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Interactive comment on “From slope- to regional-scale shallow landslides susceptibility assessment using TRIGRS” by M. Bordoni et al.

Anonymous Referee #2

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The paper describes a method adopted to estimate the applicability of the well-known TRIGRS (v. 2) stability model for landslide susceptibility assessment in a study area in northern Italy. The Authors followed an interesting procedure to establish the reliability of the model under the conditions given by a number of monitored rainfall events occurred in the area over a two-year time span. The monitoring was performed on a sample slope with great detail in determining the soil's geotechnical and hydrological parameters and directly measuring soil moisture and pore pressure at depth. All of these are key quantities in the TRIGRS model, so that their accurate evaluation allowed the Authors to determine the landslides triggering conditions in the sample slope for one benchmark event, to determine the best parameter zoning strategy, namely a pedological zoning instead of a geological zone, and to conclude that similar conditions

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must be applied to the larger study area containing the sample slope.

The paper has the merit of investigating the applicability of the model, establishing that the landslide triggering is due to the formation of a perched water table in partially saturated condition, so that the TRIGRS-Unsaturated model is suitable for the event simulations performed in the area. This fact is often overlooked in the literature and it represents a solid starting point for the proposed analysis. Moreover, it is explicitly mentioned that "the choice of the more suited method to describe the phenomena [...] depends on the objectives of the analysis" is also a valuable approach, again often overlooked or not discussed at all. The Authors also verified the actual connection between the water content, pore pressure dynamics and rainfall trends on a sample slope. That said, I would like to make a few remarks, first on particular items discussed in the draft and, eventually, on the general level and about the conclusions of the manuscript.

In the introduction, the paragraph in pg. 7411 line 25, from "In particular" to pg. 7412 line 9, until "rainfall event" seems to be unnecessarily long, and containing many unneeded information such as the relation of evapotranspiration and infiltration and corresponding references; a reference to Godt, J. W., R. L. Baum, and N. Lu (2009), Landsliding in partially saturated materials, *Geophys. Res. Lett.*, 36, L02403, doi:10.1029/2008GL035996, where the Authors "observed shallow landslide occurrence under partially saturated conditions for the first time in a natural setting" and a comparison between measured/predicted change in time of hydrological parameters was performed, seems appropriate in the citation block going from Montgomery and Dietrich, 1994 to Grelle et al, 2014 (are all these references really necessary? Can the Authors specify somehow which one is relevant to what particular aspect of the present discussion?)

Pg. 7412, line 19-21, the Authors may consider referring to another recent study over a regional scale with TRIGRS where the size of predicted landslides is also analyzed: Alvioli, M., et al. (2014), Scaling properties of rainfall induced landslides predicted by a physically based model, *Geomorphology* 213, 38-47,

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Pg. 7412, line 25-26: a recent study on a large-scale spatial generalization of field tests results for stability assessment with a quantitative model is also probably worth citing: Mergili, M., et al. (2014), A strategy for GIS-based 3-D slope stability modelling over large areas, *Geosci. Model Dev.*, 7, 2969-2982, doi:10.5194/gmd-7-2969-2014 and same comment for: Simoni, S., et al., (2008), Modelling the probability of occurrence of shallow landslides and channelized debris flows using GEOtop-FS, *Hydrol. Process.*22, 532-545, doi:10.1002/hyp.68

Pg. 7412, from line 27: among the methods to infer the input parameters for stability assessment accounting for spatial variability, it is worth mentioning the probabilistic approach, discussed and implemented in TRIGRS in: Raia, S., et al (2014), Improving predictive power of physically based rainfall-induced shallow landslide models: a probabilistic approach, *Geosci. Model Dev.*, 7, 495-514, doi:10.5194/gmd-7-495-2014 and references therein. The Authors say that they average the values of their measurements, and nothing is said about the behavior of the model when values around the mean are used; this would be the benefit of a probabilistic procedure, also intended to extend physically-based models from small to large areas.

In the same paragraph, I found rather misleading the use of "mapping units" when referring to the method of defining the different zones with homogeneous parameters. As a matter of fact, "mapping units" may be easily misunderstood, and in the rest of the paper the Authors refer to the same concept as "unit mapping of the soil", which is probably the best choice, even if I personally would use something different.

Pg. 7413, from line 11 to the end of the paragraph: I believe this paragraph should be written more carefully; first of all, there is one whole sentence repeated twice; the points i)-iv) starting at line 27 actually read i-ii-iii-iii; finally, the main purpose of the paper, given in lines 11-13, should be probably stressed earlier in the introduction.

In section 2, pg. 7414, line 14: the general definition of conti-

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mental climate doesn't seem to fit the study area, shown e.g. at: http://en.wikipedia.org/wiki/K%C3%B6ppen_climate_classification where Italy seems to be in the greenish-yellowish climate regions, general class C, defined as "temperate/mesothermal"; "continental" is definitely somewhere else. In section 3.3, pg. 7424, Eq. (2): it is not clear, so far, where does the index i run, not even if the different observations are at different places or at different times or both.

Throughout the paper: the use of " $m\ m^{-1}$ " to denote a dimension-less quantity is rather misleading, or at least uncommon in the literature. I think some other way should be found to denote a ratio, if this was the purpose of the notation.

On the general level, I have the following comments. In the paper, they Authors walk their way in showing that an accurate, long-term monitoring of a sample slope is key to determine the shallow landslide triggering mechanism at work during rainfall events - which certainly is true - and that these findings can be extended to a larger area. I find misleading defining the latter as the main purpose of the paper, for the 13 km² area cannot really be defined as a "regional scale" area. More importantly, about the method itself, it should be stressed that in many occasions the Authors mention that they "assume" the validity of their findings on the sample slope to be true on the rest of the study area as well. They define different zones, both on a pedological and a geological basis, for the spatial homogenization of the model's input parameters. The sample slope is located in one particular zone, and nevertheless "can represent in a good way the geotechnical and hydrological features of the slopes [...] in the whole study area", which seems rather arbitrary or, at least, not a robust method to assess the stability at the "regional scale".

Digging into the details of the procedure followed in the draft, we find an extension from the test site to the larger area of the depth of the water table according to Eq. (4), which seems a reasonable assumption after section's 4.2 discussion. This extension seems the most quantitative step, and I think this is again not enough to define a general "slope- to regional-scale" extension method. The remaining model input parameters

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were still to be measured, admittedly in 160 other different locations, averaged to find the final parameters. It would be nice to see the distribution of these measurements, their standard deviation, how the result change when they are changed within the observed distribution. moreover, throughout the paper it is stated that the model was "calibrated" in the test slope, which is inconsistent with the parameters being actually measured in the whole area. What is shown is more an analogy between the monitored site rather than some local quantity being used to infer something else for the whole study area. The use of two different DEMs, with different resolution, in two separate places is also mysterious; why not using the same DEM at least in one common place, to check whether the two results are reliably consistent? Moreover, it is shown in Fig. 11 that the scarps modeled with the higher resolution DEM on the test site fall on pixels modeled as unstable: as a matter of fact, the number of unstable pixels are the majority of the slope, how do the Authors comment on the many false positives?

In view of the comments above, in the Discussion and conclusions section I find a few unjustifiable statements. First of all, I don't find support for a method that keeps "the same level of reliability at different scales, both on a single slope and on an area some square km". Then, as already mentioned, no real calibration of the model, with all its input parameters, has been performed, in addition to inferring that the unsaturated condition and the presence of a perched water table are most probably present in the whole study area. The remaining conclusions about a) the performance of the calculations b) the comparison with the results of other models (even if with different initial conditions) c) the discussion about the "necessity of verifying the reliability of a physically-based model before its implementation" (I would avoid the definition of TRIGRS as a "susceptibility model" throughout the paper, actually) are in my opinion substantiated in the manuscript. In conclusion I think that, both in the title and in the text, the statements referring to a "slope- to regional-scale" extension of the applicability and "calibration" of TRIGRS should be rephrased in a way consistent with the many assumptions made in the presented simulations, in addition to addressing the other questions raised in my review. I also think that many parts of the manuscript, for example the details of

the measurements techniques and of the TRIGRS model itself, may be relegated in an appendix for an improved readability of the manuscript.

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