R.C Sidle #02

Overall Review

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 triggering 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:

Both storms accompanied heavy rainfall, it is difficult to discuss the physical phenomenon at Umyeonsan(Mt) without one another. In this regard, one that triggered landslides is storm MUIFA. On the other hand, storm MEARI played potential role in increasing ground water level via antecedent rainfall. Unfortunately, the actual proof to provide their relationship was not found. Therefore, in this study, both antecedent rainfall and heavy rainfall (57days) have taken into account for considering its effect in landslides.

Specific Comments

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?

This paper's co-author, Dr. Bae has studied in related climate change in South Korea.

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.

Areas under the process of desertification due to climate change, soils are being dried causing loss of cohesion, shear strength and lead into landslides. However during rainy season in Asia pacific region, rainfall and its intensity tends to increase. In which result in frequent occurrence of landslides.

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.

This paper dealt with two types of rainfall.

1. Climate change in South Korea (Increase in rainfall)
2. Heavy rainfall triggering Umyeonsan(Mt) landslides

Regarding to type 1, major local regions were selected to illustrate its result in graphical form. Type 2 focuses on illustrating the data provided by weather station (it handed over last 10 years of records) near by the Umyeonsan(Mt). It is worthwhile to note that at the time of Umyeonsan(Mt)'s landslide, rainfall has recorded unprecedented amount (Figure R2-R3).

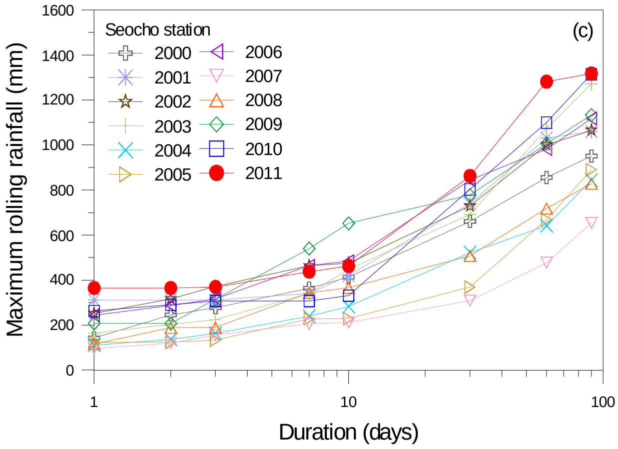


Figure R2. Maximum rolling rainfalls recorded at Seocho station

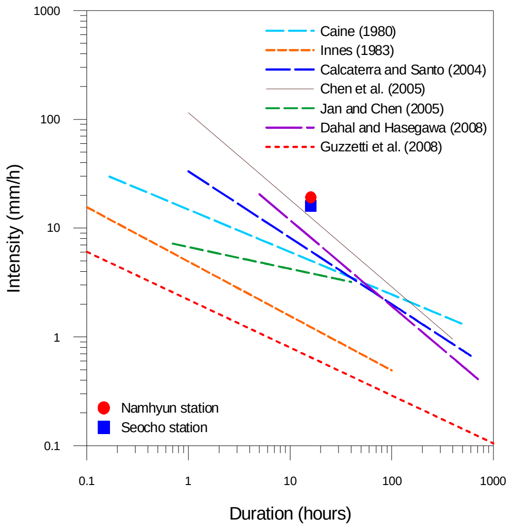


Figure R3. Comparison of measured rainfall intensity-duration data and existing intensity-duration threshold curves

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.

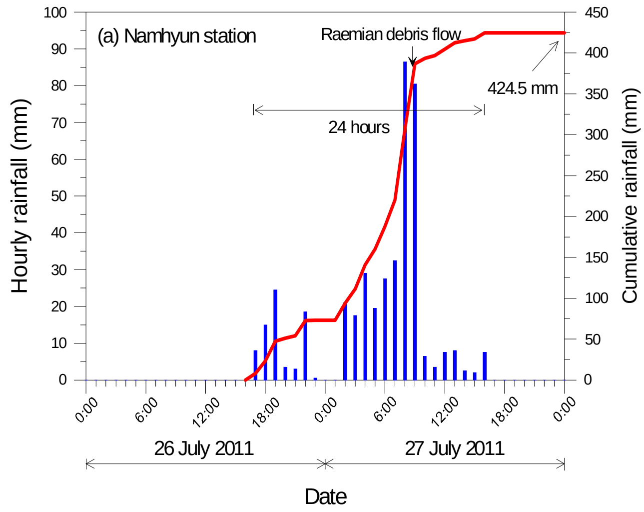
The point made earlier is definitely notable fact among the landslide types. Nonetheless, the preferential flow can be verified through conducting site investigation.

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?

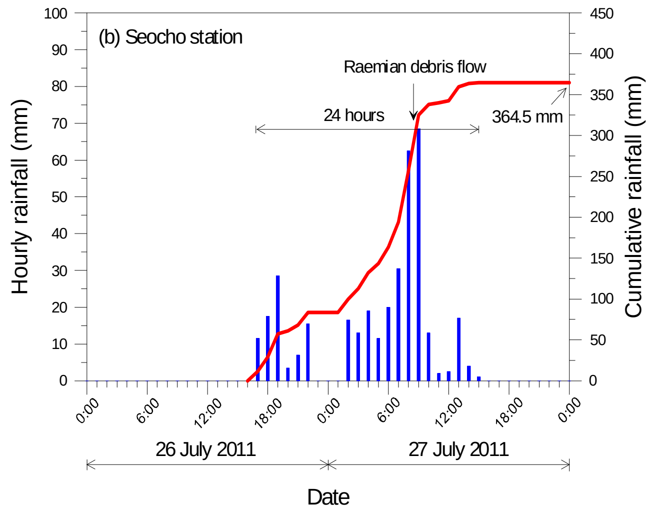
Section 4.2's title is a typo, the proper name of that section should be written as"Laboratory Test". Field matric suction is measured to estimate soil's both maximum and residual capillary effect at the site (Figure 3). It contains period of drought season so provided ample of data to be verified.

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.

Rainfall used for the assessment of landslide is provided in Figure R4.



(a) Namhyun station



(b) Seocho station

Figure R4. Hourly and cumulative rainfall of 26-27 July 2011

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

This paper develops and modifies landslide assessment model to consider various ground water levels. Generally, landslide analysis considers the infiltration by rainfall, which can be classified into three mechanisms: (1) a mechanism that considers the downward velocity of the wetting front, (2) a mechanism that considers the upward velocity of the groundwater level, and (3) a mechanism that considers both of these factors. In this study, the infinite slope model was used as a physically based model for rainfall induced landslides to use the aforementioned mechanisms for landslide analysis.

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.

Species composition is dominated by Quercus mongolica in the study areas. Based on literatures, constant strength was used in this model. As mentioned earlier, even for landslide assessment model, consideration of preferential flow paths in this case is somewhat difficult.

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.

The study mentioned earlier is used as crucial reference material in the process of developing this model. It should be pointed out that this model is capturing the essence of time dependent aspects; rainfall infiltration, ground water flow and its storage time for landslide assessment.

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.

To confirm the characteristic of unsaturated soil at the site (especially the maximum matric suction), field measurement was conducted. Soil-water characteristic curve (SWCC) test is the most general experimental method conducted to determine the characteristic of unsaturated soil.

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.

Thank you for nice comments. Number of investigation conducted around Umyeonsan(Mt) watersheds are different. For the focus of this study, nearest areas around selected watershed have been intensely investigated. There are large margin of error in rest of areas. However, such limitations were minimized by adopting kriging analysis, seismic methods and borehole data. These simultaneously collected data were integrated into point data. Thereby the data from study area are reliable.

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.

The main objective of this study is to develop new landslide assessment model based on GIS. Therefore, this paper focuses on presenting the results of this model. Specific details of investigated data and modeling have been omitted.

Consideration of both rainfall intensity and antecedent rainfall were the key factors in terms of accessing the landslide. Thereby we stated the climate change related with landslide.

Lastly physical modeling method (as well as the heuristic and statistical analysis) was not taken into account the preferential flow paths which is strictly limited to regional factors.