Answer to Reviewer No. 2 of:
Modelling catchment-scale shallow landslide occurrence by means of a subsurface flow path connectivity index, by Lanni et al.

June 29, 2012

Introduction

We thank the Reviewer#2 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, and ii) the statistical model relating soil depth and local slope.

We hope that this effort will improve the manuscript, by strengthening the weak points highlighted by her/him.

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.

Comments

Comment 1:

The map of observed shallow landslides (Fig 4), which is used as a reference for the modeled susceptibility map (Fig 5), does not show typical shallow landslides that are a result of slope water table. Most of them are connected to the stream and are – most probably – a consequence of channel erosion destabilizing the base of the slope. In my view, this is another process than the one described by the model of Lanni. I don’t see that the model includes stream runoff, which is the key for channel erosion. Also, the indicated landslides are much bigger (in the order of 100 m) than typical shallow landslides (in the order of 10-50 m)

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. However, in order to evaluate the performance of the presented model, the map of figure 5 has been only compared with the landslide initiation points reported below. Several of the landslides considered in the work evolved as debris flows.

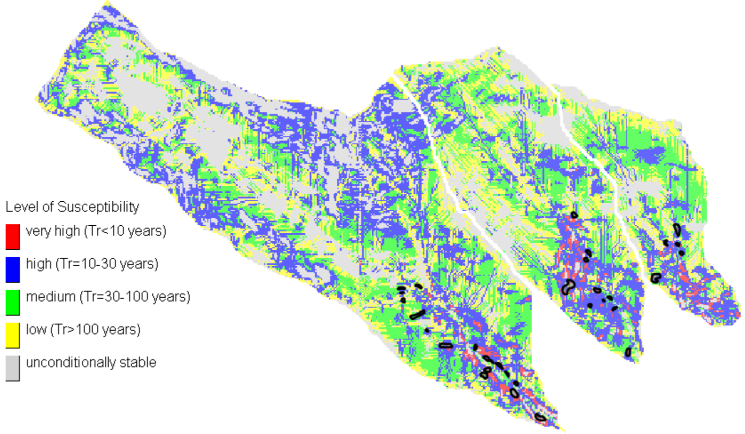

Figure 1: model results with the landslide initiation points used to evaluate the model performance

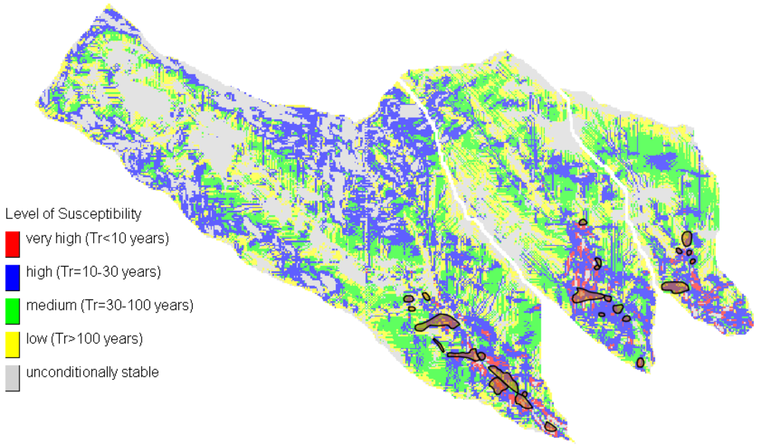

Figure 2: model results with total landlslide areas due to pore pressure increase. These areas include both the initiation area and the a portion of the run-out path

Comment 2:
The simulated pattern of return periods (Fig 5), which are said to be a good representation of the observed landslides, have strange anisotropic features that cannot be explained by the topography. I assume this is an artifact (numerical problem?).

Response
We agree with the Reviewer#2 that the simulated critical rainfall pattern shows anisotropic features. This is due to choice of using a D8 methodology in the numerical estimation of the quasi-dynamic upslope area. Indeed, these features are an expression of both the topography and of the methodology used to analyse the flow paths.

Comment 3:
Figure 6 shows a comparison of simulated rainfall-intensity duration thresholds of LANDSLIDE with observed rainfall-intensity duration thresholds of DEBRIS FLOWS. I don't think that this is appropriate. The triggering of shallow landslides on the slope is a different process that the triggering of debris flows in the channel. The second strongly depends on the (temporarily) deposited material in the channel that gets mobilized as the debris flow releases. So, this comparison is not justifiable.

Response
We agree with the Reviewer#2 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 will be corrected in the revised version of the work.

Comment 4:
Table 4 (2, I guess) is unclear to me. What exactly do C* and L** represent?

Response
Table 2 shows the proportion of catchment area placed in the intervals of critical return period and the corresponding fraction of the landslide area. For instance, please consider the case of the Pizzano catchment area where the failure happens to be between the 0-10 year return period interval. This is an area that is expected to be highly susceptible to landsliding. The fractions of catchment area and the measured landslide area included in this interval are 1.6% of the total area (C*) and 51.6% of the landslide covered areas (L**), respectively. To clarify this issue we will include a more detailed description of the Table 2 content in the revised version of the paper.

The suggestion of the reviewer is interesting. However, while randomizing the hydraulic conductivity at saturation would be easy (and there exists a lot of literature where it is performed), in the present case, we should also randomize the soil water retention curves, and, consequently, the relative hydraulic conductivity. The latter operation would be necessary because water retention curves are used to estimate the water volume in the vadose zone corresponding to a certain variation of hydraulic head, and, as a consequence, produces an estimation of timing when a perched water table start to form at the bedrock interface in a point. Frankly, we do not know how this can be accomplished in a consistent manner, since there is no guidance, at our knowledge, in literature.
Considering that we make a statistical use of the information produced, and the complication (both conceptual and practical) that such a randomization would introduce, we prefer, in the trade-off with completeness, to maintain simplicity.

Comment 6:
Soil depth is modeled as a function of local slope angle (Eqs. 20 a-d) based on a set of 49 measurements. How good is this relationship? This has to be shown. The differentiation between areas above and below 2000 m is arbitrary to me.

Response
We have collected data and make statistical intercomparison to assess the quality of the statistical relationship between soil depth and local slope. The revised version of the paper will include a section devoted to the presentation of the data set and of the statistical analysis. This section will also show the different behavior between the basin area below and above the 2000 m asl threshold.

Comment 7:
According to the study site description (chapter 2.4) the test areas include a lot of vegetation (forest, grassland), but I don’t see that this is considered in the model application.

Response
This effect may be included in the model by introducing a cohesive term in the FS equations. Actually, 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 8:
The initial soil moisture conditions are vaguely defined (page 4115, line 10-14), but they are important given that the duration of rainfall to failure is relatively short (a few minutes to a few hours). What do the authors mean with “were assumed to represent average climatic conditions: : :”? Is the initial soil moisture content uniform for the entire catchment? Or is there an altitudinal gradient or does it depend on soil depth?

Response
We agree with the Reviewer that initial conditions are very important for shallow landslide triggering processes. Here, we used an inter-storm period of 10 days and an evapo-transpiration rate of 3 mm/day.
The initial soil moisture content is not uniform through the catchments but depends on the local slope angle as it is assumed that the drainage efficiency of a pixel is directed related to its own local

slope value.

Comment 9:
For the model validation in the three test catchments, why did you work with return periods of rainfall intensity-duration thresholds instead of using real measured timeseries of precipitation? According to chapter 2.4 (Page 4113, lines 18-21) the time period, where the observed landslides were triggered, is known (2000 to 2002)

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 10:
I am not very familiar with the work of Burlando and Rosso (1996) which is the reference for the rainfall intensity-duration relationship (Eq. 17). But it seems that this relationship is uniform for the entire Central Italian Alps; did I get that right? What happens if one wants to use the model for other areas? Shouldn't Eq. 17 be formulated in a more general way?

Response
The Gumbel relationship is generally used to describe extreme rainfall events everywhere. However, the parameters of this relationship (ς_F and m) are site-dependent, and we use the parameters estimated, with high goodness of fit, for our sites.

Comment 11:
Eq. 13 is incorrect. FS should be F(r)/F(d) if the terrain is stable for FS > 1.

Response
Yes, we corrected this typo in the revised manuscript

Comment 12:
What is the advantage of Eq. 5 over the commonly used m=1-1/n?

Response
The advantage to use m=1+1/n instead of m=1-1/n is that the first one allows integration of Eq. (4). Obviously all the parameters of the soil water retention curves used has to be modified appropriately.

Comment 13:
In Appendix 1, the authors want to demonstrate that their simplified infiltration model provides similar results as the well-established Richards-model. But Fig. A1 is not a very conclusive demonstration. The simulated differences in t(wt) [time to build up a perched zone] between the models are not related to a t(wt) typical for these scenarios. So how can we know that these differences are small?

Response
A revised version of Appendix 1 is reported in the revised manuscript version.

References

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

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.*