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

Overview

The manuscript describes in detail efforts to fit a variably-saturated flow and convection-dispersion transport models to a very highly controlled field-scale experiment in a 1:1 physical analog of a hillslope. The physical models and numerical approximations are described in mathematical terminology (e.g. zero Neumann for no flow boundary condition etc.) yet readable for a wide community of hydrologists. The beauty of the paper is in the clear description of the need to increase the complexity of the model in the process of fitting first the integrated flow response (in which unique transient observations of seepage face flow-rate, and total storage of water are available in this experimental system) than the integrated transport response, than further complexity is needed to fit point observations of water content and concentrations. The model does not go very far with complexity, it starts in uniform hydraulic properties, moving to different properties near the seepage face and layered porous medium but does not go further to variability within layers, or mobile immobile formulations etc.. Hence, the well-known, good fit of the macro phenomenon relatively to poorer fits to point observations is described very clearly.

Recommendation

I am not sure there is completely new modeling knowledge here, nevertheless, the paper has “educational quality” for hydrological modelers as well as very unique experimental data (although not in focus in the manuscript), and therefore, I warmly recommend publication in HESS, following the authors pay their attention to the comments herein.

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

Major Comments

1) Title – The hillslope problem as well as the model used here and the results of the experiment, are variably saturated rather than unsaturated (saturation at 85 cm for significant duration of the experiment in most locations, Figure 11). Suggest to change to: Multiresponse modeling of variably saturated flow and isotope tracer transport in a hillslope experiment at the Landscape Evolution Observatory

1) We like the title proposed by the Reviewer and will adopt it, changing only “in a hillslope” to “for a hillslope”.

2) Discussion - In line with the previous comment. I don’t understand why the authors do not discuss the more specific setup of a hillslope that was studied here, rather than concentrating on general unsaturated flow. The hillslope case has significant differences than the general unsaturated zone (variably saturated, lateral flow component dominant, relations with evaporation and runoff etc.). Many simulation studies of hillslopes can be discussed (e.g., Fiori and Russo, 2008 WRR).

2) In the Discussion section we wished to draw attention to the numerous challenges in modeling solute transport (rather than flow) phenomena in the unsaturated zone, given that this is the area that in our opinion raised the most difficulties – physical and numerical – in our modeling of the LEO experiment, in particular with regards to capturing point-scale responses. We did not make a specific

distinction between field, hillslope, and (small) catchment-scale studies, as we feel that the issues and proposed explanatory hypotheses (last paragraph of the Discussion) apply across the board. Nonetheless, taking the cue from the Reviewer's concern about a hillslope focus and the mention of the Fiori and Russo paper, we will add a mention of transit time distribution research at the hillslope scale, as this is an excellent example of the need for better modeling of flow and transport dynamics and a very active area of current research. The first two sentences of the Discussion section will thus become three and will read: "Mass transport in unsaturated soils is extremely important in the context of biosphere, critical zone, and Earth systems research because of exchanges of water and solutes that occur across the land surface interface. The study of hillslope transit time distributions (e.g., Fiori and Russo, 2008; Botter et al., 2010; Heidbüchel et al., 2013; Tetzlaff et al., 2014) is a good example of the need for a better understanding of such water and solute exchanges and the consequent subsurface flowpaths. The simulation of unsaturated zone mass transport phenomena is however known to be a particularly complex problem, …".

3) List of symbols – There are many symbols in equations and within the text. For example, it took me too long to find what does the nee in line 27 page 7 stands for. I suggest adding a list of symbols at the beginning of the paper.

3) We do not think a list of symbols is warranted for a standard-length paper. We will however examine all the places in the paper where symbols are used, and replace or supplement these symbols with the variable names to serve as reminders and thus to make the paper more readable.

4) Use of the term heterogeneity – is misleading. Changing a homogenous model deterministically to have lower Ks near the seepage face, or different hydraulic properties at different layers doesn't make it a heterogeneous model (a term now used for a medium in which the properties vary from pixel to pixel randomly usually constrained to a PDF and a spatial correlation function). I suggest describing this type of additional complexity with different (more explicit) terms (e.g. low Ks at seepage face, layered n(vg) etc.), throughout the text, tables and figures.

4) We agree with this comment and will make the suggested changes throughout the paper (including the figures and tables).

5) Fractionation? - in water isotopes during evaporation. The term fractionation is brought up late in the methods section (page 8) as if it is totally trivial. I suggest to add a paragraph on fractionation of water isotopes during evaporation in the introduction to introduce the topic before jumping into the details of dealing with modeling it in the methods section.

5) Although we totally agree that fractionation is not a trivial topic, it is nonetheless not a main topic of the paper. We therefore prefer not to emphasize it too strongly in the Introduction, as this would entail having to also describe the other configurations that were tested (this is all done in Section 3.3). We thus propose, rather than adding an entire paragraph on fractionation, to add the following sentence at the very end of the Introduction: "The boundary condition configurations, for instance, includes a sink-based treatment of isotope fractionation to allow only a portion of the tracer to evaporate with the water."

Although we totally agree that fractionation is not a trivial topic, it is nonetheless not a main topic of the paper. We therefore prefer to introduce it as is currently done, in Section 3.3 as one of the surface boundary condition options that we investigate.

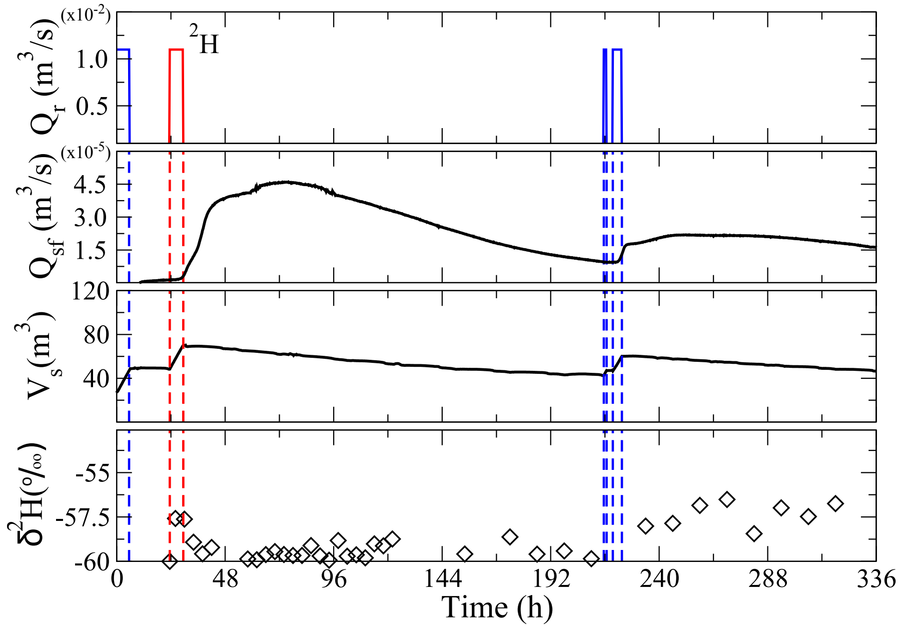
6) Van Genuchten (1985) - should be van Genuchten (1980). It would have been a specific comment for any other paper in hydrology (p. 6, l. 15 and in reference list).

6) Thanks for catching this. The error maybe crept in because we often cite the van Genuchten and Nielsen 1985 paper for these constitutive relationships. We will correct "1985" to "1980".

Specific Comments

1) P. 4, Figure 1. a) Lowest pane (delta2H) – zoom into the interval of interests in the vertical axis (< 53); b) say something on the high readings at the beginning before tracer introduction, and just before the third rain pulse. Or looking at Figure 4 there seems to be a shift of the data to the left? Solve the problem, explain.

1) Indeed there is an error in Figure 1, thanks for making us realize this. The delta^{2}H graph should be shifted by 23.5 h, as is evident from Figures 4, 6, 8, and 13. The corrected figure is shown below. The high readings straight after the beginning of the tracer introduction is a point that was also raised by the Reviewer 1. The peak in the early seepage face flow is probably due to the fact that the residual soil water in the landscape prior to irrigation had become somewhat enriched in deuterium (compared to the irrigation water) during evaporation and reflects some mixing of the new irrigation water with the evaporatively-enriched residual soil moisture. We will add this information to the section of the paper that describes the experiment. Note that in the corrected graph there is no longer a high reading before the third rain pulse. As suggested we have also rescaled the y-axis in this graph.



2) P. 5, l. 14, Eq. 2. I suggest to add the sink\source term – f(c) to the 2H transport equation here as well, rather than only elaborating on it in table 2 and related text.

2) Thanks for the suggestion. We will add two terms in the transport equation: qc^* [M/TL^3], with $c^*=c$ if q (the source/sink term in the flow equation 1) is a sink term, otherwise an imposed source concentration, and f_c , which is a generic source/sink term (our correction term used to model fractionation).

3) P. 7, l. 27. Shouldn’t the left hand side of the equation be n (or theta)*v*nee, rather than only v*nee (porous medium approximation of ratio of flux and velocity).

3) The boundary term arising from the finite element P1 Galerkin discretization of equation 1 (at the left hand side of the = sign) is:

$$-\int_{\Gamma_f} K_r(\psi)K_s(\nabla\psi+\eta z)\cdot\nu\,\mathrm{d}\Gamma_f$$

where Γ_f is the Neumann boundary of the domain for the flow. Thus, the equation in line 27 is correct. You can find more details on the flow equation discretization in Scudeler et al. (2016), cited in the paper.

4) P. 10 l. 5-8. Excellent lines – don’t touch, makes it so much easier to follow the long descriptions after.

4) Thanks.

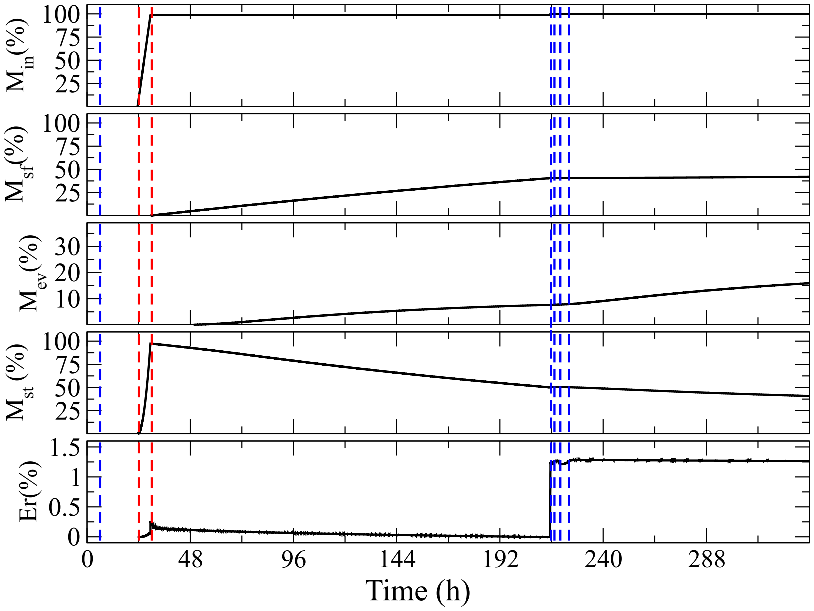
5) P. 11, l. 11. The evaporation rates – were they calculated from the water balance and the load cell data? Or how? Please elaborate.

5) The evaporation rates were calculated from the seepage face measurement and the load cell data. In particular, being V_{sf} the cumulative volume flowing out from the seepage face between two events or after the last event until the end of the experiment (information directly obtained from the flow meter measurements), being dV the change in water volume between two events or after the last event until the end of the experiment (information directly obtained from the load cell data), being dT the time interval between two events or after the third event until the end of the experiment, the average evaporation rate was calculated as $(-V_{sf}+dV)/dT$. We will add this explanation on page 4 after line 10.

6) P. 15 Figure 4. A) Say something on the early breakthrough during the heavy isotope injection. B) Elaborate in the text why was the high dispersivity simulation so much biased upwards in the mass of tracer exiting the system (earlier arrival times are expected in high dispersivity but also late ones. What were the left-in-storage or evaporated components of the mass balance in the high dispersivity run?

6) A) Do you mean for the measured response? If so we have answered this point above (see response to specific comment 1) and we are going to add that information in the revised manuscript. It is also true that the model response presents an early breakthrough after the injection of isotope as we have nonzero solute concentration at the seepage face also after the second pulse but the values are not as high as after the third pulse. These early breakthroughs may be due to dispersion effects. B) We show below the mass balance results for the simulation relative to the 0.1 m longitudinal dispersivity case. At the end of the simulation almost 40% of the mass injected has flown out from the seepage face, 16% has evaporated, and 42% has remained in storage (compared to, respectively,

~4%, 52%, and 42% of the α_l=0.001 m case). 0.1 m is 1/10 of the depth of the hillslope, compared to 0.001 m and 0.01 which are only the 1/1000 and 1/100, respectively. The effect of the high dispersivity makes the solute percolate down quickly to then flow out of the domain from the seepage face boundary. As a consequence it is also less exposed to evaporation. We would expect another breakthrough with a fourth pulse of rain. Unfortunately, no measurements were taken after 336 h. We will not add the figure below to the paper but we will discuss a little bit more in detail the results for the 0.1 m case.



Simulated mass balance results for the α_l=0.1 m case.

7) P. 16, Figure 6 and related text: solute is not a proper term for ^2H.

7) We agree, hydrogen isotopes are not solutes. In the revised manuscript we will no longer refer to deuterium as a solute.