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Hydrology and Earth System Sciences
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 #3

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In this work, the physics based model Cathy is used to investigate the response of an artificial hillslope to the application of a uniform rainfall input. The artificial hillslope (named LEO) is built by using a homogeneous soil. Based on the supposedly well known soil properties and on earlier modelling applications, the rainfall was applied uniformly in space and time to bring the hillslope to a hydrologic steady-state. However, the hillslope never reached the predicted steady-state but instead developed saturation excess overland flow. The work aims to understand why did the observed hydrological response differ so significantly from the predicted response. The answer offered by

the authors is that the experiment itself triggered some form of heterogeneity in the soil hydraulic conductivity of the seepage face. Indeed, when this heterogeneity is included in the model building, the model is shown to be able to accurately reproduce the hydrologic response. The topic is very interesting, in that it shows the hydrological implications of processes which introduce heterogeneity in a supposedly homogeneous environment. The objectives are of interest for the readers of HESS and the writing is good. Nevertheless, the paper lacks focus and a clear story line and suffers from structure.

Lack of focus and a clear story line: The title is misleading. The 'investigation of an extreme rainfall-runoff event' is evidently not the central focus of this paper. The introduction reserves too room for the general description of the LEO experiment, whereas too few is dedicated to establish a link between one main objective of LEO (examination of co-evolution of the physical and biological system) and the incipient heterogeneity which is tested by the field-numerical experiment. There are essentially no conclusions, in the sense that the implications of the obtained results are not even addressed.

Problems with structure: The main problem with the structure of the work is that it poses a very nice question assisted with a formidable experimental structure, and ends up with an answer which is just barely supported by the multiple monitoring means. The authors should use internal data (at least soil moisture data and soil hydraulic data at the seepage face) to add experimental foundation to the numerical simulations, and to reduce equifinality in the answers they are able to offer. Moreover, the authors should at least address what is the main implication of this field-numerical experiments. In my opinion, this can be stated as follows: during the observed event, the hydraulic properties of part of the hillslope evolved from one value to another. The numerical model cannot reproduce this behavior: it is based on use of static values for the hydraulic properties. This is interesting, because it is a process likely occurring in many intense events, and very often forgotten both by experimentalists and modellers. The discussion of the results should include consideration of this implication.

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Considering the general interesting topic I think that the work might be publishable after moderate revisions. In the following I will try to outline, where and how the manuscript can be improved.

Title: The title should focus on the main problem addressed by the work, which is not the investigation of an extreme rainfall-runoff event.

Abstract: The abstract should make clear the meaning of the 'saturated soil compactation near the seepage face'. This is apparently due to the transport of fine sediments during subsurface saturated flow prior the onset of overland flow. Moreover, the abstract should make clear how the heterogeneous model is built. The sentence starting with "We varied the saturated. . ." is central for this, but it is definitely hard to understand.

Introduction: After a short general description of LEO and of its aims, the introduction should describe only the facilities used for the described experiment and the relevant links to the general aims of LEO. This is not the case, and this is where the Introduction must be improved. For instance, the introduction describes the first experiment as a sequence of two artificial rain applications (P4 L18-20), with the second rain application being labeled with deuterium. It announces also that chemical analysis should inform about water transit times. Since the second rain event and the chemical analysis was never executed (or at least is not part of this work), this only adds confusion to the description of the experiment. Even more important: the reader cannot understand from the Introduction if the incipient heterogeneity tested with the field and numerical experiment is a documentation of an already started co-evolution of the system, or it is just due to an accident. Owing to lack of clarity on this, the reader cannot understand why it is important to understand the reasons for the mismatch between predicted and observed hillslope's behavior and which are the potential lessons to learn.

The first LEO Experiment: This text doesn't include information on the hydraulic behavior of the seepage face. Part of this information is instead reported at P14, L18-27, almost at the end of the paper. This last text should be moved into the description of

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the first LEO Experiment, to provide ground to the choice to decrease the saturated hydraulic conductivity for the seepage elements of the computational mesh. Also, at P7, L8, it is stated that "total mass change, total seepage flow, and soil moisture at 496 locations were recorded every 15 min during the experiment". However, the authors only use total mass change and total seepage flow in the analysis. They should make clear why the use of soil moisture data is considered inessential to better clarify the hydrological functioning of the hillsope. Otherways, they should use these data to shed light on the comparison between observed behaviours and model results.

Model setup: a figure with the description of the mesh organization should be reported to help the reader to understand how the Homogeneous and the Heterogeneous model simulations were built. This distinction is key to understand the model results; however it is left to three mere lines (P9 L 2-4) where the unclear term Ksat,sf is reported. This variable is never defined in the text. Also, it is difficult to locate the mesh grids where the conductivity was modified.

Modelling results: as reported above, one key implication of this work is that during the observed event, the hydraulic properties of part of the hillslope evolved from one value to another. The numerical model cannot reproduce this behavior: it is based on use of static values for the hydraulic properties. The discussion of the results should include consideration of this implication. In the current text, this is done only at P10 L16. Moreover, at P13 L2-4 the authors report: "With the large conductivity of the LEO soil (e.g., Ksat = $1.4 \times 10-4$ ms-1 upslope of the seepage face for the optimal M4Hetero simulation), the overland flow generation mechanism is saturation-excess". This key statement should be supported by use of soil moisture data.

Discussion and conclusion: This section falls short and fail to discuss the implications of this work. Here the authors really need to extend the discussion identifying pathways for future work. Why is the work relevant to the analysis of co-evolution? What moves it beyond the status quo in the analysis of events which are able to modify the constituent soil properties? Why should someone cite this work? I expect more from a HESS

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paper.

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