Hydrol. Earth Syst. Sci. Discuss., 9, C1562–C1565, 2012
www.hydrol-earth-syst-sci-discuss.net/9/C1562/2012/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Modelling catchment-scale shallow landslide occurrence by means of a subsurface flow path connectivity index" *by* C. Lanni et al.

Anonymous Referee #2

Received and published: 19 May 2012

The hydrology of slopes prone to shallow landslides is a field of research that has received increased attention. Modeling the lateral water fluxes and the spatial distribution of the soil pore water pressure of such slopes is very challenging because of the considerable spatial variability of soil properties, vegetation and topography, but also because of the lack of an optimal model framework that represents the controlling mechanisms of the release of shallow landslides. Here, the work of Lanni and colleagues presents an approach that is based on state-of-the-art models to simulate water flow and stability of slopes, as well as return period of rainstorm events. I appreciate very much that the authors include the issue of hydrological connectivity to

simulate the lateral sub-surface runoff. All in all, this combined model approach is a possible way to simulate the susceptibility of slopes with regard to shallow landslides.

Unfortunately, the validation of the model with field observations (Figures 4-6 and Table 2) is not very convincing. My main concerns are the following: 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). 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 can not be explained by the topography. I assume this is an artifact (numerical problem?). Also, there is no overlay of the simulated with the observed landslide area. So it is not clear to me whether the red simulated areas correspond to the observed landslide area or not. 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. 4) Table 4 is unclear to me. What exactly do C* and L** represent?

In addition, I'm questioning some parts of the application of the model in the three Italian test catchments: 1) The hydraulic soil properties are assumed to be uniform in the entire test area. (Why didn't you try to make a random or systematic variation of the hydraulic soil properties for an observed range of soil properties?) This means that hydrological connectivity only depends on soil depth. 2) Soil depth is modeled as a

HESSD

9, C1562–C1565, 2012

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. (maybe not so important) 3) 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. 4) 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? 5) 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 time-series 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).

I have also a few remarks/questions with regard to the model description: 1) I'm 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? 2) Eq. 13 is incorrect. FS should be F(r)/F(d) if the terrain is stable for FS > 1. 3) What is the advantage of Eq. 5 over the commonly used m=1-1/n? 4) 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?

Finally a few typos on page 4115: - Line 10: the initial soil moisture conditions (not the soil moisture initial conditions) - Line 26: intensities (not intensity) - Line 28: Eq. (19)

HESSD

9, C1562–C1565, 2012

(not Eq. (20))

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 4101, 2012.