Hydrology and Earth System Sciences

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Discussions

Interactive comment on "Analysis of an extreme rainfall-runoff event at the Landscape Evolution Observatory by means of a three-dimensional physically-based hydrologic model" by G.-Y. Niu et al.

Anonymous Referee #1

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General comments:

The paper presents numerical experiments conducted using data from the one hillslope of the Landscape Evolution Observatory (LEO). The concept of such experimental set up is very interesting as it allows testing functioning and modelling hypotheses under controlled conditions. The considered data set corresponds to rainfall simulation at a homogeneous rainfall rate, following by no rainfall. The land surface is bare soil. This first experiment was designed to test the functioning of the installation, but providing

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interesting data, all the more than the observed behaviour was completely different from the expected one, as given by previous numerical modelling. In particular overland flow and the formation of a small gully were observed and were not predicted by previous simulations. The objective of the numerical experiments is to investigate possible reasons for this mismatch. The question is of interest. However, only one general hypothesis, i.e. a possible heterogeneity of the soil hydraulic conductivity at the seepage face is considered, and the hillslope soil is still supposed to be homogeneous. Although the hillslope was artificially built, it is very likely that some soil heterogeneity is present in the soil and may also explain the unpredicted behaviour of the hydrological response. The authors could refer to interesting findings in the artificial Chicken Creek catchment built in Germany (e.g. Hofer et al., 2011, 2012; Hölzel et al., 2011 and more generally a special issue of Physics and Chemistry of the Earth, vol 36 (1-4), 2011). In the present paper, the authors have realised thousands of simulations with different homogeneous soils, but it would also have been possible to test the impact of possible heterogeneity in the soil properties (both horizontally and vertically). In addition, the authors mention the existence of lots of sensors measuring water pressure and water content. It could be interesting to analyse those data before building the hypotheses tested using the numerical model. The impact of possible macropores could also be analysed. The feeling when reading the paper is that the authors try to get good simulations of discharge, but for the wrong reasons. My point of view is that the publication of the results presented in the paper may be premature and that it could be more efficient to first analyse the data more in depth before possible publication of numerical simulations results. In addition, the paper does not detail enough some important part of the experimental design, the model used, his set up and this requires further attention. The reference list is also very short and almost only limited to publications about the LEO. A comparison of the authors results with results from the literature would be welcome. More detailed comments are provided below.

Specific comments:

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1) p.12620, section 2.2. The model description is very short and more information could be provided on the model functioning, numerical discretization, parameters required. Some points on how macropores and or are not taken into account could be useful also. 2) P.12620, section 2.3. More information about how the LEO hillslope was built could also be useful. Was it built to get a homogeneous soil and if yes how was this achieved? Is the rainfall applied over the whole hillslope or only at the top of the hillslope? Where are the soil moisture sensors located? Do you have measurements at several depths? Could you explain better how and where the seepage flow is measured? Is it measured at the bottom of the slope? A scheme with the experimental design could be useful. 3) P.12622, lines 1-9. The specification of the upper boundary conditions is quite rough? Did you made some sensitivity analysis of possible error on this boundary condition? Are you sure that the imposed rainfall is homogeneous all along the slope (if it is applied over the whole slop, see also question in point 2)). 4) P.12622, lines 19-24. Could you specify clearly that in senarii M1 and M2, the soil is assumed to be homogeneous? The Ksat value of simulation M2 is very large. Some comments about the realism of this value would be welcome. 5) P.12623, liens 1-10. The authors mention that they consider heterogeneous configuration, but the way the heterogeneity is taken into account in the model is really not clear. Do you only modify the Ksat of the last layer of nodes (i.e with y value around 60 in Fig .3?). A figure showing how the heterogeneity is considered would help understanding what is really done. 6) P.12623, lines 10-12. Why do you retain such a narrow range for Ksat and Ksat,sf, as compared to the range used in M1 and M2? 7) P.12628, lines 25-28. The authors mention the existence of soil moisture measurements scattered within the whole slope. It would be necessary to assess the relevance of the model simulations/hypotheses with these data, before concentrating on only one functioning hypothesis: soil heterogeneity at the seepage face, but without questioning the hypothesis that the remaining of the slope is homogeneous. 8) Section 4. The discussion should be enhanced with reference/comparison with other studies. 9) P.12629, lines 1-10. This paragraph should come sooner in the discussion. 10) Figure 4. Could the

authors provide more information about the way red dots are obtained? Why are the data horizontal beyond 0.20 m3/m3. If this relates to the explanation given p.12629, lines 5-8, then the data should be removed from the analysis.

References Hofer, M., Lehmann, P., Biemelt, D., Stähli, M., and Krafczyk, M.: Modelling subsurface drainage pathways in an artificial catchment, Physics and Chemistry of the Earth, Parts A/B/C, 36, 101-112, http://dx.doi.org/10.1016/j.pce.2010.04.020, 2011. Hofer, M., Lehmann, P., Stahli, M., Seifert, S., and Krafczyk, M.: Two approaches to modeling the initiation and development of rills in a man-made catchment, Water Resources Research, 48, 10.1029/2011wr010719, 2012. Hölzel, H., Rössler, O., and Diekkrüger, B.: Grope in the Dark – Hydrological modelling of the artificial Chicken Creek catchment without validation possibilities, Physics and Chemistry of the Earth, Parts A/B/C, 36, 113-122, http://dx.doi.org/10.1016/j.pce.2010.04.017, 2011.

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