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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Effects of coupled hydro-mechanical model considering two-phase fluid flow on potential for shallow landslides: a case study in Halmidang Mountain, Yongin, South Korea" *by* Sinhang Kang and Byungmin Kim

Anonymous Referee #1

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The paper studies the effect of two phase fluid flow analysed in a coupled hydro-mechanical manner on rainfall infiltration modelling and slope stability at a regional scale. The research also looks into the variation of hydraulic conductivity through the Kozeny-Carman equation owing to the soil deformation under subsurface water infiltration. The study is interesting in that the authors seek to extend the two-phase coupled hydro-mechanical behaviour under rainfall infiltration from the previously slope-scale limited study to a regional scale and also, strives to understand the changes in hydraulic conductivity owing to infiltration-induced deformation. However, the research

lacks novelty and has significant shortcomings with regard to the methodology and fails to impress upon the reader the need for such a complex undertaking instead of the traditional single-phase modelling at a regional scale.

Highlighted below are the major issues in the study:

1) The use of the Kozeny-Carman equation (16) to link the volume changes in unsaturated soil with the variation of saturated hydraulic conductivity (ks) doesn't seem reasonable. The Kozeny-Carman equation is used to roughly predict the vertical saturated hydraulic conductivity for homogenised soils. As far as I am aware Chapuis and Aubertin (2003) or any other studies have not tested the equation to model volume change behaviours like swelling or collapse under saturated or unsaturated conditions. Whilst procedures to measure the volume change in unsaturated condition exists, the quantification of corresponding changes in hydraulic conductivity owing to the volume changes under rainfall infiltration is a task yet to be accomplished. The paper also doesn't explain how the Kozeny-Carman equation for saturated hydraulic conductivity is used to model effective stress changes and the subsequent variations in unsaturated hydraulic conductivity under rainfall infiltration. In light of the above shortcoming, the reviewer thinks the review of the results presented in the paper as of now would be a fruitless exercise.

2) The authors have not clearly explained how the two-dimensional model for seepage analysis (FLAC) has been applied at a regional scale. Has the subsurface-flow routing from different grid cells been considered? Also, any attempts at validation cannot be seen (e.g. field-based monitoring, streamflow data from gauge stations, etc.).

3) Did the authors use the effective stress estimated during the hydro-mechanical coupled seepage analysis in FLAC in the assessment of the factor of safety? Please provide a detailed explanation in section 4.2.

4) It is difficult to follow the motivation of the authors in conducting the two-phase coupled hydro-mechanical based infiltration modelling at a regional scale. No information

could be found in the paper with regard to the volume change behaviour of soils from Central Korea under unsaturated conditions (soil volume changes under wetting and drying). This is a fundamental issue the authors need to sort out before attempting to model at any scale. Also, under circumstances of volume change, authors would require to carry out SWCC corrections for volume change as well.

5) Another drawback is the lack of description with regard to the field mapped landslide characteristics, evidence from sites in all zones with regard to soil profiles (single or several different layers) and soil depth especially when field investigations were carried out (mentioned in Section 3). Please provide necessary details.

6) Vanapalli et al. (1996) used two approaches to calculate the shear strength. The first approach was to use a dimensionless number "normalised area of water with k as a fitting parameter" and the second approach was to use a normalised degree of saturation (defined as effective saturation in this paper) wherein the residual degree of saturation needs to be estimated. The authors in this study have substituted Bishop's matric suction coefficient with the saturation of a wetting fluid variable (Equation 12 and Equation 14). Could the authors explain the basis for equating the degree of saturation (instead of an effective saturation) of a wetting fluid with the Bishop's matric suction coefficient?

7) The authors have focused more on the modelling aspect with advanced two-phase modelling at the regional scale and did not worry much about the variability in input data which clearly will influence the safety factor values. It is recommended that such a study (sensitivity analysis) be undertaken in the region. Also, could the authors explain why only watershed criteria was used in creating the zones? Why wasn't geological information used? Please explain in detail.

8) The reasoning for the selection of a 10-m DEM is not clear (Section 3). Why is channelisation important for slope stability analysis? I can understand its importance in debris flow modelling. Please explain by also including information with regard to the

size of landslides mapped.

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