

## ***Interactive comment on “Improvement of shallow landslide prediction accuracy using soil parameterisation for a granite area in South Korea” by M. S. Kim et al.***

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This paper tests the impact of material properties on landslide model performance using SHALSTAB at a study site in Korea and quantifying performance with ROC curves. The authors present a new method to calibrate the cohesion and friction angle parameters based on measured soil depths and local slope at observed shallow landslide scars. They find that:

- using measured cohesion and friction angle values (Case I) results in worst model performance;

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- using measured soil depth and slope angle at ~20 landslide scars to calibrate soil strength parameters results in best model performance (Case III); and

- using average soil depth with observed slope angle for the same landslide scars results in intermediate model performance (Case II).

These findings are well supported by their observations and the method that they have used is appropriate and robust. My one major concern with the paper's main finding is that given an existing shallow landslide inventory a simpler approach would be to calibrate the model parameters using the ROC curve as the objective function rather than the landslide depth-slope data. If the authors could demonstrate that their method performs better than this approach by comparing their performance, using a split test (with a training set of observed landslides and a second set for model testing), then I would be much more comfortable with the authors' claim to have found an improved method for calibrating soil strength parameters.

There are a number of problems with the equations in the paper, mostly relating to style rather than substance. I do not expect these to significantly alter the results but they do need to be addressed.

1) There is some confusion over the reference frame inside which the authors are measuring soil thickness. They say (L12 p233) that soil thickness is measured slope perpendicular however in the diagram (fig. 2) it is shown measured in the vertical. Equations 1-4 are correct assuming that soil thickness is measured slope perpendicular. However, equations 5 to 7 assume that soil thickness is measured in the vertical. This results in an additional cosine beta term in each of equations 5-7. This is the most critical issue that needs fixing since it may have an impact on the results.

2) There are also several inconsistencies in notation between equations: density  $\nu$  unit weight notation,  $\theta$   $\nu$  beta for slope. The authors split C into root cohesion and soil cohesion components but then ignore root cohesion in the rest of the paper and define C differently in equations 5 to 7. The authors should try to keep the equations as simple

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and consistent as possible. All of their equations can be directly related to equation 1, making this clear to the readers will really help them to understand what the authors are trying to do in this paper.

The set up and structure of the paper is a little confusing at present. The aim of the paper wasn't clear to me; nor was what exactly each different case represented and why there were then three different versions of each case. I suggest that the authors make a clear statement at the very start of the paper that their aim is to test the impact of soil strength parameters on model performance and to find a way to use observed landslide depth and slope data to calibrate these soil strength parameters. They should then set out each of the cases that they will test and each of the subsets within those cases with a clear justification for why they are testing them and exactly what each case includes.

Much of the introduction seems focused on the influence of soil depth on slope stability calculations. However, the paper's findings are focused on the impact of soil strength parameters on model performance. The authors should shift the focus of the introduction away from spatially variable soil depth (which they do not include in their model as I understand it) and towards how people have measured and parameterized soil strength in their models.

I think some of the figures could be condensed. Figs. 5, 8 and 10 could be combined onto a single figure using line rather than bar graphs and differing line styles. This would then enable inclusion of another figure showing the ROC curves for each of the model runs. For me these curves are probably the best way that I can interrogate model performance so I think their inclusion is important. Figure six is very good, the authors should consider creating a similar matrix of maps combining figures 4, 7 and 9. This will enable the reader to compare between cases much more easily. I recognize that this will result in fairly small maps so perhaps they could also consider a second matrix of maps showing only the predictions for the sub-catchment in which soil depth was measured.

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Specific Comments P228 L5-10: the different cases are not clear from this description. L12: are the accuracy values on the ROC analysis area under the curve? If so make this clear. L13: "so parameters calculated from a stochastic..." this explanation is not clear. You should explain that you calibrate the soil strength parameters to ensure that observed landslide depth and slope angle does not fall outside the limits defined by the infinite slope equation. L 19: climate change is a strange way into this paper, why mention it here? The paper does not go on to address climate change and I don't think that this is necessary here. L 20: changes to landslide magnitude and frequency in response to climate are still contested. I don't think the increasing frequency and magnitude of landslides is necessarily relevant to your paper. L 24: Muss should be Mudd

P229 L8: short intense rainfall is important but SHALSTAB does not deal with this. L9 "previous rainfall" is there a word missing here? L 16: "landslide warning systems" insert reference. L 16: replace instability with geotechnical. L 20: rather than talking about flows from upslope discuss pore water pressure here. L 23: these examples are both of very simple hydrological treatments, include one or two more complex examples. L 25: include a reference for the infinite slope equation. L 27: is the infinite slope treatment appropriate for the landslides that you're studying? See Milledge et al., 2012.

P230 L4: Dietrich et al 1995 included spatially varying soil depth. L17: stability is always affected by the friction angle not only when gradient exceeds the friction angle.

P 231 L5 your comment on increased heavy rain storms needs a reference otherwise leave it out it is not essential to your argument.

P232 L 20: O'Loughlin proposed a hydrological model, Montgomery and Dietrich used this hydrological model to predict landslides.

P 234 Sec 3.2 the way that you introduce your approach here is slightly awkward. The model that you use to calibrate your soil strength properties is not really stochastic. Though lida used equation 5 in a stochastic context the equation itself is really just a

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rearrangement of your equation 1. If you rewrite the equation using your own notation and do the same for equation 6 and 7 then you can introduce your calibration method in a way that is consistent with the model you are using. P 236 L 15 the number of drops at which bedrock is identified might vary between sites. You could test this for your site by examining the number of drops at test sites next to observed landslides where the depth of the failure plane is known.

P 239 L 20: you go into a lot of depth about what your result might imply for the impact of the different soil strength measurement methods on the models performance. You then rightly point out that there are several other reasons why we might expect these results. Given all of this I'm left wondering if there is any more you can say than different techniques result in different strength parameters and these will result in different model predictions; the extent to which these differences are related to the measurement method relative to other sources of uncertainty is unknown. It is also worth noting that the most successful soil strength parameters used in infinite slope model (in terms of model performance) are really effective parameters since the model does not account for strength on all the margins of the unstable block.

P 240 L 17 think realistic should read unrealistic on this line.

P241 L5: Equation 6 but not equation 7 assumes that shallow landslides only occur when the soil is completely saturated. These equations together set the limits on the depth-slope space in which landslides can occur.

L27-29: this is a good explanation of cases II and III and needs to come earlier in the paper.

P 242 L2 the detail of exactly how you changed the internal friction angle and cohesion (i.e. your calibration method) is very important here. If you optimized friction angle and cohesion to ensure that all landslides sat within the limits defined by equations six and seven then what causes the difference between a B and C in case II? I guess that there are several parameter combinations that satisfy this requirement but you should tell us

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in more detail exactly how you found these parameter combinations.

P 245 L 17: Antecedent rainfall plays an important role in the initiation of landslides but SHALSTAB assumes hydrologic steady-state (i.e. all slopes are in equilibrium with infinite duration antecedent rainfall at the current rate). Perhaps in this context it is worth discussing the fact that rainfall duration also plays an important role in landslide initiation, since this is the property of storms most severely violated by SHALSTAB's hydrological assumptions.

P 246 L 11 I think rather than prior to soil parameterization, which could be achieved based on measured soil strength values you should call this prior to calibration of the soil strength parameters.

P 247 L1: the hydrological parameters and hydrological behavior of the study area is likely to be important; however, it would be very difficult for you to investigate this within your current modelling framework even hydraulic conductivity would be difficult to investigate in your steady-state model. I don't think you need to include this sentence since it is not essential to and may distract from your main findings.

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 3, 227, 2015.

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