Hydrol. Earth Syst. Sci. Discuss., 11, C6026–C6033, 2015
www.hydrol-earth-syst-sci-discuss.net/11/C6026/2015/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Quantification of the influence of preferential flow on slope stability using a numerical modeling approach" *by* W. Shao et al.

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Received and published: 11 January 2015

General comments

This is an interesting and generally well written paper that investigates possible effects of preferential flow on slope stability using numerical experiments. Relating hydrological processes and slope stability is a relevant topic that is within the scope of HESS. Existing literature on the topic is limited, and especially quantitative studies are rare.

Some points of the study need clarification, and a few missing points needs to be addressed. These are detailed in the following, and in the specific comments below.

Finally, minor technical corrections should be made.

One issue I have to point out is that basic parts of the present paper have also been published as "Shao, W., T. Bogaard, and M. Bakker (2014), How to Use COMSOL Multiphysics for Coupled Dual-permeability Hydrological and Slope Stability Modeling, Procedia Earth and Planetary Science, 9(0), 83-90." This work is sort of a proceedings publication and publicly available, but is not mentioned in the present manuscript. Clear differences between the two works are that the proceedings paper presents benchmark tests of the model for both hydrological and slope stability calculations, while the present manuscript reports much more details on the modelling results and includes two different rainfall and two different cohesion scenarios. So I do not think that this has to be considered prior publication according to the HESS policy, although this finally has to be decided by the editor. In any case, the authors should ensure that they avoid copyright issues. The copyright for the proceedings paper is at Elsevier B.V. and the content is licensed under the CC BY-NC-ND license, so the work at least has to be cited if (unchanged) contents are reused in another paper. Apart from that, I would suggest to include reference to the proceedings paper anyway to point out the benchmark simulations, and further expand on this previous work.

For the present manuscript, I would suggest streamlining the argumentation, and discussing in greater detail the influence of the chosen model setup and scenarios. These include the role of flow paths arrangement, which has not been discussed, or the role of time-variable rainfall, as the rainfall input dynamics in the study were highly idealised. From the hydrology side, the presentation of the results related to infiltration excess after longer rainfall period should be revisited.

The model setup consists of two soil layers with very contrasting hydraulic properties, but with the same mechanical properties, which seems an acceptable choice for a start, but also deserves discussion. A parameter sensitivity study, as suggested by Reviewer 1, would be excellent, although easily going beyond scope of the paper. If it is done, it should ideally also include other mechanical properties like the angle of

internal friction.

The selection of cited papers sometimes appears to be made with the intention to include some less-often cited, recent work, which is a credit to the authors. The literature review on the relationships of preferential flow and slope stability, however, could be made a bit more comprehensive. A quick selection of papers you might want to consider additionally:

- Pierson, T. C. (1983), Soil pipes and slope stability, Quarterly Journal of Engineering Geology, 16(1), 1-11.
- van Asch, T. W. J., J. Buma, and L. P. H. van Beek (1999), A view on some hydrological triggering systems in landslides, Geomorphology, 30(1-2), 25-32.
- Wienhöfer, J., F. Lindenmaier, and E. Zehe (2011), Challenges in Understanding the Hydrologic Controls on the Mobility of Slow-Moving Landslides, Vadose Zone J., 10(2), 496-511.
- Ghestem, M., R. C. Sidle, and A. Stokes (2011), The Influence of Plant Root Systems on Subsurface Flow: Implications for Slope Stability, Bioscience, 61(11), 869-879.
- Handwerger, A. L., J. J. Roering, and D. A. Schmidt (2013), Controls on the seasonal deformation of slow-moving landslides, Earth and Planetary Science Letters, 377, 239-247.
- Wilson, G. V., and G. A. Fox (2013), Pore-Water Pressures Associated with Clogging of Soil Pipes: Numerical Analysis of Laboratory Experiments, Soil Sci. Soc. Am. J., 77(4), 1168-1181.

Specific comments

13056, ll. 15f: “positive effect on slope stability as it drains the water from the matrix domain. . .” – Pore-pressure in the matrix domain was not considered in the stability calculations. This reasoning should be revised.

13057, l.7: What is “the empirical infiltration model”? Maybe I missed that, or perhaps you might like to consider “an empirical infiltration model”.

13057, ll. 10-11: Meaning of the cited reference is not clear; the factor-of-safety concept is much older and treated in many textbooks.

13057, l. 12: Please describe the limitations that are important for your study, and pick that up in the discussion.

13057, ll. 17-28: Perhaps you could put the COMSOL software used in this study into this picture.

13057, l. 28: Not clear why pedotransfer functions are mentioned here. The Beven and Germann citation is not needed twice.

13058, ll. 20f: “Field studies have shown that preferential flow is one of the major mechanisms. . .” Only one study is cited - are there more studies? Does any study really show that preferential flow is the major mechanism for landsliding? In contrast, I rather would say that there are some studies that suggest an important role of preferential flow, and these need to be cited in the paper.

13058, l. 22: Please explain why you consider soil pipes being “minor” flow paths.

13058, ll. 23f: “clearly associated” with slope failure: I do not think that statement is tenable, nor supported by the cited references, since it would mean everywhere where preferential flow occurs, also slope failure occurs. Please also check the references, the Krzeminska et al. and Debieche et al. studies were not conducted in forested areas.

13058, ll. 28f: The given references are not suitable – both are review articles on pref-

erential flow, and neither of them is treating the relation to landslides or slope stability. Please insert suitable references.

13059, ll. 6f: What is the “empirical model”? Did all cited references use the same model? Were Vrugt et al. mainly concerned with preferential flow modelling?

13059, l. 21: Perhaps you might like to cite Shao et al. (2014) here (Shao, W., T. Bogaard, and M. Bakker (2014), How to Use COMSOL Multiphysics for Coupled Dual-permeability Hydrological and Slope Stability Modeling, Procedia Earth and Planetary Science, 9(0), 83-90)

13061, ll. 10f: Please consider moving the definition of the water exchange coefficient from the discussion section here; or rather specify that it was used as a fitting parameter in your study.

13061, ll. 11-14: Would it not be more correct if the water exchange between the two domains would be limited by the lower value of the hydraulic conductivities?

13062, l. 6: Should it not be $i = i_f + i_m$ with $i_f = w_f i$ and $i_m = w_m i$?

13062, l. 13: How is infiltration capacity specified in your model?

13063, ll. 3f: Bishop’s equation has not been mentioned before.

13063, l. 5: As I understand, your model calculates pressure head in the unsaturated zone directly. Why is the reduction by effective saturation needed?

13063, ll. 9-17: Please explain in more detail the difference between the “local factor of safety” and the traditional factor of safety. To me, both appear the same.

13063, l. 17: Do you mean “first and third effective PRINCIPAL stress”?

13064, l. 13: What possible boundary effects did you encounter, or could you think of? Please consider including this in the discussion section.

13064, l. 21: Maybe consider explaining what a “roller boundary” is.

13064, ll. 22-26: Please specify also the time discretization and perhaps information on the numerical solver.

13065, ll.14-17: I do not fully understand the Bogner et al. reference. Do you mean that this is an assumption in your study, or a general fact?

13065, l. 26: What is meant by “non-unique parameter set“? I would expect to get two parameter sets, one for each domain.

13066, l. 19: Maybe consider replacing “seepage outflow” by “return flow”

13066, ll. 25f: As I understand, rainfall rates were constant in your simulations. Why then should rainfall rate exceed infiltration capacity after a while? Or is it saturation excess, which is not a function of rainfall, but infiltration and redistribution? Please differentiate.

13067, l. 12: Please specify “all three boundaries”.

13067, l. 17: Are there different groundwater levels for the upper and lower layers in your model?

13069, l. 20ff: This exchange from matrix to macropores is quite astonishing. Is this realistic? What drives these flows?

13072, ll. 7-9: Prior to this study – remember Shao et al. 2014; also the Krzeminska et al study was in this direction. Maybe you are willing to relax the rather strong statements “systematic” and “fully coupled”, which both are not exactly to the point, since not a wide range of scenarios was analysed, and the influence of slope movement on flow phenomena was not included.

13072, ll. 15-17: Please discuss how explicit representation of flow paths could possibly change your findings.

13072, l. 17: “Several field studies”, but just one is mentioned.

13072, l. 27: Why does the slope has to be forested? How is the influence of trees considered in your study?

13073, l. 16: Also the pressure difference determines the water exchange.

13075, l. 3: Was the Lanni et al. study really concerned with preferential flow?

13075, l. 6: Consider replacing “hazard assessment” with “slope stability”.

13075, l. 7-8: Please include available studies on the effect of preferential flow on slope stability in this discussion.

13075, l. 12: If the complexity is due to the rainfall characteristics, why did you not chose more realistic rainfall scenarios? How would intermittent rainfall change your results?

13075, ll. 16-18: Meaning of “bimodal response” not clear, please explain in further detail and add references. Which effects could not be modelled, for example, with a single bi-modal soil hydraulic parameterisation?

13075, ll. 20-22: But of course it is also very difficult to correctly parameterise a single-permeability model, given the commonly unknown, heterogeneous structure of the sub-surface above the scale of a representative elementary volume.

13088, Table 1: Please include the values for cohesion.

Technical corrections

13057, l. 28: Start a new paragraph.

13069, l. 15: Reword – maybe “pressure difference between domains causes ...”

13072, l. 22: “Numerical simulationS” (“require”, “are”)

13074, ll. 14f: K_{sa} -> K_a ? (also Table 2)

13074, l. 21: Van der Spek . . . showed

13075, l.22: “A coupled”

13075, ll. 23f: Reword ”consequent slope failure area”

13092, Figure 3 : “Left BOttom seepage"; Maybe consider replacing “Surface seepage” by “Return flow”

13097, Figure 8 caption: Check: “Positive values (blue) . . . negative values (blue)”

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 13055, 2014.