General

We are grateful to the editor and two reviewers for their time and effort and for giving us the opportunity to further improve the manuscript “Hillslope-scale experiment demonstrates role of convergence during two-step saturation”. The reviewers’ comments have been constructive and insightful and have helped us make improvements. Below we provide point-by-point responses to the comments made by the referees.

Response to referee #2, report #1

Specific comments:

1. *Why soil moisture content decrease with depth. The authors also cannot give a true explanation without measurement of the soil hydraulic characteristics from the artificial catchment. Usually, soil cores drilling are necessary step for artificial hillslope experiment though the soil volume is backfilled under controlled conditions, e.g., the volume density keeps constant.*

    We agree that taking soil cores would allow us to measure soil hydraulic properties across the hillslope and could (at least potentially) help us understand the reason why we observe decreasing water content with depth. But even without taking the hillslope apart, we should be able to formulate hypotheses for this unexpected behavior, especially since we have so much data to analyze. We’ve formulated several in the manuscript, and are currently working on another study that focuses on the wetting front behavior. This study involves additional experiments at our and other experimental facilities. It is important to note that for LEO, this was just the first of a long series of experiments that will be performed on the hillslopes to study the development of hydrological pathways and that it is therefore essential to minimize disturbances such as drilling soil cores.

2. *Soil water content exceeds the maximum porosity in Figure 5. Does such results happen to all the sensors, and if you have calibrate them before setting up for observations? As it can significantly influence your conclusions in that it determined overland flow through water balance analysis.*

    Values exceeding maximum porosity were observed at all locations that were saturated during the experiment. This has been made clearer on P.7, L.1:
    “The time series in Figure 5a as well as the time series of all other sensors that reached saturated conditions during the experiment show…”

    Before sensors were installed in the hillslope, the soil moisture sensors were calibrated by exposing the sensors to typical soil moisture conditions. Since the previous version of the manuscript, Decagon recalibrated the sensors, this time including values close to but not up to saturation. Again, no overshoot was observed and the calibration curves did not change significantly. A reference to this recent recalibration has been added to the revised version on P.7 L.5-7. However, separate tests have shown that when submerged under the water table (fully saturated conditions) the sensors yield values above porosity. This is currently under investigation with Decagon, who have no idea as to why this happens..

When we applied a maximum value to the soil water content to exclude the values exceeding maximum porosity from data analysis, we improved the storage estimates compared to those estimates derived from the load cells (Figure 5b). We agree it was not clear that we enforced a maximum value during the water balance analysis and we have added this information to the revised version (P.8 L.18-20).

Response to Referee #3, report #2

I can tell that the authors have made efforts to revise the manuscript based on review comments to the previous version, and I appreciate the improvement in the current version. However, I still have some comments, mainly in the discussion on the runoff mechanisms.

1. *P.12 L.10-15: This is not clear to me. I don't see necessarily a conflict here. The gravitational flow rate, in this conceptual manner, is not determined by the lower storage deficit. It is only determined by the availability of water from the upper layer. If multiple layers are considered in such conceptual models, water can fill the tension water reservoirs from the top layer to the bottom layer, and then fill the free water reservoirs from bottom to top.*

    We agree that this passage is not clear and leads to confusion, so we have removed this part from the discussion. We feel that the fact that the conceptual models do not allow convergent flow is more important, as convergent flow was the main driver of overland flow generation in this experiment.

2. *P.13 L.19-25: It is more likely due to the change of porosity with depth. One reason could be the bulk density increase with depth due to consolidation. Another reason may be fine particles being flushed down to fill pores in the lower layer. An increase of hydraulic conductivity with depth seems unlikely to occur in an originally uniform soil profile. Have you measured soil properties at various depths? It will be more helpful to discuss possible mechanisms based on measured data.*
   Even though this observation is apparently unexpected, I don't think the observation and any of the potential mechanisms go beyond the framework of the basic theory of vadose zone hydrology.

    A decrease in porosity with depth could explain decreasing soil moisture with depth in the second phase. Porosity data of soil cores taken from barrels in which soil material was compacted in a similar fashion do not show a clear relation between porosity and depth. We do not have data from the hillslope itself, so we tested the hypothesis of decreasing porosity with depth by looking at the storage estimates similar to those shown in Figure 5b of the manuscript. If we allow the porosity to vary between the maximum porosity and the lowest porosity measured in the soil cores (respectively 39% and 33%), results show that the peak in storage is underestimated by 21.5 mm, or 9% of the peak value (see Figure 1 below). In addition, this difference of 6% porosity cannot sufficiently explain the 15% difference in soil water content between the 5 and 50 cm depths (Figure 4 of the manuscript). Nevertheless it is an important theory to mention and we have added this to the revised version on P.13 L.17-21.

    We also agree that it is unlikely that the hydraulic conductivity increases with depth and have made this clearer (P.13 L.25).

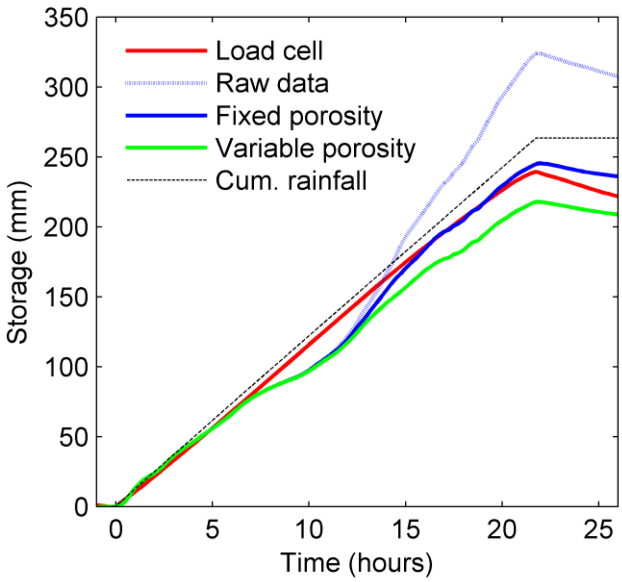

Figure 1: Storage estimates in time relative to the start of the rainfall event. The estimates are derived from i) load cell data, ii) spatial averaging of raw soil moisture data, iii) spatial averaging of soil moisture data with a cutoff of 39% and iv) spatial averaging using a maximum porosity varying between 39% at the surface to 33% at the bottom of the soil profile.

3. *P.14 L.2: Any data to support this statement? It will be helpful to note here that the infiltration front is downward and the saturation front is upward.*

   We have repeated the respective rates and referred to the Results section where the infiltration front speed was calculated (P.14 L.3-5). In addition, we have noted the directions of the infiltration and saturation fronts as was suggested (P.14 L.3-4).

4. *P.14. L.6: It is not a surprise that the speed of saturation front was faster than the infiltration rate because the infiltration rate is measured over the entire surface but the saturation front moves in the pore space.*

   Here we made an error by referring to the “infiltration rate” rather than the “infiltration front speed,” causing confusion. The infiltration rate is indeed measured over the surface, while the infiltration front speed is measured in the pore space. This has been corrected in the revised version (P.14 L.9).

5. *P.14. L.8-9: This expectation is based on the assumption that the total pore space is vertically constant. However, the observation does not support it. The observation suggests that the available pore space does not decrease upward, so that the total pore space decreases downward, which means the soil in the lower layer is more compacted. It supports my discussion in 2.*

   We agree that in theory compaction could be an explanation for this observation and have added this to the manuscript (P.14 L.12-14). We refer to our response to comment 2 for the reason why we do not expect significant changes in porosity with depth.

6. *P. 16 L.15-16: I don't think this is a valid argument. When the tension saturation is reached, extra water from the top can still move downward to fill the free water reservoir and does not generate runoff unless the infiltration rate exceeds the soil infiltrability. This does not explain the early overland flow, but the lateral flow occurred at this time could be the mechanism.*

    We agree that it is not certain that saturation overland flow occurred due to tension saturation at the surface, and have rephrased the sentence to mention this as a possible explanation instead of a likely explanation (P.16 L.18).