Nat. Hazards Earth Syst. Sci. Discuss., 2, C2121–C2128, 2014
www.nat-hazards-earth-syst-sci-discuss.net/2/C2121/2014/

Natural Hazards and Earth System Sciences Discussions

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NHESSD

2, C2121–C2128, 2014

Interactive Comment

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.

Anonymous Referee #1

Received and published: 22 September 2014

Overall Review

The manuscript combines two well-known models for infiltration and groundwater flow and the "infinite-slope" approach for analyzing a disastrous landslide event occurred in 2011 in the Umyeonsan mountain in Korea. Despite the importance of the topic, e.g., numerical modeling of landslides and of the specific analyzed event, I fail in recognizing the added value of this study in terms of novelty in the modeling component (see major comment 1), as well as the paucity of analysis and discussion of results leaves the reader with the feeling that we cannot learn much from this case study (major comment 2). Furthermore, the way the article is written and the content is organized can be largely improved (major comment 3).

MAJOR COMMENTS

1) I have problems in identifying the novelty of the manuscript which combines the classic "infinite slope" approach for the geotechnical component (Wu and Sidle, 1995; Casadei et al., 2003; Dhakal and Sidle, 2004; Rosso et al., 2006), the Green-Ampt infiltration module and a classic groundwater flow model (as can be found in text-book literature e.g., Brutsaert 2005). All these approaches are well known and their combination in a specific case study does not represent a considerable novelty. This is especially the case considering that landslide modeling literature has been evolved significantly in the last years presenting distributed, time-continuous, process based models, including multiple soil layers (e.g., Baum et al 2010, Liao et al 2010, Arnone et al 2011, Simoni et al 2008) conversely to what the authors wrote (line 13-14 page 5588). Connected with this comment the literature review on numerical modeling of rainfall-induced landslides is extremely poor, there is almost no hint about "landslide modeling" in the introduction. Only in the conclusions (page 5588, line 4-5) the authors mentioned few previous works which are rather old.

2) Another major comment concerns the fact that almost the entire manuscript is dedicated to explain the models or the data. For instance, an excessive length is dedicated to measurements, which are not so important in this article. There is very little space for the results, which will be the most interesting part. Results are marginalized in one page (pp 5586-5587) and in Figure 8 only. Much more can be done in terms of data analysis starting with some rigorous and serious statistical comparison of model results with landslide observations, computing false positive, false negative, true positive, and true negative predictions and using for instance ROC (Receiver Operating Characteristic) curves (Frattini et al., 2008, 2010; Nefeslioglu et al., 2008). Furthermore, more discussion is needed, why somebody should use the approach the authors presented

rather than other approaches, which are the advantages and disadvantages? How it is expected to perform beyond the presented case study? Otherwise, the study will remain very limited.

3) Finally, the use of terms, the structure of the sentences and sometime entire parts are highlighting a strong need to revise the English, not much in the grammar but more in the description and presentation of the content. Exemplary are the titles of section 4.1 and 4.2 that do not mean anything or the very confused Section 6. Many more comments on this regard are given below.

MINOR COMMENTS

Page 5576, Line 1-2. I would delete the first sentence of the abstract. General sentences as this one require a large amount of references to be trusted, plus “rainfall” is missing before patterns.

Page 5576 - Line 16. Starting with such dogmatic statement about climate change without supportive references is at least inadequate. I would leave only the part related to “South Korea”.

Page 5576. Line 21. It is not clear to what “record of heavy rainfall” the authors refer to if to one event or many events occurring during June and July 2011. Please be more precise.

Page 5576. Line 26. I am not sure writing “hazard area” is correct.

Page 5577. Line 4-6. These sentences are repetitive within the sentence and with what written above.

Page 5577. Line 11. Unclear what the “source of rainfall-induced landslide” is.

Page 5577. Line 15. I agree with the authors but please include some reference for this “broad consensus”.

Page 5577 Line18. “impact” is missing after strong.

Page 5577. Line 20. The use of “within” is not correct.

Page 5577. Line 21-24. It is not clear, if the authors are carrying out themselves a study of trends in rainfall patterns or if they want to refer to previous studies. This is clear only later in the section. I would suggest specifying that this analysis is included already in the introduction and definitely writing it explicitly at the beginning of this section.

Page 5577. Line 24-25. Another dogmatic sentence without references, please at least use a more dubitative form, e.g., “is likely to have caused”

Page 5577. Line 26 up to Page 5578 Line 6. As written above this analysis of trend is entering in the paper without any introduction. Furthermore, it is very poor, why only 4 stations are presented? What about the other 52 stations, do they have the same trend, similar, reverse? Which is the magnitude of the trends? Which is the statistical significance of trends? The authors need to do a better effort in presenting in a more scientifically sounding manner this evidence of change in rainfall patterns, which is interesting and relevant for the article, or alternatively refer explicitly to previous literature on the topic.

Page 5578- Line 7-11. Probably this part would fit better in the “Site description” section which is Section 3.

Title of Section 3 is equal to Section 4. My suggestion is to rename it in “Site description”.

Page 5578- Line 25-26. This is part of the caption Figure and not of main text.

Page 5579. Line 5-7. I am sorry but I do not understand the sentence, please rewrite.

Title of Section 4 is equal to Section 3. My suggestion is to rename it in “Field Measurement”.

Page 5579. Line 9-14. This is an example of an extremely complicated sentence to say that “soil hydraulic properties need to be measured.”

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Title of section 4.1 is impossible to understand and overall this section is difficult to follow even though it describes just measurements, furthermore there is no need to describe the actual geotechnical tests carried out if references are provided.

Page 5579. Line 16. SWCC is not defined before.

Title of section 4.2 is again impossible to understand, my suggestion would be “Initial conditions”.

Section 4.2. It is not clear to me the rationale of using measurements of initial conditions in terms of soil water potential carried out in 2012 to initialize the model for the event of 2011. Maybe there is a connection or an explanation but if it is not provided it is difficult to understand the rationale used by the authors.

Page 5581. Line 1. I would be careful in defining the model “physically-based” given that only one vertical layer of soil is adopted and that simplified conceptual approach are used especially for infiltration.

Page 5581. Line 2. I do not understand the reference for the GIS.

Equation (1). Please re-check the equation or provide additional explanation, it seems to me that the unsaturated soil has not been considered in this equation (e.g., see Rosso et al 2006).

Page 5582 Line 14. It is unclear to me what is new on the procedure of Table 2, it seems a standard one dimensional approach to deal with groundwater flow.

Page 5582. Line 17. I am also not sure in how the Green-Ampt conceptualization has been modified, simply because there is also a water-table depth in each cell, it does not mean that Green-Ampt has been modified.

Equation (4). Maybe it would be good to state that the form of Dwn becomes like this because the soil depth H would be in both the nominator and denominator, see also confusion in Table 2.

Equation (6). Written in this form the equation is valid for a steady-state flow of groundwater; however, I understand later in text the authors are evolving though time the groundwater height using flow directions, this needs to be specified better.

Page 5584. Line 19-20. This part ingenerates some doubt on how things are presented. If the authors used equation (6) then the flow directions should be computed from the surface of the water table and not by the "slope direction of the bedrock", in the last case the authors will simply use a "kinematic approach" to solve groundwater flow, e.g., the slope of the energy equal to the slope of ground (in this case bedrock), no need for equation (6) anymore. The latter assumption "kinematic approximation" would be questionable especially in milder slopes.

Page 5585. Equation (11). What is D? I assume the flow direction, please define it.

Page 5585. Line 22. What the authors refer to with "various types of rainfall events"?

Page 5586. Line 18-21. Information about soil depth are not part of the result but should be given in the presentation of the case study. It seems the authors have a lot of data to define a very important information for landslide prediction, this would deserve a more accurate description in any case in the methodological section and not here.

Page 5586 Line 27 to 5587. Line 1. These sentences are somewhat very vague and overall all of the analysis of results need to be carried out in a much more rigorous way (see major comment).

Page 5587 Line 5 -19. All this part is very confusing.

Table 2. In the table, only unitless quantities as water content or difference in water content are given while the text always refers to depths. Please introduce the soil depth H whenever appropriate.

Figure 8. It is very difficult to understand the difference between the two failure types identified by the authors.

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 5575, 2014.