

## ***Interactive comment on “Significance of tree roots for preferential infiltration in stagnic soils” by B. Lange et al.***

**Anonymous Referee #1**

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### General comments

This study aims at assessing the impact of tree roots on soil preferential flow. The authors conducted infiltration experiments on stagnic soil in forest stand. The TDR time series were then analyzed with the rivulet approach developed by Germann et al. (1997).

The interesting paper shows the potential of the rivulet approach to adequately assess soil infiltration properties. The paper is generally well written but the theoretical part is a bit difficult to follow without reading first Germann et al. (2007).

An important hypothesis here is that the effect of roots can be independently assessed

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for a broad range of soil textures (Table 1). The authors end up with two regression equations which describe the preferential infiltration only in function of the fine root length (FRL) by soil volume, thereby neglecting the potential effect of soil structure. On the other hand, in your dataset, the hydromorphic soil properties and the location of the horizon are uniquely linked to the type of root and the amount of roots which are found (p. 2385, lines 20-22 and p. 2386, lines 11-12). The main question is therefore: do you investigate the effect of the roots themselves or the soil-root system as a whole? You should have the same soils without any roots to be able to uniquely relate the preferential infiltration properties to the density of roots only. In your case the FRL of Eqs. 11 and 12 is also an index of soil type and soil location. So, have you proved that in general "tree roots improve soil structure and infiltrability"? (lines 5-6, p. 2391) or can you really generalize your results to all forest soils? (lines 23-24, p. 2391). I do not think that you can generalize your results to other soil types or even state that the roots are the primary factor in your case (even if there are for sure a key factor). You should perhaps reshape these types of statement, which can be found in the title, abstract, discussion and conclusion sections or better indicate the limitations of your study.

## Specific comments

- p. 2375, l. 6-8: you mention that the Richards equation is inadequate for preferential flow but just after (lines 9-13), you mention studies using it to model preferential flow. Gerke (2006) did not explicitly state that Richards equation is inadequate but rather that its application is rather limited. Please rephrase.

- p. 2377-2378: Several variables in the equations 1 to 11 are not defined. For instance  $v_w$  in eq. 4,  $u_{\{w,j\}}$  in eq. 5, or  $w_{en}$  in equation 8. Additional typos, like the 'j' underscript, line 7, p. 2378, makes the full understanding of the theory difficult.

- p. 2378, l. 14, 15: it is worth (re)explaining why you choose  $N=10$  and how you decide the repartition between  $w_j$ . By reading Germann et al. (1997), I understand that you actually partition the time period into  $N$  sections of equal duration and then estimate

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the corresponding  $w_j$  with the increasing limb of  $w(Z,t)$ . Please clarify that explicitly in the manuscript.

- p. 2380, l. 16-17: bulk density is likely not enough to state that "root growth is not limited by soil compaction". This is probably true that there is no compaction in the studied soil but root growth could be limited even in a not compacted soil due to soil dryness (which increases the soil mechanical resistance). Please rephrase or give a reference.

- p. 2380, line 24: what you characterize is not the root morphology (which characterizes the complete root architectures through indices) but rather the root diameter distribution profile. You should change this everywhere throughout of the text and should be more precise when you state that "root morphology is a key factor for infiltration" (line 8-9, page 2391).

- p. 2384, lines 14-15: see previous remark on the term "root morphology".

- p. 2384, line 11: I do not understand why the large roots are not considered in the further analyses. It seems a bit contradictory with the assumptions that preferential flow occurs in the larger pores (p. 2389, lines 23-25). Even if the large roots do not constitute the main part of the root system, it is not a good reason to simply neglect them. Either you should consider that part as well or give more arguments why this type of roots is neglected. This is of particular importance for your general conclusions (and also the title): are all the tree roots significant for preferential infiltration? Are your conclusions also valid for large roots?

- p. 2389, line 10: another reason could be the difference between root length density profiles and root architecture, which would then describe how large soil pores are connected.

- p.2389, line 27: "kind of funnel effect": please clarify.

- p. 2392, line 1: did you show that "the water storage space" is enlarged? I would

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rather say "enlarge the potential soil volume accessible to surface water".

Reference : Germann et al., Vadose Zone J. 6:207-220.

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