Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-80-RC4, 2019

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Relevance and controls of preferential flow at the landscape scale" *by* Dominic Demand et al.

Nicholas Jarvis (Referee)

nicholas.jarvis@slu.se

Received and published: 26 March 2019

This paper presents and interprets a large dataset on soil water content measurements made at the landscape scale in an effort to elucidate climatic, soil and topographic controls on the occurrence of preferential flow. I think it's an interesting study which should be publishable in HESS.

As the fourth person to comment, I hesitate to add too much to what the others have already written. However, one thing that surprised me was the data on hydraulic conductivity measured by Hood infiltrometer. Some of the values at matrix saturation are as large as 500 cm/day, which seems excessively large for infiltration rates measured at a tension of 6 cm, ostensibly unaffected by soil macropores. Even some of the total

saturated hydraulic conductivities seem extraordinarily large to me, varying up to 1500 to 2000 cm/day. Maybe my surprise is just a consequence of the fact that I am more familiar with arable soils, not forest soils. No details are given of the method. I wonder, for example, how the 3D nature of the flow under the infiltrometer is accounted for? If it isn't accounted for, you could seriously overestimate K, especially in strongly layered soils. Could the authors give more details on the method?

The authors attempt to test the hypothesis that preferential flow in macropores in only generated if the rainfall rate exceeds the matrix infiltration capacity, such that the pore water pressure is close to atmospheric pressure at the soil surface. But their approach is rather indirect and therefore prone to errors and uncertainties. The best (only proper?) way to test this hypothesis would be to install tensiometers to measure soil water pressure potential, as well as the probes for soil moisture content. I think their conclusions on this point may be a little suspect, especially considering the unusually large matrix infiltration rates they measured (see above). Connected to this, I think the authors could consider re-phrasing the text at lines 739-743: these non-capillary flow mechanisms certainly contribute to flow close to saturation. However, studies of the physics of these flow processes suggests that they also require pressures quite close to atmospheric pressure for them to generate faster flow velocities than those in the matrix (see discussion and cited papers in Jarvis, 2007, p.528-529). I haven't seen any later studies that clearly contradict those findings.

The fact that preferential flow is strongest when the soil is dry suggests that the likeliest explanation of your results is the occurrence of water repellency, which is known to be a common feature of forest soils. Water repellency causes water potentials to quickly reach very close to zero, even during quite light rainfall, so that water can flow into surface-vented macropores even when the soil is dry. The authors do briefly mention hydrophobicity as a possible reason for their results (lines 694-697), but then seem to dismiss it, which I think is a pity. Preferential flow through macropores generated by the occurrence of (sub-critical) water repellency has been reported in several studies in recent years (see those cited in the review by Jarvis et al., 2016. Vadose Zone Journal, doi:10.2136/vzj2016). I think this topic should be discussed more fully in the paper and some of these recent studies cited.

I didn't get a clear idea of whether the hypothesis on Line 145 was accepted or rejected? The first question is what is meant by "dominate"? Is it the frequency of rain events that generate preferential flow or the amount of water recharging through the unsaturated zone (or something else)? Looking at the text on lines 426 and 432-433, it would seem that preferential flow was not a dominant process (which would also tally with the very high matrix saturated hydraulic conductivities). But I got a different impression from the conclusions, at lines 828-832. Could this be clarified?

Finally, one general comment on terminology: I think it would good if the authors avoided the use of the term "wetting front" and "wetting front velocity". If you have strong preferential flow, there should not be a well-defined wetting front. Maybe you can write "maximum pore water velocity" instead of "wetting front velocity"?

Specific comments

1.) Line 41: Jarvis (2016) is not in the reference list. I think you mean Jarvis et al. 2016?

2.) Lines 64-68: you neglected one very important method and that is the analysis of breakthrough curves for non-reactive solutes (tracers). Perhaps this could be added here with one or two appropriate references?

3.) Lines 70-72: These are not really direct measurements (see line 69). In this respect, X-ray tomography of flow/transport is the only method that gives direct measurements (see Sammartino, S., et al. 2015. Identifying the functional macropore network related to preferential flow in structured soils. Vadose Zone J., doi:10.2136/vzj2015.05.0070; Koestel, J., Larsbo, M. 2014. Imaging and quantification of preferential solute transport in soil macropores. Water Resources Research, 50, 4357–4378).

4.) Line 202: robur

5.) Line 311: "non-uniform flow" is simpler and better than "non-homogeneous wetting front"

6.) Lines 324-328: This is confusing. I think it could be written more clearly and much simpler: "In addition, the hypothesis is tested that preferential flow in macropores is only generated if the rainfall rate exceeds the matrix infiltration capacity, such that the pore water pressure reaches values close to atmospheric pressure at the soil surface"

7.) Line 368: "matric" not "matrix"

8.) Line 377: Delete "Mualem"

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-80, 2019.