Answer to Reviewer No. 3 of:
Modelling catchment-scale shallow landslide occurrence by 1 means of a subsurface flow path connectivity index, by Lanni et al.

June 29, 2012

Introduction

We thank the Reviewer#3 for the revision of this manuscript. Accordingly with his/her general comments we introduced new elements in the revised version which are related to: i) the description of the shallow landslides archive, ii) the intercomparison with the QD SLaM quasi-dynamic model, and iii) the statistical intercomparison between the models and the surveyed landslides.
We hope that this effort will improve the manuscript, by strengthening the weak points highlighted by the Reviewer.
We tried to answer here every comment in detail, although issues of typographical or editorial illustrations and tables are not incorporated here, but have been modified in the revised version of the manuscript.

Summary of the major points raised by the Reviewer #3

Comments

Comment 1:
The proposed approach is a very complex way to obtain susceptibility map. Then the question to address by the authors should be: what is improved compared to a classical map overlay susceptibility map?

Response:
We argue that the model is suitable to define a map of shallow landslides susceptibility. It is now well accepted that shallow landslide triggering is preliminary due to increase in pore-water pressure induced by subsurface hydrological processes (e.g., Iverson, 2000). The subsurface flow paths that link source areas to a generic point in the hillslope control the development of pore-water pressures at that point and the local value of the factor of safety, FS. These subsurface flow paths are spatially variable and temporally dynamic. Even if two locations have the same mechanical (i.e., cohesion and frictional angle) and geomorphological (i.e., upslope source area, local slope, aspect, curvature, etc.) attributes (that are normally used in classical map overlay to derive shallow landslide susceptibility map) there may be a substantial difference between them in terms of shallow landslide susceptibility, depending on whether the point hydrologically connects with its own upslope source area.
Thus, it is crucial to enhance our ability to represent subsurface hydrological connectivity in shallow landslide modelling, in order to be able to better delineate those areas most prone to shallow landsliding. This is exactly the purpose of our paper.
Concerning the request to quantify ‘what is improved compared to a classical map overlay susceptibility map’, the revised manuscript includes i) a discussion about the value of the current modeling approach with respect to statistically-based approaches, and ii) an intercomparison with the QD_SLaM model (Borga et al., 2002; Tarolli et al., 2010), which is based on the same quasi-dynamic approach but which doesn’t include the unsaturated modeling part.

Comment 2:
The weakest point of the paper is the comparison with field data. Figure 4 shows typical landslide due to undercutting or stream erosion activity and typically these landslides are too large for

shallow landslides. Why are there no landslides in the upper ranges of the catchment? I doubt strongly if this data set can be used to validate your model or even qualitatively indicate the model works for the right reason.

Response:

The study area includes both hillslope failures due to fluvial erosion and shallow landslides due to pore pressure build up. Our landslide map reports only the areas which are impacted by the latter type of landslides. The landslide inventory described in this work is part of a more comprehensive archive of shallow landslides which has been described in other papers as well (Borga et al., 1998, 2002a,b, 2004; Tarolli et al., 2008, 2011) and executed with a common surveying methodology. We agree with the reviewer that the landslide scars indicated in Fig. 4, include both the landslide initiation point and a portion of the run out path (indeed, most of the landslides considered in the work evolved as debris flows). However, in order to evaluate the performance of the presented model, the map of Figure 5 in the original manuscript has been only compared with the landslide initiation points reported below (Figure 1 of this document). To avoid confusion, the revised manuscript includes the map with the initiation points in place of those reported in Figure 4.

No landslides were found in the upper part of the catchment, which is characterized by a smoother topography and by shallower soils. Correspondingly, the model shows that landsliding susceptibility is concentrated in the lower portion of the catchment.

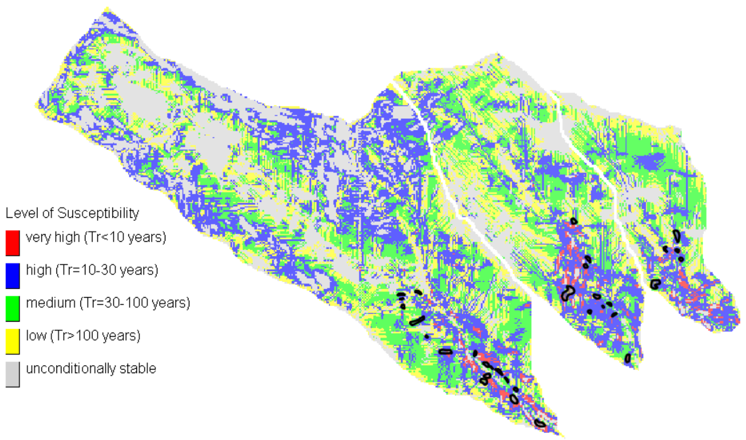

Figure 1: Model results with the landslide initiation points used to evaluate the model performance

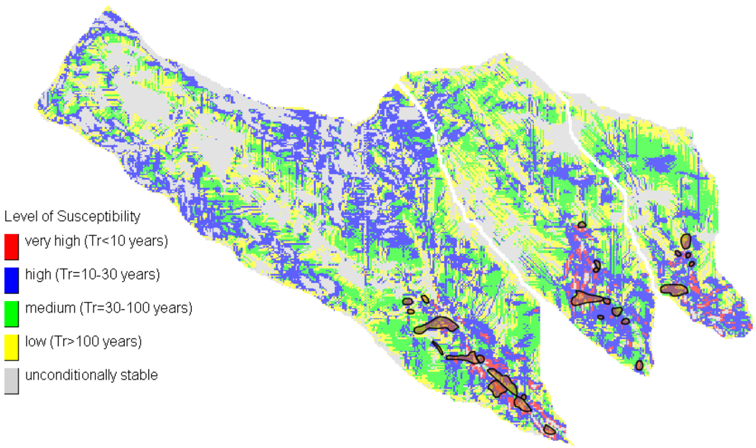

Figure 2: Model results with total landlslide areas due to pore pressure increase. These areas include both the initiation area and the a portion of the run-out path

Comment 3:
Comparing your model results with a debris flow data set is doubtful. Are debris flows generated in the same way as shallow landslides? Why would the proposed method to assess shallow landslide susceptibility be validated with a debris flow data set?

Response:
We agree with the Reviewer#3 that the use of the term Debris Flows is not appropriate here, since we are focusing on shallow hillslope instabilities. The inappropriate use of the term is due to the fact that, as reported above, most of the landslides observed in the study area eventually evolved as debris flows. This statement has been corrected in the revised version of the work.

Comment 4:
I do not really understand Table 2. But it seems that the observed landslides are in 0-10y and 10-30y return period classes. But how often are you wrong? A more standard way of showing how well your method is doing is by a kind of success rate, prediction rate method (see for example Chung and Fabbri 2003, Validation of spatial prediction models for landslide hazard mapping, Natural Hazards, 30 (2003), pp. 451–472).

Response:
As a response to this question we extended the intercomparison between model results (obtained by the two models: see our response to Point 1) and surveyed landslides. The intercomparison procedure, which is a generalization of the assessment method reported in the original manuscript and has been introduced by Borga et al. (2002a), is based on comparing the proportion of catchment area which fall beneath various thresholds of instability-predicting variables (the severity of the critical rainfall) to the corresponding fraction of the landslide area. To compare the two models, the threshold needs to be set so that the same percentage of terrain elements falls beneath the threshold. The two sets of mapped unstable regions, resulting from the application of the models, will be partially overlapping, but will not be the same. Then the percentage of observed landslide area within the region is computed for each model. The model with the higher percentage provides a better prediction of landslide hazard. The assessment is repeated for various thresholds. Note that the methodology implicitly assumes that the value of the index variable is decreasing for increasing hazard, as it is for the severity of the critical rainfall. The significance of the differences arising between the two model outcomes is quantified in statistical terms. Also, the revised version shows how this methodology relates to published procedures for the validation of landslide susceptibility models, such as the one proposed by Chung and Fabbri (2003).

Comment 5:
The spatial variability of the soil depth is a critical parameter in your model, but it is determined in a quite rudimental way associated with quite some uncertainty. There are 49 points on 7.5 km2 or one point per 0.15 km^2. Could you assess the uncertainty and indicate the effect of these errors in the susceptibility map. Could you compare or at least discuss this soil thickness estimation with different techniques to estimate the soil thickness.

Response:
As a response to this point, we extended in the revised manuscript version the section dedicated to the assessment of the relationship between soil depth and local slope. Moreover, we evaluated the uncertainty when generalizing the relationship to ungauged topographic elements. In the revised version of the manuscript we report on how our methodology relates to recent empirical approaches on this issue (Tesfa et al., 2009; Catani et al., 2010). Moreover, we discuss the applicability of process-based approaches for predicting the spatial variation of colluvial soil depth in our sites (Dietrich et al, 1995; Heimsath et al., 2005; Saco et al., 2006).

Comment 6:
The authors present a sound and very worthwhile methodology but do not show it works. The authors could in my opinion elaborate on success and prediction rates, on the effect of uncertainty of especially the soil thickness and compare the results with more classical susceptibility mapping.

Response:
Concerning this point, the revised manuscript includes an intercomparison with the QD_SLaM model (Borga et al., 2002a; Tarolli et al., 2010), which is based on the same quasi-dynamic approach but which doesn't include the unsaturated modeling part.

Specific comments:

Comment 7:
Could you elaborate a bit why lateral flow occurs on hillslopes and that can be modeled without taking into account explicitly preferential flow?

Response:
The model does not take into account preferential flow paths. It is a model to be applied at catchment scale where information about preferential flow are not available.

Comment 8:
Is the infinite slope assumption the best to use? Is it valid to apply? See for example: Milledge et al (2012): DOI:10.1002/esp.3235

Response:
Rightly, Milledge et al (2012) state that the infinite slope stability model assume implicitly that (a) grid cells represent the dimensions of the predicted landslides and, (b) the grid dimensions are large enough relative to the failure plane depth so that the infinite slope assumption is valid.
They showed that the infinite slope assumption can be valid if the ratio L/H is greater than 25, but that this ratio can be smaller for low cohesion soil. In our application, the ratio L/H is generally larger than 15 and the soil is cohesionless. Therefore, the use of the infinite slope model appears reasonable for our application. It is also worth pointing out that at the catchment scale the infinite slope stability model is the only suitable to predict shallow landslide occurrence with a reasonable number of input parameters and it is sufficient to derive a map of shallow landslide susceptibility

Comment 9:
Eq20: Is the threshold of 2000m or 2500m based on something? I do not see why this differentiation could be relevant, especially with only 49 observations.

Response:
The elevation of 2000 m asl define a threshold in soil pedological properties (as reported also by Aberegg et al., 2009). The analysis of the soil type distribution showed that *Episkeletic Podzols* and *Dystri-Chromic Cambisols* predominantly appear on slopes between 1400 to 1900 m asl. *Enti-Umbric Podzols* are characteristic for southern exposures at altitudes higher than 2000 m asl. While we were unable to detect significant differences in the geo-mechanical properties of the soils above and below the threshold, we found differences in the relationship between soil depth and local slopes.

Comment 10:
Above 40 or 45 degrees slope there is an assumption of negligible soil thickness. I did not get from the model description where is the precipitation going that applies to these pixels? Is it laterally transferred to the downslope cells or is it that water neglected?

Response:
Lateral flow in a point is described by using the dynamic topographic wetness index which is calculated by considering the upslope contributing area. Therefore, in our work it is assumed that the precipitation that applies to the points with negligible soil thickness is laterally transferred to the downslope cells.

Comment 11:
P.4115, L8: there is more than 50% forest coverage in the catchments and there are soils of only a few decimeters till 1 meter. How can we then assume cohesionless soil? Could you overlay the landslide inventory with the land use map? How many landslide occur in forested part of the catchments?

Response:
The revised version includes a land use map of the catchments. Concerning the effect of forests on hillslope stability, our field survey included several observation concerning the morphology of the root system, with specific attention to the distribution of roots and whether they cross the failure plan. The survey revealed that the trees in this region are characterized by shallow root systems that spread laterally with small vertical sinker roots that penetrate deeper into the soil. Owing to these observations, we decide not to consider the root strength contribution into the shallow landslide stability analysis. Since the factor of safety calculated by the infinite slope stability equation is fairly insensitive to the values of tree surcharge (Borga et al., 2002a), we omitted considering this factor too.

Comment 12:
P 4115, L 24-25. The authors state they do the modeling for a predictive procedure, not for diagnostics. As stated before, the modeling approach is really interesting, but is a deterministic model that needs some calibration / validation / verification. Why otherwise doing it and not adopt standard map overlay techniques?

Response:
The revised manuscript includes an intercomparison with the QD_SLaM model (Borga et al., 2002; Tarolli et al., 2010), which is based on the same quasi-dynamic approach but which doesn't include the unsaturated modeling part.

Comment 13:
P.4116, L 5. Why using the rainfall statistics and not the measured rainfall?

Response:
As we do not know, as often happens, the exact rainfalls (with its spatial distribution) that generated our inventoried shallow landslides, we presented an alternative approach to test the ability of a susceptibility map to indicate where shallow landslides are more prone to occur. Actually we think that our approach is, in the context of missing information, very much valuable, and, as a matter of facts, the observed triggered areas are generally close to points characterized by low return periods

for the critical rainfalls.

Comment 14:

P.4116, L.16: I do not see that the results are good. You do not show this. Here I find the paper very weak. If I glance to figures 4 and 5, I think you are mostly wrong and sometimes correct. All the high susceptibilities in the upper part of the catchment are not in your inventory. See my general comment to follow the success rate, prediction.

Response:

Please, refer to Point 6 of this document.

Comment 1:

Comment 15:

The authors are totally right that other methods do not account for (unsaturated) hydrology and surely not lateral contributions. But do you proof it is important? Does taking into account of unsaturated hydrology and lateral flow improve the susceptibility mapping?

Response:

In the revised manuscript this will be showed by comparing our results with the results obtained by using the QD_SLaM model (Borga et al., 2002; Tarolli et al., 2010), which is based on the same quasi-dynamic approach but which doesn't include the unsaturated modeling part.

References:

Aberegg, I., Egli, M., Sartori, G. and R. Purves, 2009: Modelling spatial distribution of soil types and characteristics in a high Alpine valley(Val di Sole, Trentino, Italy). S*tudi Trent. Sci. Nat.*, 85.

Borga, M, G. Dalla Fontana and F. Cazorzi, 2002b: Analysis of topographic and climatic control on rainfall-triggered shallow landsliding using a quasi-dynamic wetness index. *Journal of Hydrology, 268(1-4), 56-71, 2002*.

Borga, M., Dalla Fontana G., Da Ros D. and L. Marchi, 1998: Shallow landslide hazard assessment using a physically based model and digital elevation data. *Journal of Environmental Geology, 35(2-3), 81-88, 1998*.

Borga, M., F. Tonelli and J. Selleroni, 2004: A physically-based model of the effects of forest roads on slope stability. *Water Resour. Res., 40(12), W12202 10.1029/2004WR003238, 2004.*

Borga, M., G. Dalla Fontana, C. Gregoretti, and L. Marchi, 2002a: Assessment of shallow landsliding by using a physically based model of hillslope stability. *Hydrological Processes, 16, 2833-2851, 2002.*

Catani, F., S. Segoni, and G. Falorni, 2010: An empirical geomorphology-based approach to the spatial prediction of soil thickness at catchment scale. *Water Resour. Res.*, 46, W05508, doi:10.1029/2008WR007450.

Chung, C.-J.-F., Fabbri, A.G., 2003: Validation of spatial prediction models for landslide hazard mapping. *Natural Hazards*, 30 (3), 451-472.

Dietrich, W. E., R. Reiss, M.-L. Hsu, and D. R. Montgomery, 1995: A process-based model for colluvial soil depth and shallow landsliding using digital elevation data. *Hydrol. Processes*, 9, 383 – 400, doi:10.1002/hyp.3360090311.

Heimsath, A. M., D. J. Furbish, and W. E. Dietrich, 2005: The illusion of diffusion: Field evidence for depth-dependent sediment transport. *Geology,* 33, 949–952, doi:10.1130/G21868.1.

Saco, P. M., G. R. Willgoose, and G. R. Hancock, 2006: Spatial organization of soil depths using a landform evolution model. *J. Geophys. Res*., 111, F02016, doi:10.1029/2005JF000351.

Tarolli, P., M. Borga and G. Dalla Fontana, 2008: Analysing the influence of upslope bedrock outcrops on shallow landsliding. *Geomorphology*, *93, 186-200, doi:10.1016/j.geomorph.2007.02.017, 2008.*

Tarolli, P., M. Borga, K-T. Chang, S. Chiang, 2011: Modelling shallow landsliding susceptibility by incorporating heavy rainfall statistical properties. *Geomorphoplogy, 133, 3-4, 199-211.*

Tesfa, T. K., D. G. Tarboton, D. G. Chandler, and J. P. McNamara, 2009: Modeling soil depth from topographic and land cover attributes. *Water Resour. Res*., 45, W10438, doi:10.1029/2008WR007474.