

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

**Anonymous Referee #1**

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The paper is basically a well-documented modelling case-study. Data quality is high, data analysis is accurate, and the application of the model is correct. Nonetheless, the overall impression on the paper is weak, due to a general lack of something new and interesting for the scientific community. In other words, the paper does not really add something new to the Literature, except the case study itself.

1 - The paper claims, starting from the title, that a new RELIABLE methodology is proposed to pass from local to regional scale susceptibility analysis. This is not true. First of all, the paper is not proposing any susceptibility mapping. The result of the models is a back-calibration of the April 2009 event, which is not a susceptibility map, because it is related to a specific rainfall input, and does not consider other possible conditions. Hence, the final result is a simulation map, and not a susceptibility map. Second, I tried

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to understand in what consists the transfer from local to regional scale, but I really did not understand it. The models of the two scales are developed independently (with the exception of the depth of the perched water table, I'll discuss it below), and basically the local scale model represents a sort of verification (test) of the mathematical model. Indeed, the authors claim that the local scale analysis is used to calibrate the model, but also this point is not true, since a real calibration is not done (the parameters are obtained from laboratory tests and from pedotransfer functions, and used in the model as is, without a tuning of these parameters to improve the best fit: this could have been a calibration procedure).

2 - A second big issue regards the interpretation of the hydrological processes and the implication for regional scale modelling. From the analysis of the monitoring data, it seems quite clear that the soil becomes nearly saturated in winter period, until April. Interestingly, it seems that the sensitivity to single rainfall events at depth increases significantly when the soil is nearly saturated, because in summer period the deeper layers seem almost insensitive to single rainfall events, even if significant in intensity. This behavior should be the effect of the transfer of pore pressure in saturated condition, as described by Iverson (2000). One of the conclusions of the authors is that there is a thin perched water table above the contact soil-bedrock, as testified by pore pressures at 1.2 m bgl in the monitoring site. I have some problems about it. First, the fact that the soil is saturated and that the soil water content is higher in the soil with respect to bedrock does not demonstrate that the bedrock itself is not saturated. In other words, groundwater can extend well below the soil-bedrock contact. We don't know where the main groundwater is located, because this has not been investigated, but it could be possible that the main groundwater up-rises in winter time up to the surface (is there any spring in the area? How far is the river?). This means that there is no proof that the saturated zone is actually a THIN perched water table, and not the main groundwater. In other words, the hydrology of infiltration is studied independently from the overall slope hydrology, and this is a limitation for the understanding of what really happen in the soil. Another observation could support the hypothesis of a main

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groundwater. Observing the data, it is clear that the saturation of the deeper levels occurs in autumn, quite rapidly, and independently from single peaks of rainfall. This suggests that the formation of this saturated deep zone is not related to local vertical fluxes, but to a more regional (and seasonal) lateral flow of the groundwater. The implication of this is very important. If the position of the saturated zone is not related to a perched water table (controlled by local vertical fluxes of rainwater) but rather by the up-rise of the main groundwater due to lateral recharge from upward contributing area, then the position of this saturated zone would be different in different position of the slope, and not always located 0.1 m above the soil-bedrock contact. This hypothesis is furtherly supported by the field observations of the distribution of actual landslides in 2009 at some specific morphological position. Hence, the only contribution that local scale modelling and monitoring offers to regional scale analysis (the position of the perched water table 0.1 m above the soil-bedrock contact) could be false. In any case, to be better investigate.

3 – the analysis completely neglects the uncertainties of the parameters. It is well known that the parameters are extremely variable in space (and in time, sometimes). Part of the spatial variation of the parameters is accounted in the analysis by distributing the parameters values according to mapping units, but even within each mapping units the parameters can significantly vary. I do not claim that the authors should have done a full stochastic model, but I expect to have some ideas about the degree of uncertainties, and the effect of this uncertainties on the modelling (sort of sensitivity analysis). For instance, the soil properties are derived from testing 160 samples (it is not clear if all tests has been done on all samples, for instance triaxial test. . .), and the mean values have been applied to each unit. Nothing is said about the standard deviation of these measures within each unit, which could be interesting to understand if dividing the area in mapping units really helps. I'm afraid, in fact, that the standard deviation is higher than the difference about the parameters among the units. Another problems regards the simulation of rainfall events with TRIGRS used for validation (not calibration) (Fig. 8 and 9). Here, it is quite difficult to understand how good is good.

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I mean, TRIGRS simulates an increase of soil moisture with rainfall, but this is trivial. What could have been the results with different parameters? Maybe nothing would have changed. Maybe a different set of parameters could help improving the fitting, which is not pretty good, at the end, especially in the timing of the maximum variation of the soil moisture. The authors are satisfied because the mismatch is within 2/3 kPa. Yes, but also the variation has the same range, thus the error is more than 100 % of the variation. Is this a good result? Again, some kind of sensitivity analysis could support the discussion about the results of these simulations.

4 – the quality of the model is evaluated by using the success index (SI) and error index (EI) measures. Indeed, it seems that these measures corresponds to the well-known True Positive rate (TP rate) and False Positive rate (FP rate), respectively. And the plot with FP rate in x-axis and TP rate in y-axis is the ROC plot. I don't understand why the authors uses different terms with respect to the established literature. My only doubt regards the way the authors define the SI. They claim that the SI is the ratio between the pixels with landslides and the pixel simulated as unstable. This is a non-sense, because the smaller is the number of pixels simulated as unstable with respect to the landslide pixels, the higher would be the success. I guess that the authors meant the inverse. However, also the inverse is not a good success measure, because it does not specify that the pixels simulated as unstable need to be CORRECTLY classified as unstable. In this way, the measure is a real success measure, and corresponds to the TP rate, eventually.

5 – the English needs to be improved significantly. I'm not a native English speaker, but I'm familiar enough with English to recognize an overall sloppy presentation, with many redundant sentences, and some imprecise terminology.

In conclusion, in order to improve the paper to a level that is acceptable for NHES, it is necessary to redefine the focus of the paper, to improve the interpretation of the hydrological process, to introduce some critical analysis of the sensitivity/uncertainty, to improve the discussion.

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