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Discussions

Interactive comment on "Susceptibility assessment of landslides under extreme-rainfall events using hydro-geotechnical model; a case study of Umyeonsan (Mt.), Korea" *by* S. S. Jeong et al.

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The paper reports on devastating landslides in a mountainous area near Seoul that were triggered by two typhoons in 2011 - one in June and another in August. Although one of the stated objectives of the paper is to investigate the cause of the landslides, insufficient site and triggering (rainfall) data are presented to make a cogent argument. Although it is stated that a large amount of field data were collected (bottom of pg. 5578), few supporting field data are present, raising concerns about some of the trig-

gering mechanisms inferred by the modelling approach that the authors used. Most obvious is the lack of temporally explicit rainfall data for the two typhoons (except for a brief mention of 307 mm/day in Fig. 2); it is not even clear which typhoon was modelled and which typhoon (or both) caused damages. These issues need attention if the authors should elect to resubmit this paper. Additionally, the paper should be reviewed for English content prior to any resubmission. My more specific comments are as follows:

As it is well known, rainfall patterns have a strong influence on slope stability, but where is the evidence that "global climate change led to fluctuations in rainfall pattern" at the South Korean site?

Pg. 5577, L. 15-19 The global climate change scenario described herein (i.e., increased evapotranspiration) would decrease the probability of landslding, not increase it.

Pg. 5577-5578, L. 24-25 & L. 1-11 I do not agree with the author's statement "The quantitative increase and frequency change of recent rainfall patterns often cause shallow soil slope failures in comparison with past rainfall patterns" – shallow landslides would only increase if higher intensity storms occur, and there is no evidence to support this. And why did you select the 4 stations you did to show the short-term (30 yr) records of annual daily maximum rainfall? There were many other stations much closer to the Umyeonsan site. It almost appears that you selected the most distinct patterns for maximum daily rainfall increase; I hope this was not the case, but in any event, you should have selected rain gages near the Umyeonsan site. No statistics are shown in Fig. 1 and the trends at the two sites closest to Seoul (Inje and Jecheon) have weak increasing trends at best. Furthermore, 30 years of record is not sufficient to really talk about major climate trends. The statement that "the cumulative rainfall for 2 months before landslides event in 2011 was unprecedented in the last 10 years" does not necessarily represent a triggering event of catastrophic proportions.

Sections 3 & 4 pgs. 5578-5579 Where is the information on soils in this area? This is critical. Poorly developed and poorly structured soils may indeed experience relatively uniform infiltration and percolation of rain water, which is the premise used by the authors throughout this paper. However, if soils are highly structured and have preferential flow networks – both lateral and vertical – then preferential flow may dominate movement of water to a failure plane, not matric suction. Without knowing something about these soils and the site (e.g., tension cracks, vegetation), this is difficult to assess and the author's assumptions seem speculative. You finally report soil information in Table 3, but it is very general with no spatial specificity. From the limited information you present, it appears that the soils are cohesive and likely have significant clay content. Therefore I would expect that the soils are indeed structured and may contain preferential flow paths. Finally, and very importantly, where are the rainfall hyetographs (in various locales) of the storm(s) that triggered the landslides? These data are essential.

Section 4.2 Why is this entitled "The chemistry of development"? – it has nothing to do with "chemistry". The period of monitoring described herein seems inadequate to parameterize the slope hydrology model; only two storms occurred in this period as noted in Fig. 3. What was the point of this short-term monitoring?

Section 5 I would argue that you are assuming that shallow landslides occurred during unsaturated conditions via loss of suction – where is the evidence? What precipitation did you use in your YS-slope model? You presented none in the paper.

Sections 5.1, 5.2, & 5.3 What about prior published models of rainfall infiltration affecting slope stability? For example, the works of Iverson (2000 in WRR) and Godt et al. (2012 in WRR) – as well as others; these prior studies should definitely be acknowledged and any differences in your YS-slope model should be noted. How was root cohesion estimated? Was it constant over the entire area as suggested on pg. 5581 L. 21-22? I doubt it was constant. And again, this type of rainfall-infiltration model may work reasonably well in unstructured soils where a rather uniform wetting front progresses during a rain event, but does not work well in soils that contain preferential flow paths, which often occur in unstable soil mantles.

Section 5.4 Please cite some of the other studies that looked at shallow groundwater routing in hillslopes related to landslide initiation – papers by Wu and Sidle (1995 in WRR), Montgomert and Dietrich (1994 in WRR), and Dhakal and Sidle (2004 in WRR) – and note how your model differed from or improved these.

Section 5.5 Did you determine soil-water release curves based on small cores? These are often not such a good analogue for field scale hydrological behaviour. You need to better describe your methods.

Pg. 5586, L. 18-21 Only 14 boreholes to estimate soil depth? And you never say how deep soils were. I doubt that 18 sites is enough data to conduct a decent kriging analysis – did you derive some depth information via seismic methods? If so, explain how this was combined with the borehole data and either provide a map of spatially variable soil depth or other information on spatial variability.

Pgs. 5586-5587 L. 22-27 & 1-19 Much more effort needs to be put into describing the results of the modelling exercise with respect to local site conditions and the triggering events. Unfortunately, the spatial information on site conditions (e.g., soil data, topography, micro topographic features like hollows, etc.) and event rainfall are not sufficiently provided by the authors. This remains a major deficiency. And, there is absolutely no evidence that climate change had anything to do with these landslides. All mention of climate change should be removed from the paper as it is speculative at best.

Latter part of Conclusions section: The only way to be able to state your model is superior to others, is to test it against these. Otherwise this assertion is speculative.

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/2/C2848/2014/nhessd-2-C2848-2014-supplement.pdf

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