

The paper describes a new method for evaluating landslide susceptibility based on a modified formulation of the infinite slope model. The method takes into account the variation of slope stability associated with the downward propagation of a wetting front through an unsaturated granular soil. The work is interesting, clearly written, and well-illustrated. There are however several issues that I think should be clarified before publication.

1. My first concern is the proposed formulation of the infinite slope model with wetting front propagation (equation 7, p.798). At p.796, the authors states that “*The existing equation for calculating the factor of safety of an infinite slope is as follows (Hammond et al., 1992)*”

$$FS = \frac{c_r + c_s + \cos^2\theta[\rho_t g(D - D_{wf}) + (\rho_t g - \rho_w g)D_{wf}]\tan\phi}{D\rho_t g \sin\theta \cos\theta} \quad (1)$$

However, this is not the original equation proposed by Hammond et al. (1992). Hammond and co-workers did not considered the presence of a wetting front but a saturated soil thickness (with positive groundwater pressure) at the base of the unstable layer. In their model  $D_w$  is the thickness of the perched water table measured from the top of the bedrock. Authors should clearly state that equation 1 is not the original one, and explain how this formula (in which  $D_{wf}$  is the depth of the wetting front measured from the ground surface) was derived. It is an important point because the paper mainly focus on the application and validation of this model.

2. Regardless the derivation, equation 1 (which is the base for equation 7) seems to provide strange results. Let consider the term in square brackets, that gives the effective stress on the slip surface:

$$\rho_t g D - \rho_t g D_{wf} + \rho_t g D_{wf} - \rho_w g D_{wf} = \rho_t g D - \rho_w g D_{wf}$$

In case of no wetting front ( $D_{wf} = 0$ ) the equation predicts effective stress=total stress= $\cos^2\theta\rho_t g D$ , which is the expected result in dry conditions. However, if a wetting front exists ( $D_{wf} > 0$ ) the equation predicts positive pore water pressure ( $\rho_w g D_{wf}$ ) and subtracts this value to the total stress to compute effective stress at depth  $D$ . I do not understand why a wetting front at depth  $D_{wf}$  should induce positive pore pressures at the base of the soil cover. For the limit case  $D_{wf} = D$  we obtain  $\cos^2\theta(\rho_t g - \rho_w g)D$ , which is the effective stress at the base of a saturated soil layer with slope-parallel seepage.

Therefore, equation (1) does not seem to consider the presence of a wetting front. Rather, it computes the conventional factor of safety of an infinite slope in which (positive) groundwater pressures are measured from the ground surface downward (!). Of course I could be wrong and I hope I am. Anyway, I kindly ask the authors to better explain how their model was derived.

3. The authors motivate their approach by stating that shallow landslides in Korea “*are associated with the advance of a wetting front in the unsaturated soil due to rainfall infiltration, which results in an increase in water content and a reduction in the matric suction in the soil.*”. Although this is certainly possible, some data should be provided to support this statement. For instance, landslides could be triggered by the formation of a temporary, perched water table at the base of the soil cover in response to intense rainfall

events. In this case pore pressures are positive at the time of failure. Are there field data to support the hypothesis of failure in unsaturated conditions? If not, I suggest to leave this just as a (realistic) working hypothesis.

4. More credit should be given to previous studies that investigated slope stability in unsaturated soils. There are several models (e.g. TRIGRS) where water infiltration in unsaturated conditions is accounted for. What are the advantages of your approach compared to existing models?

#### Minor comments

- p.795, row 2: what do you mean with “to easily break down in water” ?
- p.795, row 5-10: I would not say that “existing methods of infinite slope stability analysis are limited.. because the unit weight and thickness of the soil layer are assigned constant values”. This is not a limitation of the method but a way the method is commonly used. One assumption of the infinite slope model is that soil properties are constant along the slope (or in the reference cell). However, there is no limitation in changing soil properties with time. Please clarify this concept.
- Equations 8-10-11: the letter  $t$  is used to indicate time, rainfall duration and infiltration water detection time. Please use different letters to avoid confusion.
- p. 804, row 2-8. I do not see any significant difference between the volumetric water content of the different samples. Moreover, why the maximum volumetric water content should increase with the unit weight of the soil? Should not be the opposite (higher density=less voids)?
- p.806, row 8-9. Please compare the observed infiltration velocities with the measured values of saturated hydraulic conductivity of the soil. They should be similar.
- p.807, row 23. How many samples were collected to create the hydraulic conductivity map shown in Fig. 14? Which kind of test were done? How did you reconstitute the samples?
- p.808, row 10. It is not clear how the infiltration velocities obtained from the soil column tests were used to compute factor of safety (equation 7). Did you selected a specific time instant to compute the saturation depth ratio  $H(t)$ ?
- p.810, row 3. You say “..an analysis using the previous steady state model” but I can’t find where this model was previously described.
- Conclusions can be shortened avoiding repetitions
- Figures 9 to 15 can be compacted in 1-2 figures