

Response to Reviewer comments for manuscript HESS-2016-228, “Multiresponse modeling of an unsaturated zone isotope tracer experiment at the Landscape Evolution Observatory”, by Carlotta Scudeler, Luke Pangle, Damiano Pasetto, Guo-Yue Niu, Till Volkmann, Claudio Paniconi, Mario Putti, and Peter A. Troch

Summary:

This manuscript provides detailed comparisons of multiple hydrologic response variables using a sophisticated integrated hydrology model and highly controlled experiment at the Landscape Evolution Observatory. The authors experiment with different levels of complexity within the model and demonstrate the importance of model heterogeneity if the goal of the model is to match spatially distributed points as opposed to integrated responses. Results also indicate the importance of considering more than just integrated hydrologic response variables when determining model parameters.

Recommendation:

Overall I find the paper to be well written. I think it provides an interesting comparison of a state of the art experiment with state of the art modeling that will be interesting to the hydrologic community and should be published in HESS. I find their scientific approach to be sound; however, I do think that some changes to the manuscript to better outline all of the test cases and highlight differences would make the discussion easier to follow. I also think that the manuscript would be of broader interest if the authors would devote some discussion the relevance of these findings to other commonly used or similar modeling approaches. I have provided detailed suggestions to this effect below.

We wish to thank the Reviewer for the attention to our work. The comments raised by the Reviewer are addressed below.

Major Comments:

1. The introduction is focused on the need for multi objective parameter optimization. This is a good motivator for this work, but also the study is not really presenting advances for parameter optimization. Rather it's evaluating the impact of different parameterizations on model response. Therefore, I think it would be helpful to provide more background on heterogeneity and variably saturated flow processes and the state of the practices for both modeling and observations. I think this would provide a better context for where both the modeling and observations used here compare to previous work.

1) We agree with the reviewer that the paper is not really about parameter optimization. In the same sense, neither is it really about heterogeneity, as we do not conduct a systematic analysis based on complex configurations and innumerable realizations. But parameter estimation and heterogeneity are certainly underlying themes of the paper, and we agree that a mention of heterogeneity in the context of variably saturated flow processes is warranted. In the Introduction we will add a sentence on this and include additional citations. Lines 7-9 of page 2 will become: “... to more complex models. Traditional challenges, on both experimental and modeling sides, are associated with soil heterogeneity, variability in parameters, and variably saturated conditions (e.g., Binley et al., 1989; Woolhiser et al., 1996; Neuweiler and Cirpka, 2005; see Reference below). An added source of

complexity arises when passing from flow modeling to flow and transport modeling (e.g., Ghanbarian-Alavijeh et al., 2012; Russo et al., 2014).” See also reply to specific comment 1 below.

2. *I would appreciate more details on why the observational experiments were setup the way they were. For example, how were the rainfall rates and timing determined?*

2) We will add in section 2 just after the sentence ending “...precipitation at rates between 2 and 40 mm/h.” the following additional description of the rain system: “Each landscape at LEO has 5 independent plumbing circuits, each including a different array of sprinkler heads, and therefore generating a different irrigation flux.” And in section 3.1 we will add after the first paragraph the following new paragraph providing additional details on the rainfall rates and timing: “At the time of this experiment we consistently used one plumbing circuit because the spatial distribution of irrigation produced by this circuit had been well characterized by in situ testing. This allowed us to examine the possible influence of spatially heterogeneous irrigation patterns on flow and transport. The purpose of the first irrigation application was to increase the average moisture content of the landscape, which had received no irrigation for more than 40 days prior. The second irrigation application was used to introduce the deuterium tracer. No additional irrigation was applied for multiple days so that the tracer transport within, and out of the landscape, would be affected by soil-moisture redistribution and evaporation. The third and final irrigation application was applied with the intention of forcing additional tracer mass beyond the seepage face boundary, to reveal additional detail in the measured breakthrough curve. In retrospect, and following laboratory analysis that spanned several weeks, we only observed the initiation of the tracer breakthrough curve at the seepage face.”

3. *It can be hard to keep all of the different simulations setups straight throughout the paper. I think this could be addressed by expanding on Figure 2 to better label different aspects of the domain that are discussed in the model setup and creating a new table or conceptual model that summarizes all of the runs in one place.*

3) In Figure 2 we will indicate the seepage face and the atmospheric forcing boundary. The revised figure is shown below. The only thing that differs in the model setup amongst the different simulations is the treatment of the atmospheric boundary. With the introduction of a new table containing the boundary condition setup for each simulation it should be easier to follow the setup of the different simulations (see response to specific comment 7 below).

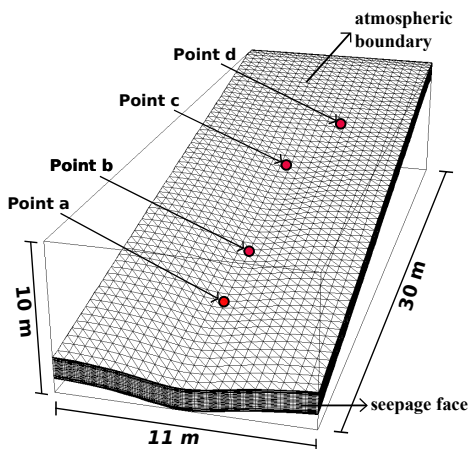


Figure 2. 3D numerical grid for the LEO landscape. Points a, b, c, and d are the locations where samples were extracted during the experiment for subsequent laboratory analysis.

4. *The discussion of differences between basins is mostly qualitative. I think some additional figures that plot differences between scenarios for key metrics and discussion points would strengthen the conclusions.*

4) We are not sure we understand what is meant by 'differences between basins'. Does the reviewer mean the 3 different LEO hillslopes? The experiment being analyzed in this paper was conducted on just one of the 3 hillslopes. This is mentioned in the first sentence of section 3.1 (“...performed at the LEO-1 hillslope ...”) but we agree that this is not at all very clear. We will add the following clarification after the first sentence of the last paragraph of the Introduction: “Both of these experiments were performed on the first of the three hillslopes at LEO to be commissioned, hereafter referred to as LEO-1.” We believe that the revisions to the main text and figures that we are bringing to the paper, in response also to the other two reviewers, will help strengthen the conclusions (see, for example, response 3 to Reviewer 1 and response 6 to the specific comments of Reviewer 2).

5. *This study uses the CATHY model, but it focuses on addressing larger questions in model uncertainty and parameterizations. Given this goal I think some additional discussion on the degree to which these results are specific to the model you are using or would be universal to other integrated flow and transport models would be quite helpful.*

5) We agree with this remark and will add the following new paragraph at the end of the Discussion section: “The broad results of our study should be quite universal, particularly to deterministic numerical models based on the 3D Richards and advection-dispersion equations. However, any model has its specific features and differs, for example, in the way equations are coded (e.g., choice of numerical solvers) or interface conditions are implemented (e.g., free-surface vs boundary condition switching). For insights on the impact of specific model differences in the performance of CATHY-like models, see the intercomparison studies of Sulis et al. (2010; see References below) and Maxwell et al. (2014; already cited in the paper). These intercomparison studies have thus far focused only on flow processes, and there is an urgent need to extend the analyses to solute transport phenomena, in order to properly guide our assessment of the physical and numerical correctness of competing models as these models continue to increase in complexity. For instance for this study there are aspects of the CATHY model related to how we implemented evaporation and fractionation that might be expected to negatively impact the generality of our findings, although in terms of isotope tracer mass exiting the seepage face the impact was quite small. But the implementation here was somewhat ad hoc, and more study is needed on the importance and proper representation of fractionation in solute transport models, especially under strongly unsaturated conditions.”

Specific Comments:

1. *Page 2, line 8: Please expand on this point. What do you mean by ‘an important example of this complexity’? Are you saying that parameter estimation has been particularly challenging for mass transport?*

1) We are alluding here to the added complexity (more equations, more parameters, etc) when passing from flow to flow and transport modeling. We will clarify this sentence to: “An added source of complexity arises when passing from flow modeling to flow and transport modeling (e.g., Ghanbarian-Alavijeh et al., 2012; Russo et al., 2014).” This sentence is also made clearer with the new sentence added just before this one (see reply to major comment 1 above).

2. *Page 3, line 6: Clarify, “infrastructure” for what?*

2) We will change this to “research infrastructure”.

3. Page 3, line 10: From this description it sounds like a simple sloping slab but from Figure 2 it appears that it is actually a tilted v sloping to the center of the domain. Please clarify. Also you could annotate the slopes on Figure 2 to make this even more clear.

3) In fact LEO consists of three v-shaped hillslopes. The average slope of each landscape is 10° , as stated in the paper, while the local slope varies from upslope positions to the convergence zones, with maximum slope of 17° near the convergence zone. Since it is difficult to incorporate this information graphically in Figure 2, we will add this information to the text, in section 2.

4. Page 4, line 2: You should clarify that you are talking about just the rain from the first event here not ‘all the rain water’.

4) We are talking about all rain water. The confusion is perhaps due to the phrase “... and generated seepage face outflow that started after 5 h” at the end of this sentence. We propose to split this sentence in two: “All the rain water applied infiltrated into the soil. Seepage face flow started 5 h after the beginning of the experiment.”

5. Page 4, line 2: Also here you switch from using the term ‘irrigation’ to ‘rain’. It will be easier to follow if you pick one term and stay consistent.

5) We agree that this is inconsistent and may cause confusion. Since we use the terms “rainfall” / ”rain” / ”precipitation” more than “irrigation” in the paper, we will use these former terms exclusively.

6. Page 7, line 14: Please expand here to clarify how you decided on this lateral resolution.

6) In the revised manuscript we will explain why we have chosen this discretization (see our response 23 to Reviewer 1, comment 12).

7. Page 7, line 25: This is a very dense and long sentence. In my opinion it would easier to follow and refer back to if this information were provided in the form of a table. Also, if you keep this in paragraph form you should tie the three numbered experiments listed to simulations a-f in Table 3?

7) We will add the suggested table (it will become Table 1 in the revised manuscript), shown below, and we will revise (shorten and simplify) the paragraph in question.

Simulation (see Tables 4 and 5)	Rain with ^2H -enriched water (second pulse)		Rain with no ^2H -enriched water (first and third pulses)		Evaporation (between rain pulses and after the third pulse)	
	Flow	Transport	Flow	Transport	Flow	Transport
a-f, g-i	$q_n^f = -12 \text{ mm/h}$	$q_c^t = v \cdot v c^*, c^* = 1$	$q_n^f = -12 \text{ mm/h}$	$q_c^t = 0$	$q_n^f = 5 \text{ or } 3.9 \text{ mm/h}$	$q_n^t = 0$
j	$q_n^f = -12 \text{ mm/h}$	$q_c^t = v \cdot v c^*, c^* = 1$	$q_n^f = -12 \text{ mm/h}$	$q_c^t = 0$	Sink q (Table 1)	Source f_c (Table 2)
k	$q_n^f = -12 \text{ mm/h}$	$q_c^t = v \cdot v c^*, c^* = 1$	$q_n^f = -12 \text{ mm/h}$	$q_c^t = 0$	Sink q (Table 1)	Source f_c with $f_c = -q_c$

Table 1. Treatment of boundary conditions at the land surface during the rainfall and evaporation periods for the flow and transport models.

8. Page 8 line 16: How did you determine the 38cm depth for evaporation? This seems arbitrary.

8) This is a point that has also been raised by another reviewer (see our response 6 to comment 2a of Reviewer 1). The parameterizations were chosen in order to qualitatively reproduce the experimental results obtained by Barnes and Allison [1988], where it is shown that, for isotope profiles in unsaturated soil and under evaporation, the maximum concentration can also occur at 50 cm from the surface. Above this point the isotope concentration decreases rapidly towards the surface due to the diffusion of water vapor to the soil surface. In our model we assume that the region dominated by water vapor diffusion is also the one characterized by evaporation, and selected 38 cm for the threshold. We will describe this better in the revised manuscript.

9. Page 9 line 6: It would be helpful to have visual on your model figure for where the seepage face is occurring.

9) In the revised manuscript we will show graphically, in Figure 2, where the seepage face is set. The revised figure and caption are shown above (see response to major comment 3 above).

10. In my opinion the source sink terms listed in Tables 1 and 2 would be more easily interpreted graphically. Alternatively, I'm not sure that this information is necessary for the interpretation of the results as long as you describe how you got these terms so potentially these tables could also be deleted.

10) We agree and will replace Tables 1 and 2 with the figure shown below.

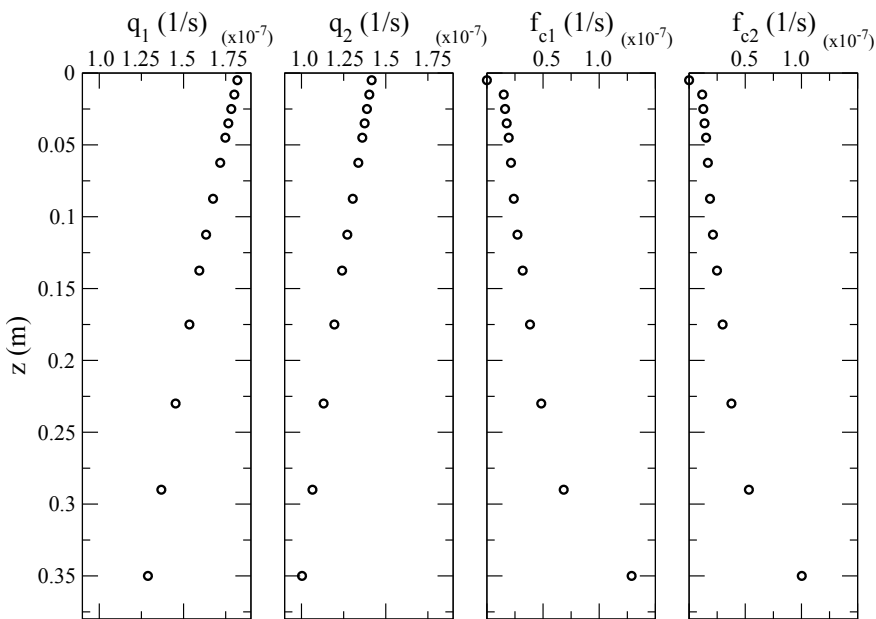


Figure. Sink term q and source term fc over depth z added to the flow and transport equation, respectively. q_1 and f_{c1} are applied between rain pulses 1, 2, and 3, while q_2 and f_{c2} are applied after rain pulse 3.

11. Table 3: Why is simulation e repeated twice in this table

11) This is a mistake, the last one should be “f”.

12. Table 4 is difficult to follow. I think you need a separate table describing the setup of runs g-l and then

report only the output metrics in this table. Also, it might help to just focus on runs g-l here and add the information for simulations a-f to Table 3.

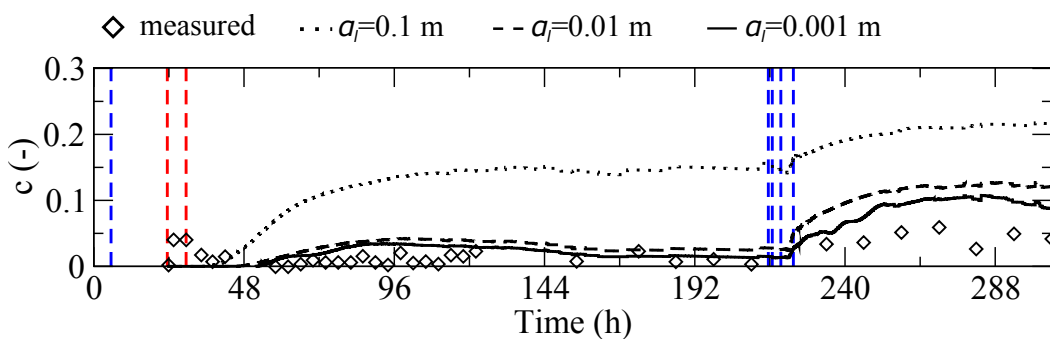
12) We think it is important to keep Table 4 since it summarizes all simulations performed and makes it easier to follow the text in both the Simulations performed subsection and in the Results section.

13. Figure 3: Please describe what ‘simulated, preceding case’ means in the caption.

13) The “preceding case” results will be removed from this figure (see our response 29 to Reviewer 1, comment 18, that also includes the new figure and caption).

14. Figures 4, 6 and 8: I think the diamonds for the measured values should be smaller so that they are not overlapping each other or the axes so much.

14) We agree. We show below the new Figure 4 with smaller diamonds for the measured values. We will do this also for Figures 1, 6, 8, 12, and 13.



References

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