Response to comments of Referee #4 Nicholas Jarvis

We thank Nicholas Jarvis for reviewing our manuscript and his comments on the preferential flow phenomena we have observed. We answer below to each comment in a point-by-point reply. For clarity, the comments of the referee were copied in black and our comments are in blue.

General Comments

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?

Forest topsoils can have extremely high saturated hydraulic conductivities (see e.g. Greenwood & Buttle 2014, doi: 10.1002/eco.1320; Gonzalez-Sosa et al. 2010, doi: 10.1002/hyp.7640). Our soils were very structured and permeable (sometimes infiltration was too high to fill the hood of the infiltrometer). At some points the values were verified with double ring infiltrometer measurements being in the same range of conductivities. All measurements include tensions close to saturation and hence, the saturated hydraulic conductivity values (tension 0 cm) are more reliable. Matrix saturated hydraulic conductivity (tension 6 cm) was calculated from a Gardner function that was fitted to the measured tensions. Due to the high macroporosity at many forest locations pressure in the hood was difficult to adjust and measurements could only be conducted for maximum tensions of 1-3 cm. Hence, fore some sites matrix saturated hydraulic conductivity is just an extrapolation of the Gardner fit. However, high matrix saturated hydraulic conductivities were mainly measured in the sandy topsoils of the Marl and Luxemburg Sandstone and therefore the values seem to be plausible to us.

The hood infiltrometer is described in greater detail in Schwärzel & Punzel 2007 (doi: 10.2136/sssaj2006.0104). The derivation of matrix saturated hydraulic conductivity from measured infiltration rates (hood infiltrometer) accounts for the 3D nature of flow using the solution of Woodings 1968 (steady state infiltration from a circular source).

We will clarify the hood infiltrometer method in the revised manuscript.

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).

We agree that tensiometers would help to validate our preferential flow observation. At each site we had one profile of the Decagon MPS-2 sensors which measure only water potentials < -90 hPa. Therefore the sensors were not suited to detect preferential flow.

Our method (max. rainfall rate exceeds the matrix infiltration capacity) is indeed an indirect estimation of preferential flow occurrence, as mentioned by the referee. In contrast to a direct method (like tensiometers) the aim of the analysis was not to validate our preferential flow measurements, but rather to compare the observations with an estimation by a capillary approach (based on matrix hydraulic conductivities).

To have a clearer structure of the study (especially results) and focus on the main analysis we will remove this comparison of measured data with a capillary approach ($P_{max} > K_{mat}$). Therefore, the hood infiltrometer measurements will only be used for an additional estimation of fast flow (measured $v_{max} > K_{mat}$; see response to RC1).

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.

It is correct that also alternative flow processes (e.g. film flow) require a relatively low soil water potential (high saturation). On P23L13-15 we wrote: “Higher occurrence of measured *NSR* compared to capillary theory prediction could indicate other initiation and flow mechanisms […]”. The sentence is vaguely phrased and we apologize for that. The meaning was, that unknown initiation processes (local depressions, channeling of water by vegetation, hydrophobicity, etc.) can locally lead to higher water contents and alternative flow processes. We will clarify this sentence.

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.

The strongest increase in preferential flow occurrence during dry conditions was observed in the clay-rich sites, which is why we have considered structure development to be the major effect. However, since four of the six landscape units show an increase during dry conditions (although some are just small) we will stronger consider hydrophobicity in the revised discussion section.

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?

The word “dominates” might be too strong and we will rephrase the hypothesis and the conclusion. The new hypothesis is that preferential flow is a common process that is frequently involved in flow during infiltration events. Further, preferential flow increases the flow velocity compared to matrix flow and changes water content distributions in the soil by non-homogenous flow. We mainly focus on the frequency of preferential flow occurrence. To draw a conclusion on the amount of water that is transported or that contributes to groundwater recharge it would require a physically based model (out of the scope of this study). Therefore, we only included Fig. 3 to highlight that water content change was highest during the NSR events or in a greater depth for sites where flow velocity was high (Marl).

To answer the question if preferential flow is “dominant”: We found preferential flow in all our landscape units, but being temporally highly variable. We were able to find hotspot landscapes (clayey soils, forests) and hot moments (dry, high rainfall intensity) of preferential flow occurrence. This verifies that preferential flow is a common and important, spatially and temporally variable process, but maybe not a dominating process.

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”?

We agree that the term is not precise. We used this term since it is relatively often used in the literature (see e.g. Hardie et al. 2013, doi: 10.1016/j.jconhyd.2012.10.008; Germann & Hensel 2006, doi: 10.2136/vzj2005.0080). However, the term “maximum pore water velocity” is more appropriate and we will change it according to your suggestion.

Specific Comments

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

We apologize for giving a citation that is not in the reference list. The citation was changed to the intended reference: Larsbo et al. 2014 (doi:10.5194/hess-18-5255-2014).

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?

We will add the analysis of breakthrough curves as a potential method with Koestel et al. 2013 (doi:10.1002/wrcr.20079) as a reference.

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).

We agree that the mentioned methods are no direct measurements. We will change the sentence to: "Another way to identify the potential for PF are measurements that can be related to the number and volume of macropores or cracks."

4.) Line 202: robur

We corrected the latin name.

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

We changed it as suggested to the term "non-uniform flow".

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"

We thank you for this suggestion of an alternative and simpler phrasing. We changed it as suggested.

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

We corrected the word as suggested.

8.) Line 377: Delete "Mualem"

We removed the word "Mualem".