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Interactive comment on "Estimates of tree root water uptake from soil moisture profile dynamics" *by* Conrad Jackisch et al.

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1 General reply to all referees

We sincerely thank Jesse Nippert, Jia Hu and Leander Anderegg for their intense study of our manuscript and their constructive feedback. We clearly understand that we have to simplify some of the dense writing and figures to convey our findings more clearly. The referees made several detailed suggestions for this, along which we will organise the revisions. We will self-critically check for simplifications of jargon and clarity in our arguments.

With respect to the observed process dynamics of measured and inferred variables,

we will carefully revise the manuscript towards i) a more detailed description of the observed results, ii) more coherent argumentation lines, and iii) the limits of the presented approach. We will put specific attention to a) the conversion of sap flow velocity and rhizosphere water withdrawal to flux rates, b) the assessment of the coherence of the diurnal signal with the assumed step shape, and c) the reference to inferred matric potential. At a meta-level, we have to make sure not to overstretch the data set at hand which is basically a first reference. We hope that many more researchers will employ, test and evaluate the proposed approach to estimate RWU, which together will form a more comprehensive picture of the complementary information in RWU, SF and ET.

2 Specific replies to the review by Jia Hu

The referee's comments are given in *italics* with our answers in regular font style.

In this study, the authors examined root water uptake in beech trees, along a soil moisture profile in two sites with different moisture conditions. There were some interesting findings. First, they found differences in depth of water taken up by the roots between the two sites (sandy soils use deeper water). This was particularly evident during the period of maximum transpiration rates. They also found that while RWU could be estimated from changes in soil moisture, there were also instances where transpiration was occurring, but RWU was not measured. In a comparison between the sand versus slate sites, although SF was similar throughout the year, RWU was quite different.

Thank you very much for this summary in which we see our study well-understood.

One of the main ways in which this manuscript can improve is to clearly discuss the *reasons for comparing RWU and SF. The authors state that the aim of the study "is to evaluate the potential and limitations of the diurnal decrease of rhizosphere soil moisture measurements as an estimate for RWU in ecohydrological field studies." They make the point that RWU is an under measured observation, and that other proxies, such as changes in soil water content, are used to infer RWU. Meanwhile, sap flow sensors measure transpiration rates in trees, but because of stored water within trees, sap flow does not measure RWU uptake either. So linking RWU and SF (as mentioned in hypothesis 2) seems to be an important link. However, what wasn't clear for me is that if ET is an important metric to quantify for ecohydrological studies, what does RWU measurements provide that SF measurements don't? In other words, what additional processes related to ET do RWU measurements elucidate? I think this discussion could be enhanced more in the introduction. For example, in lines 28, the authors state, "Furthermore, spatially distributed monitoring of both RWU and soil moisture and SF could help to elucidate differences between the influence of the geological and pedological settings on water supply to transpiration and the influence of the plants themselves and their adaptations in root systems, dynamic sourcing of water and transpiration efficiency." Does this suggest that RWU influences the "geological and pedological settings on water supply to transpiration" while SF measurements assess the "influence of the plants themselves and their adaptation in roots systems...?" But SF also influences RWU, so shouldn't SF and RWU be considered in a framework that acknowledges that they influence each other?*

Thank you very much for raising our attention to this point. We see SF and RWU as communicating pairs of a common process, however, with slightly different foci. The method we propose to infer RWU from soil moisture measurements can help to assess the influence of (abiotic) site characteristics on the water availability for the tree. Additionally, assessing the dynamics of RWU from different depths also provides information on the hydrological conditions and processes within the rooting zone. In contrast, SF is mostly used as proxy for actual tree transpiration (with some uncertainty regarding tree water storage and assumptions during the calculations of sap flux), and

is also influenced by adaptations of the tree to the local site conditions. We therefore suggest to measure both SF and RWU for a better understanding of the water transport through trees. We will clarify this notion in the revisions.

Figure 3. Why are estimates of Dq positive if the soil moisture decrease throughout the three day period? In page 6, line 22, does "change in soil moisture" refer to Dq? If so, again, why is Dq positive? The positive values of Dq during the daytime is confusing because in Figure 4B, Dq during the daytime hours is shown as negative. In Equation 1, the authors also state that a check to evaluate the data is that " day slope of soil moisture is negative (decline in soil moisture during the day)..."

We agree that it is confusing that we define the change in soil moisture negatively in Fig. 3 but regularly in Fig. 4 and the calculation. We will clarify this in the revisions.

Page 7, line 1. Do the bolded a) and b) here refer to a subset of "soil moisture (b)" from page 6, line 15? If so, I would change "a) and b)" to "i) and ii)" as to not confuse the reader.

Thank you for the suggestion. We will change this as proposed.

Page 7, line 4 and 5. No need to say "no STRONG decline in soil moisture" or "no TOO STRONG increase in soil moisture" since STRONG or TOO STRONG are quite subjective. I think that saying "no decline in soil moisture" or "no increase in soil moisture" followed by the rates of increase or decrease is sufficient.

Thank you. We will change this as proposed.

Page 7, line 30. Why was the assumption made that measured sap flow originates in the soil moisture decrease? Could there be any storage of water in the trunks (i.e.

might lags between RWU and SF exist)?

We do expect some lags between RWU and SF due to water storage in the tree, that is why we have highlighted this (rather blunt) assumption. It appears difficult to quantify such a storage effect without further data (i.e. ET and more references of SF). However, with a temporal aggregation to daily steps we see a relatively high correlation between RWU and SF. Thus we do not expect the lag effect of water storage in the trunk to be very pronounced at this resolution.

Page 7, line 31. "This is done by linear regression of daily sap flow to the sum of RWU over the soil profile with assumed zero intercept." Is the assumption again here that water from the different soil layers instantaneously feeds into the transpirational stream - in other words, there is no lag in when water is taken up by the roots and then transported to the trunk of the tree?

It is generally correct that we neglect an intercept within the tree by applying a regression. However, since we sample the recorded data to daily aggregates, differences between the fluxes with shorter temporal footprint should cancel out (i.e. the lag between sap flow and RWU in Fig. 3). Hence "instantaneous" connection is not assumed. Nevertheless, we find strong differences in RWU and SF (Fig. 8), which might hint to water storage dynamics within the tree. However, we cannot assume to have sampled all sources of RWU with the soil moisture profile. Especially at the slate site it is very likely that roots can source water from local subsurface pools or films in the gravelly subsoil. We will clarify these points in the revised version of the manuscript.

Page 8, Line 1. "The resulting factor is the mean reference area required to supply to observed sap flow." Is the 'factor' mentioned here the area or the volume? If RWU is summed across the different soil depths in which soil moisture is measured, how is the resulting factor estimated as area and not volume?

As stated in the mentioned subsection a proper comparison of SF and RWU requires them to be defined as fluxes. This means that we have to refer to a cross-sectional area of active xylem for SF and a reference rhizosphere volume for the observed change in soil moisture attributed to RWU. Here the height of each volume increment is given by the integration length of the soil moisture profile probe, which is 0.2 m. Without knowledge about the actual root distribution we simply assumed a cylindrical rhizosphere. The "factor" is hence the projected area of this cylinder which can be expressed as radius for a plausibility check (see legend in Fig. 7). Since the RWU is defined in mm/day (a volume normalised by the area) the factor has to be an area to derive the volume flux. We will reconsider how this step can be clarified.

Page 10, Line 2. "In later summer, the RWU signal ceases although the sap flow signal continues at lower rates." In Figure 6, I don't see when this occurs across the entire instrument period.

The visual comparison of sap flow (L/day) and RWU (mm/day) dynamics has its drawbacks. This is why we opted to extend the analysis with the estimate for fluxes instead of the direct signals. However, it is not clear how much the assumptions to derive the volume fluxes will blur the actual signal in the observations. We agree that this statement can be seen as subjective. As Fig. 6 is subject to revisions, we will revise the interpretation accordingly and opt to refer to the following analyses in Fig. 8 instead.

Page 10, Line 32. "With a working-hypothesis of a closed water balance...the linear regression also results ....at the sandy site the cylinder would have a radius of 4.2m...slate site one would estimate a radius of 5.5m." I may have missed this, but how did you reach these readius values? Where is the linear regression model reported? I see that there are radius values reported in Figure 7, but how were these calculated?

Please see above (comment to P8L1). We will clarify this step in the revised manuscript accordingly.

Page 12, Line 2. "However, the high initial correlation drops in July. At the sand site, this marks the shift to RWU ranging below SF. At the slate site, no such transition is apparent." In Figure 8, when the spearman correlation drops, the precedes when RWU drops below sap flow. There are also instances later in July when RWU is consistently below SF but the spearman correlation ratio does not change. What does this mean?

The Spearman rank correlation exactly "punishes" the change in ranks. Frequent changes result in low correlation values (e.g. August at the slate site). When RWU is consistently below OR above SF the correlation can become rather high. Since this is not giving the full picture, we report the KGE as alternative measure of correlation which "punishes" deviation of the dynamics and the absolute values.

Page 14. Line 9. I would recommend changing the work "ambivalent" to "mixed."

Thank you. We see the awkward wording and will change it.

Page 16, line 9. "What is the optimization function of the plant's RWU sourcing and SF variability?" What do the authors mean by this? Please explain.

Gao et al. (2014) show that climate leads to an adaptation of the rhizosphere storage capacity. Saveyn et al. (2008) show how different SF can take place in the xylem under different weather conditions. We agree to your argument that RWU and SF have to be considered as interactive processes. Hence we expect the plants to adapt to climatic and site conditions. We expect that this adaptation is not a random process but some sort of optimisation.

Page 16, line 12. Yes, wounding from sap flow sensors can indeed underestimate

sap flux velocity, and non-homogenous xylem depths can influence estimates of total transpiration rates, but it seems unlikely that these effects would be most noticeable during periods when both sap flux and RWU begin to decline. The authors allude to other factors in the previous paragraph (e.g. stem storage, leaf level transpiration) that offer more likely explanations for why correlations between RWU and sap flux correlations decrease as the soils dry out.

Thank you for your evaluation of these influencing factors. We will carefully check that affecting factors are presented in a balanced manner.

3 Bibliography

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