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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.

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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 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.

R: Thanks for the comments and for the time you have taken to review this paper.

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).

R: LEO was built with homogenous sandy loam and carefully compacted at every 25 cm thickness to explore the co-evolution of the soil-water-plant system from an initially homogeneous soil. This is the reason we started our simulations with the homogeneity hypothesis.

The main purpose of this paper is to explore the possibility of heterogeneity development in the LEO soil and its effects on overland flow generation, while considering the uncertainties in soil property parameters.

We could include vertical heterogeneity in the simulations. However, this would significantly increase computational demands. It took about a week to finish the 20,000 runs presented in the paper. Combining the "vertical heterogeneity" (regarding the uncertainties in soil parameters at different layers) would take about "n" weeks, where n is the total number of combinations of varying parameter values in the vertical direction and can be easily about 1000. We are using a physically-based model based on the 3D Richards equation, which is much more time-consuming than the one developed in Hofer et al. (2012) for the Chicken Creek catchment. Also, including vertical heterogeneity would not alter the final conclusion (that confirms incipient heterogeneity).

However, we do agree that vertical heterogeneity is important in the broader context and that it may have developed in LEO, in this way affecting the internal moisture states. This study was not aimed at investigating where the heterogeneity developed (although we did set different Ksat values at the seepage face) but rather to answer what happened in the LEO soil that produces a different hydrological response (especially the overland flow) from what was expected based on preliminary analyses.

We are currently running the CATHY model to simulate soil moisture at different layers and horizontal locations.

These points are now mentioned in the new Discussion section.

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.

R: A detailed analysis of the soil moisture data of the LEO soil is included in another paper, "Hillslope experiment demonstrates role of convergence during two-step saturation" led by A. I. Gevaert, which has been submitted to HESS-D. We cite this paper to support our modeled mechanism of overland flow production.

The impact of possible macropores could also be analysed.

R: We have added the following paragraph to the Discussion section: "The model we used in this study solves the Richards equation based on Darcy-Buckingham theory, resolving matrix flow and not macropore flow. There are many modeling studies that use percolation theory and other approaches to simulate hydrologic connectivity of macropores to form preferential flow pathways and threshold-like hydrological responses (e.g., Lehmann et al., 2007; Hofer et al., 2011). At this early stage of LEO, with complete absence of organic matter and vegetation roots, we do not anticipate macropore-related processes to be dominant. Macropores might possibly exist around the sensors, although in this case subsurface flow would be enhanced and would very likely have prevented generation of overland flow."

The feeling when reading the paper is that the authors try to get good simulations of discharge, but for the wrong reasons.

R: The main focus of our paper is not on getting good simulations but rather on hypothesis testing, as made clearer in the revised manuscript's the Discussion section: "A thorough investigation of the fine particles at the seepage face or upslope is not feasible as this would alter the soil structure of LEO-1. The physically-based hydrological model used in this study allowed us to make a probabilistic assessment of the incipient heterogeneity hypothesis while considering uncertainties in soil parameters. Under heterogeneous conditions the model produced better results for seepage flow and total water storage, as well as overland flow that is comparable to estimates from a water budget analysis. It was not our intention to improve the modeling accuracy through parameter calibration but to test the hypothesis of incipient heterogeneity development."

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.

R: See above responses that clarify the context and objectives of our study.

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.

R: We added two more references about the model, one on subsurface water and another on surface water routing.

We also revised the experimental design and model setup, and we added seepage face nodes in Figure 3.

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.

R: We added a Discussion section and a broader range of references.

More detailed comments are provided below.

Specific comments:

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.

R: We added more references on the model and some comments on macropores (see above points).

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?

R: Additional information on the hillslope construction and constitution has been added in the Introduction and in Section 2.1.

Is the rainfall applied over the whole hillslope or only at the top of the hillslope?

R: On the whole slope. The paper has been revised to make this information explicit (Section 2.3).

Where are the soil moisture sensors located? Do you have measurements at several depths?

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R: This is not a focus of the paper. Detailed information on this is included in the HESSD paper mentioned previously.

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.

R: We revised Figure 3 and added a sentence in Section 2.1: "Seepage face flow was recorded through six tipping buckets and six flow meters installed at six sections of the seepage face."

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?

R: Yes, we ran many experiments before the systematic runs.

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).

No, we are not sure. We compared the homogeneous rainfall pattern to an inhomogeneous pattern that was measured by hundreds of cups before the first experiment. The difference in the modeled seepage flows between the two cases is negligible. These results are not included in the paper.

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.

R: This is now stated more clearly.

We already have a sentence that explains this value: "M2 uses the same parameters except a greater Ksat (= 3.8×10–3 m s–1) resulting from a calibration against the starting time of measured seepage face flow for a LEO-1 test run with 20 mm h–1 of rainfall applied for 5 h in November, 2012."

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.

R: We revised Figure 3 to convey this better.

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?

R: We actually did more experiments with a wider range of parameter values than described in the paper. We focus our attention only in the narrow ranges because our model generates a feasible overland flow only within this range.

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.

R: As mentioned above this is included in another HESSD paper, and further analyses of LEO experiments using also internal data is ongoing.

8) Section 4. The discussion should be enhanced with reference/comparison with other studies.

R: We added a Discussion section, citing more papers:

"Unlike other artificial large-scale hillslopes such as Hydrohill in China (Kendall et al., 2001) and Chicken Creek in Germany (Gerwin et al., 2009; Hofer et al., 2011), LEO was built with homogenous soil and with a focus on evolving heterogeneity from a "time-zero" homogenous condition through co-evolution of the soil-water-biota system over a time scale of years (Hopp et al., 2009; Dontsova et al., 2009). Development of catchment morphology and soil catena driven by hydrological processes through soil erosion and deposition may be one of the major causes that induce heterogeneity and that in turn exert strong feedbacks on hydrological processes (e.g., Beven et al., 1988; Sivapalan, 2005; McDonnell et al., 2007; Troch et al., 2009). At LEO it was not expected that soil heterogeneity would develop in such a short time period during an intense rainfall event that induced significant subsurface saturated flow. This is one of the main reasons that our pre-experiment model predictions failed to produce overland flow. A thorough investigation of the fine particles at the seepage face or upslope is not feasible as this would alter the soil structure of LEO-1. The physically-based hydrological model used in this study allowed us to make a probabilistic assessment of the incipient heterogeneity hypothesis while considering uncertainties in soil parameters. Under heterogeneous conditions the model produced better results for seepage flow and total water storage, as well as overland flow that is comparable to estimates from a water budget analysis. It was not our intention to improve the modeling accuracy through parameter calibration but to test the hypothesis of incipient heterogeneity development. The model we used in this study solves the Richards equation based on Darcy-Buckingham theory, resolving matrix flow and not macropore flow. There are many modeling studies that use percolation theory and other approaches to simulate hydrologic connectivity of macropores to form preferential flow pathways and threshold-like hydrological responses (e.g., Lehmann et al., 2007; Hofer et al., 2011). At this early stage of LEO, with complete absence of organic matter and vegetation roots, we do not anticipate macropore-related processes to be dominant. Macropores might possibly exist around the sensors, although in this case subsurface flow would be enhanced and would very likely have prevented generation of overland flow. In this modeling study we assume that all soil parameter values vary horizontally and are static during the modeling period. Evolution of heterogeneity due to coupled water and sediment transport processes, which may occur in particular under intense rainfall conditions, is beyond the ability of state-of-the-art hydrological models and requires more attention in ongoing efforts to develop coupled Earth system models. Likewise, soil erosion models that consider only surface processes (e.g., Hofer et al., 2012) are also inadequate to this task."

9) P.12629, lines 1-10. This paragraph should come sooner in the discussion.

R: Yes, we moved this paragraph to an earlier section.

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.

R: We modified the text for Figure 4. We prefer to keep the data in the picture, because the curve is still indicative of the greater n value at least for the relatively dry range.

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