Hydrol. Earth Syst. Sci. Discuss., 10, C7655–C7658, 2014
www.hydrol-earth-syst-sci-discuss.net/10/C7655/2014/

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.

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Received and published: 2 February 2014

This is an interesting paper that points to the enormous potential of the artificial hillslope laboratory, LEO. The results of the first experiment conducted in LEO are already interesting, however the description of the results has the effect of guiding the reader to more traditional questions. This is unfortunate, and I hope the authors can fix this problem in a resubmission at the end of this discussion.

First of all, when viewed one way, the results show that, however well-conceived, one

can never achieve perfect homogeneity in the real world. However, it is clear that this was already expected by the developers of LEO, in that the focus of the experiment on not reproducing the real world, but on exploring how heterogeneity evolves over time, indeed how hydrological variability and landscape heterogeneity co-evolve. The discussion of the results already indicate that this is already happening, in that the authors are ascribing differences between model predictions and actual observations to this emerging heterogeneity, explaining the compaction (even relative compaction) of the soils even as the experiment is happening.

R: Thanks for the comments and for your time in reviewing the paper.

Given all this, the focus on characterizing on the errors between outputs from various model configurations and actual observations, gives the impression that they are merely asking traditional questions, i.e., fitting a hydrograph, in this case for just one event. I am not against these details, as the modeler still must get the model to mimic the observations, and there is certain amount of equifinality in this fitting.

R: We revised the text everywhere in the paper to reflect that this is not a traditional calibration study. We wanted to address the impacts of soil property uncertainties on our conclusion about heterogeneity development, resulting in a probabilistic assessment of heterogeneity. We added a paragraph in 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."

However, I would have found the results more informative, for this event and for the

best parameter combinations of the model, some deeper insights into the internal dynamics that led to the hydrograph that was observed. For example, the dynamics of the groundwater table during the event, the soil moisture, and the saturation area etc. would provide more insights. Note that it is here that LEO is most innovative and helpful compared to real world field experiments, the ability to observe the space-time dynamics of water partitioning. Also, any additional information on change of structure and heterogeneity will also be insightful, and will shift the focus in the appropriate direction.

R: A detailed analysis of the soil moisture data has been included in another paper submitted to HESS-D, entitled "Hillslope experiment demonstrates role of convergence during two-step saturation" led by A. I. Gevaert. We cited the paper to confirm the saturation-runoff generation mechanism as simulated by our model.

Another comment on the presentation: from the beginning the authors framed the aim of the paper as hoping to explain the big difference between the observed and predicted hydrographs. This is the valid approach: however, towards the end the paper veers away somewhat from this goal. I was expecting a clear, conclusive statement on the causes of this difference, and I did not find it. They may want to make sure to go through the entire paper and ensure the main message is carried through to the end.

R: Thanks for the suggestions. We revised the paper significantly from the title all the way to the conclusion.

One final question/suggestion: the title has the word "extreme rainfall-runoff"> What is the motivation for this phrase?

R: We changed the title to "Incipient subsurface heterogeneity and its effect on overland flow generation – Insight from a modeling study of the first experiment at the Biosphere 2 Landscape Evolution Observatory"

Do the authors think that the event studied is extreme as to cause the erosion that happened? The 12 mm/hr intensity does not sound like too extreme to me.

R: We think this event is extreme by the total amount (264 mm) of this single event, generating significant subsurface flow and overland flow. Now we call it "an intense rainfall event" in the paper, though it is not very intense in intensity.

Another question/suggestion about the title: it might be better for the title to reflect the main message coming out of the paper. As it is now, the title is somewhat neutral, and does not attract attention to the main question/issue that is really highlighted in the paper.

R: Thanks for the suggestion. We changed it (see above).

Overall, I like this paper and would like this paper to be eventually published in HESS. I would prefer if the paper undergoes some (perhaps moderate) revisions to address the concerns raised above and attract sufficient attention to some really important issues in hydrologic process understanding and distributed modeling.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 12615, 2013.