepared at different scales for five geographical areas in the Conterminous United States (New Mexico, New Jersey, Oregon, California, North Carolina), and two morphometric indexes computed from the SRTM data. Using the digital elevation data, Godt et al. (2012) prepared maps of relative relief R and terrain slope S for their study area, and sampled R and S for single points representing individual landslides in their five inventories. For each inventory, they constructed an Empirical Cumulative Distribution Functions (ECDF) for the two morphometric indexes (R and S), and sampled the 10th percentile from each ECDF. The five (R, S) data pairs were plotted in a single graph, and fitted by a linear function. The linear fit was then used as a threshold model to separate terrain conditions where landslide susceptibility was considered "negligible" (below the threshold line) from terrain conditions where "some" landslide susceptibility was expected (above the threshold line) Godt et al. (2012)"*.

6. P2821L18 – "exponential model" would seem to be a more precise expression that should be used throughout the paper

Throughout the text, we have changed the way we refer to the different models. In the new section "3. Non-susceptibility landslide models" we now define the single models, and we refer to them with their acronyms i.e., (i) linear regression model (LNR), (ii) linear quantile regression (QLR), and (iii) non linear quantile regression model (QNL). We do not consider very important the fact that the model is "exponential". What is important is that it is "non-linear".

7. P2821 and elsewhere – Results should be reported in a Results section, not in the section presenting the methods.

We have changed the structure of the paper considerably, also considering this suggestion. We do not have a "Results" section anymore. Further, we now present and discuss the “models” and not the “methods”. We think this is more appropriate.

8. P2822, first two paragraphs of section 4 should be moved to the Methods section.

We have changed the structure of the paper considerably, also considering this suggestion

9. Figure 8 – According to the exponential model and using the authors’ wording, location with a relative relief of 1000 m and a slope angle of 40 degrees or higher would still be considered “non-susceptible”. The greater the relief within a 500 m radius, the steeper must be the slope to be considered susceptible to landslides, according to this study. It is difficult to see how this would be consistent with physical landslide processes as expressed by e.g. infinite slope stability models; e.g. the SHALSTAB model of Montgomery & Dietrich (1994, in Water Resources) predicts unconditionally unstable conditions when the slope angle exceeds the friction angle (typically smaller than 40 degrees), and conditionally unstable conditions at a smaller slope angle. These process models are typically only based on slope-scale variables (slope angle, upslope contributing area), and it is therefore surprising that the present empirical models consider relative relief within 500 m of horizontal distance as one of two predictors of landslide susceptibility.

We concur with the referee that our QNL model predicts that a larger regional relative relief (R) requires a steeper slope (S) for an area to be considered susceptible to landslides. This is evident from the visual inspection of our Figure 8. However, we do not agree with the referee that this is surprising. Indeed, abundant evidence exists (e.g., Korup, O.: Distribution of landslides in southwest New Zealand, Landslides, 2(1), 43–51, 2005; Guzzetti, F., Ardizzone, F., Cardinali, M., Galli, M., Reichenbach, P. and Rossi, M.: Distribution of landslides in the Upper Tiber River basin, central Italy, Geomorphology, 96(1-2), 105–122, doi: 10.1016/j.geomorph.2007.07.015, 2008) that in landscapes landslides are less abundant (and not more abundant) on steep slopes, and that landslide density increases with slope gradient up to a threshold, above which the landslide density decreases, more or less rapidly. Guzzetti (2006) has stated, “This is a typical condition in the central Apennines, and elsewhere (Iwahashi et al., 2003). The abundance (area) of landslides, and in particular of deep-seated slides and slide-earth flows, increases with increasing terrain gradient up to a maximum value, where landslide area is most abundant, and then it decreases rapidly with increasing slope. Reasons for this behavior are found in the relationship between lithology, strength of the rocks, and slope instability.” Also, our synoptic scale, threshold-based, non-susceptibility models is very different from the deterministic model SHALSTAB of Montgomery & Dietrich (1994), or other similar deterministic models. The scopes and “backgrounds” for the two types of models are very different. The size of the areas investigated by the two types of models is also very different. Our model is synoptic, and constructed for the whole of Italy. The deterministic models have been successfully applied in areas of limited extent, from less than a square kilometers to some hundred of square kilometer. Most of the deterministic models (including SHALSTAB) adopt (implicitly or explicitly) the “infinite slope” modeling approach. It is quite difficult to sustain that the “infinite slope” modeling approach captures well the “physical landslide processes” controlling medium-to-large landslides, which are characterized by complex geometrical / lithological / hydrological settings not considered by these models.

10. P2823L14-25 should be moved to Methods section

We have re-structured the text significantly, and the section was moved to the new Section 4.

11. P2823L16-18 Three comments of fundamental importance: a. This description of the index “I” is confusing due to partly unconventional terminology. E.g. if Nc is the “number of [observed?] landslide cells” and Lc is the “number of [observed?] landslide cells that overlaid [predicted?] non-susceptible cells”, then the index I is false negative rate (FNR), or one minus the sensitivity. This is not

the false positive rate, as suggested by the authors, unless my interpretation of L16-18 is incorrect. I don't think the new and misleading term "matching index" should be introduced for this quantity since matching index sounds very much like the overall accuracy, which is a different measure of model performance.

We do not think that use of the "matching index" I represents a case of use of "partly unconventional terminology". The index was used by (i) Carrara, A., Cardinali, M. and Guzzetti, F.: Uncertainty in evaluating landslide hazard and risk, ITC J, 172–183, 1992; (ii) Ardizzone, F., Cardinali, M., Carrara, A., Guzzetti, F. and Reichenbach, P.: Impact of mapping errors on the reliability of landslide hazard maps, NHESS, 2(1/2), 3–14, 2002; and (iii) Galli, M., Ardizzone, F., Cardinali, M., Guzzetti, F. and Reichenbach, P.: Comparing landslide inventory maps, Geomorphology, 94(3-4), 268–289, 2008. Also, we do not think that – unfortunately – an accepted terminology exists. A paper published recently by Peres and Cancelliere in the journal HESS-D – Hydrology and Earth System Sciences, Discussion (www.hydrol-earth-syst-sci-discuss.net/11/2759/2014/, doi: 10.5194/hessd-11-2759-2014) tackles the problem, and has prompted a discussion on the use (or misuse) of the terminology. Also, we do not agree that "matching index" measures overall accuracy. In the text, we specify clearly how to calculate I. Specifically, we write *"To quantify the differences, we used the matching index I (Carrara et al, 1992; Galli et al., 2008), which measures the overall proportion of landslide cells that overlay non-susceptible cells in each landslide map i.e., I = Oc/Nc* $\times$ *100, where Oc is the total number of landslide cells overlaying non-susceptible cells, and Nc is the total number of landslide cells. The matching index (I) is equivalent to the False Positive Rate (FPR), the ratio of the False Positives (FP) over the sum of the True Negatives and False Positives (TN + FP)"*

We stress that our work is meant to decide on "non-susceptible" areas, and not on the "susceptible" areas. As explained before, the task of determining landslide susceptibility is conceptually and operationally different from the task of outlining non-susceptible landslide areas. We have added and changed language in the text in several places to explain the difference. Since our work aims at defining "non-susceptible" areas, a True Positives (TP) is an area predicted as "non-susceptible" and where landslides were not observed. A False Positives (FP) is an area also predicted as "non-susceptible" but where landslides were observed. True Negative (TN) and False Negatives (FN) respectively are areas predicted to be "non non-susceptible" and where landslides were and were not observed. This is the opposite than what is normally done when assessing landslide susceptibility. We maintain that this is the ("obvious") consequence of the fact that our model attempts to predict the "non-susceptible" areas. We have added language in the Discussion to clarify the issue, adopting the "conventional" terminology proposed by the referee. The text reds, *"The difference has consequences on the measurement of the performance of the threshold model. When measuring the predictive performance of threshold-based non-susceptibility landslide models , correct predictions are areas predicted as "non-susceptible" where landslides were not observed (True Positive – TP), and areas predicted "non non-susceptible" where landslides were observed (True Negative – TN). Incorrect predictions are areas predicted as "non-susceptible" where landslides were observed (False Positive – FP), and areas predicted as "non non-susceptible" where landslides were not observed (False Negative – FN). This is the opposite to what is most commonly done to evaluate the performance of landslide susceptibility models (Rossi et al., 2010)". Using this terminology, and conserving the context of the work, the text now reads, "The matching index (I) is equivalent to the False Positive Rate (FPR), the ratio of the False Positives (FP) over the sum of the True Negatives and False Positives (TN + FP)."*

b. Furthermore, a good sensitivity can easily be achieved by setting a very low decision threshold, i.e. considering most of the study area as unstable. The use of the sensitivity for classifier comparison therefore only makes sense if its counterpart, the specificity, is held constant. The authors in fact seem to be holding the sensitivity constant at 95% in Models QLR and QNL as they perform quantile regression for the 5th percentile of the data distribution of landslide locations.

Based on what we have stated above on the completeness of the inventories, and on the fact that our model aims to define “non-susceptible” areas (we can’t say nothing about the “non non- susceptible” areas), we cannot calculate either FN nor TP, and hence we do not have a way to estimate the Sensitivity. We can estimate the Specificity, the complement to 1 of the FPR (our matching index). This is what we do in the paper.

c. Finally, since the article claims that the identified areas are not susceptible to landslides, the authors should report the negative predictive value (NPV) as a measure of their ability to achieve this goal. The NPV is the proportion of the predicted non-susceptible area that was observed to be unaffected by landslides. As with the sensitivity, the NPV will vary with its counterpart, the positive predictive value (PPV). Perhaps the authors should consider specifying a desired NPV value in advance (e.g. 0.99 or 0.999), and to choose a decision threshold (or percentile, in the case of quantile regression) that is high (low) enough to achieve this NPV; PPV can then be used as a performance measure at the fixed NPV.

Using our approach, the reviewer NPV is in effect the positive predictive value (PPV=TP/(TP+FP). Since we can’t estimate the TP we also cannot estimate the PPV.

12. P2824L1-3 (and entire Results section): With a false negative rate of 6% in the QNL and QLR models, the identified area is not exactly “non-susceptible”, but it perhaps has a low susceptibility (as expressed by the NPV, see previous comment). The index I does not provide sufficient evidence for a low susceptibility.

Throughout the text, we now consider “non-susceptible” areas where the susceptibility is expected to be “negligible”. Thus, we acknowledge that a (minimum) level of susceptibility is expected. This is a result of how the threshold model was constructed. The quantile regression model was instructed to keep 5% of the empirical data points below the threshold. The matching index I provides a quantitative measure of the performance of the threshold model. Considering all the uncertainties associated to

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the analysis, a value of I = 6% indicates that the threshold model performed (almost exactly) as it was supposed to perform.

13. P2824L4-5 "too conservative", based on what, and in what sense? Can a model be "too conservative" if the goal is to identify "non-susceptible" areas? (See one of the previous comments regarding trade-off between NPV, PPV.)

We do not understand this comment, fully. Any model can (and will) be "conservative" or "liberal", or using different words "optimistic" or pessimistic", when compared to other models. In our case, even the simple visual inspection of Figure 7 reveals differences in the general performances of the three models. The linear model LNR classifies as non-susceptible a very small portion of the territory (only 21.9%). It is therefore very conservative. A user adopting this model will take little chances of saying than an area is non-susceptible, because most of the territory (78.1%) is defined as "non non-susceptible" by this model. We added language in the text to address this argument. The text reads, *"Fig. 7 compares the three non-susceptibility threshold models for Italy. The QLR model (map II in Fig. 7) classifies as non-susceptible the smallest portion of the territory (21.9%), followed by the QNL model (map III in Fig. 7) (41.9%), and by the LNR model (map I in Fig. 7) (61.9%). The differences are significant. Visual inspection of Fig. 7 explains the differences, and suggests that the QLR model is too pessimistic (conservative), because it does not recognize as non-susceptible large regions in flood plains and other flat areas where landslides are clearly not expected (Fig. 7 II-b). Conversely, the LNR model is too optimistic (liberal), because it classifies as non-susceptible large parts of the slopes where landslides are expected (Fig. 7 I-b). The QNL model classifies as non susceptible the flood plains and other flat areas, and classifies correctly most of the sloping terrain. Overall, we consider the QNL model (Fig. 7 III-b) neither pessimistic nor optimistic."*

14. P2824L14-23 and P2825L14-25 should be moved to the Methods section.

We have changed the structure of the paper considerably, also considering this suggestion. Part of the text was moved to the new Section 4 part to the new Section 5.

15. P2826L3-4 and L14-15: The presence of landslide susceptible terrain somewhere within a census region does not imply that its population is located in susceptible areas; it may be located in non-susceptible areas within the census region. Rephrase or omit; these statements don't seem to be important since the focus is on the prediction of non-susceptible areas.

We agree with the referee that the presence of landslide susceptible terrain in a specific census zone does not imply that the population is located in a susceptible area, necessarily. The sentence in the text was misleading, and we removed it. We point out that – to the best of our knowledge – census information for single census zone is the best information available on the geographical distribution of the population at the national scale, in Italy. For specific areas, higher resolution information on the geographical distribution of the population exists, but (i) it is available for very limited urban areas, and (ii) it was not available to us. We maintain that any study that attempts to establish an impact of a hazard (in our case "landslides") on the population of Italy, will have to use the census information provided by the National Institute of Statistics – ISTAT. We acknowledge that this information can be of "coarse" geographical resolution in rural and mountain areas. Indeed, this was a further motivation for us to attempt to determine areas "not-susceptible" to landslides, as oppose to "susceptible" areas. We note here that for e.g., insurance and re-insurance companies the information provided by the application of the model i.e., that 33.1-million people (in 2001) lived in the non-susceptible census zones, 57.5% of the total population, is a valuable information that was not available before this study.

Same applies to L19 ("people living . . . in susceptible areas"), which may explain the lack of correlation in L20-21 and the referenced Fig. 5. Also rephrase P2830L12 and P2814L16 accordingly.

The text was amended, accordingly.

16. P2826L25-27: I.e. conversely 27% of the fatal landslide events occurred in the areas identified as by the authors as "non-susceptible"? Consequently, if I understand this correctly, the identified "non-susceptible" areas are in fact quite susceptible to landslide events.

Indeed, 27% of the fatal landslide events in the period between 842 AD and May 2013, and 30% of the fatal landslide events in the recent period between 1963 and 2012, occurred in areas classified as "non-susceptible" by the model. We are not surprised by this result, for a number of reasons. First, fatal landslides in Italy are predominantly rapid to fast-moving landslides (e.g., rock falls, minor rock slides, soil slips, debris flows) (see Guzzetti, F., Stark, C. P. and Salvati, P.: Evaluation of flood and landslide risk to the population of Italy, Environmental Management 36(1) 15-36, 2005). With a few exceptions, slow moving translational and rotational slides, earthflows, and complex and compound movements do not cause fatalities. The later, are the predominant types of movements in the regional inventories used to construct the non-susceptibility threshold model. Second, fatal landslides can be very small, and controlled by local terrain conditions that are not captured by the morphometric information used to construct our model. Third, some of the fatal landslides travel significant distances from their source areas, and into areas where terrain is gentle and relief is limited (e.g., debris flows depositing on debris fans, or rock falls reaching the alluvial plain). Fourth, no information on the vulnerability to landslides of the population was used to construct the non-susceptibility model. And lastly, landslides and their direct human consequences are very difficult to predict. Overall, we maintain that a synoptic-scale model capable of determining where 73% (70% for the most recent period) of the fatal landslide events have occurred i.e., in areas considered "non non-susceptible", is a valuable model. We have added language to the text to address the issue. The text reads, *"We consider this a good result, for the following reasons. First, fatal landslides in Italy are predominantly rapid to fast-moving landslides, (e.g., rock falls, minor rock slides, soil slips, debris*

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flows) (Guzzetti et al. (2005). With a few exceptions, slow moving translational and rotational slides, earth flows, and complex and compound movements do not cause fatalities, frequently. The later, are the predominant types of movements in the regional inventories used to construct the non-susceptibility threshold model. Second, fatal landslides can be of very small size, and controlled by local terrain conditions that are not captured by the morphometric information used to construct our non-susceptibility model. Third, fatal landslides can travel significant distances from their source areas, and into areas where terrain is gentle and relief is limited (e.g., debris flows depositing on a debris fan, rock falls reaching the alluvial plain). Fourth, no information on the vulnerability to landslides was used to construct the non-susceptibility model. Lastly, the human consequences of landslides are very difficult to predict. We maintain that our synoptic-scale model, capable of determining where 73% (70% in the most recent period) of the fatal landslide events have occurred (i.e., in areas considered "non non-susceptible") is a valuable model.".

17. P2827L2-8 and L12-18 should be moved to the Methods. I would expect the authors to provide a justification for the application of the proposed models beyond the calibration area.

We have changed the structure of the paper considerably, also considering this suggestion.

We do not understand why a model (in this case our non-susceptibility model) should not be applied "beyond the calibration area". The main scope of construction a model is to apply it for predictive purposes (see eg., Michie, D., Spiegelhalter, D.J., Taylor, C.C. (eds.). Machine Learning, Neural and Statistical Classification, 1994, available at http://www.amsta.leeds.ac.uk/ ~ charles/statlog/.

18. P2828-9 The Discussion is rather limited and does not sufficiently discuss the present results in the context of the broader literature. E.g. advantages and disadvantages of the proposed methodology compared to other approaches for

landslide susceptibility modeling, e.g. using classification methods (see Critique of Method II above) or process-based models (see comment 9.)

We have considered this comment very thoroughly. As a result, we have expanded the Discussion to include a description of the differences between our modeling approach to determine non-susceptible areas, and other well-known approaches to assess landslide susceptibility. Here, we clarify that our work is not meant to find an alternative approach to determine landslide susceptibility. As explained before, the task of determining landslide susceptibility is conceptually and operationally different from the task of outlining non-susceptible landslide areas i.e., areas where landslide susceptibility is expected to be negligible. We have added and changed the language in the text to explain the difference. Our response to the first comment of the referee lists the amendments and additions made to the text to clarify the issue.

19. P2830L19-21: To the contrary, the authors should advise against the use of the present results for insurance or reinsurance purposes if the information provided on P2826L25 is correct, according to which 27% of the fatal landslide events occurred in the areas identified by the authors as "non-susceptible".

We disagree with this last statement of the referee. As specified before, and in the revised text *"We maintain that our synoptic-scale model, capable of determining where 73% (70% in the most recent period) of the fatal landslide events have occurred (i.e., in areas considered "non non-susceptible") is a valuable model"*, particularly for insurance and re-insurance companies. Although, the performances of our threshold model are far from being "perfect", we are not aware of any other model that outperforms these performances. We maintain that for insurance or re-insurance companies knowing where landslides are not expected (i.e., where susceptibility is expected to be "negligible") is very important. And knowing that in the "non-susceptible" areas fatalities can occur, is even more important as it helps defining – quantitatively – the risk taken by the insurer / re-insurer when selling insurance. Further, insurance and re-insurance companies are not interested in landslide fatalities (or casualties) only. Indeed, they are interested in covering losses caused by landslides to e.g., structures, infrastructures, private and personal properties, the agriculture. For these purposes, our model can be useful.

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