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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 Leander Anderegg

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

In this manuscript, the authors pair soil moisture measurements that have high spatial and temporal resolution with tree sapflow measurements at two different sites to test whether such soil moisture measurements can be used to estimate daily transpiration and identify depths of root water uptake (RWU). They find promising similarities between sap flow and estimated RWU during a fairly wet period at their site with sandy soil, but worse correlations at a site with more heterogeneous soil characteristics and a time series that extended into a drier period. They also found interesting evidence for differences in the depth of RWU at the two study sites, though this is somewhat deemphasized in the text. While the estimated daily RWU uptake appears promising in some regards, they also found a confusing lack of relationship between RWU and soil matric potential calculated from soil moisture release curves and soil water content. All told, these results suggest that the method is promising but still has some kinks to be *worked out, some of the largest probably relate to spatial heterogeneity at large scales (lateral variation over meters) and fine scales (inability to infer matric potential from soil moisture release curves on nearby soil samples.*

This is an interesting manuscript that presents a promising approach to estimating transpiration and RWU at high temporal scale. However, my three main concerns are: 1) The writing and figures are extremely dense and sometimes confusing/contradictory. I had to read the Results at least two times, and often had to parse out individual sentences multiple times before I could begin to follow their meaning. Some of this could be due to a difference in fields (hydrology vs the plant ecophysiology terminology that I am more familiar with), but I would recommend a considerable expansion of the Results to explain the more complicated an nuanced findings and make this interpretable by a broad audience. I have given multiple suggestions below in the 'Specific Comments' section, but would general recommend a careful edit and clarification of the most complicated sentences in the Results. I also would recommend simplifying some of the figures by breaking out aspects into multiple panels rather than layering on 4-5 different sources of information that I found almost impossible to interpret simultaneously. In particular, Fig 6 and 12 are nearly impenetrable (and Fig 11 is also quite dense).

Thank you for your intense study of our manuscript and taking the challenge to dig out our messages so well. We gratefully receive your suggestions to clarify and simplify the manuscript including some of the figures for better understanding.

2) The introduction oversells the novelty of monitoring soil moisture to estimate RWU dynamics and transpiration. True, the ability to monitor soil moisture with high enough precision to assess daily RWU is fairly novel and new, but people have been measuring soil moisture to estimate depths of RWU and understand transpiration budgets for decades! In fact, I would argue that gravimetric or volumentric soil moisture measurements are the original method for estimating transpiration (e.g. just to name a couple that come up with a quick google search: Denmead & Shaw (1962) "Availability

of soil water to plants as affected by soil moisture content and meteorological conditions" Agronomy Journal; Novak (1987) "Estimation of soil-water extraction patterns by roots" Agricultural Water Management). Thus, I think it is important in the Introduction to stress that it is the precision of these measurements (allowing high temporal and spatial estimation of RWU) that is interesting, not the method and theory itself.

We generally agree to this point and will seek for a more balanced presentation. Despite the clear reference of RWU to soil water content our aim is to highlight the capability of this easily available technique for such analyses - given the level of precision and spatial coherence. We will revise the introduction accordingly.

3) I think the authors do a good job honestly discussing where their approach did not perform well, but I would both urge them to focus and structure the discussion around a coherent argument for what the key processes and attributes are that screw up these measurements (e.g. what are the 4 biggest problems, list them out, and show us how you concluded that these are what is causing the method to fail at the Slate site and in dry soils).

Thank you for acknowledging our efforts. As often, one quickly arrives at "it depends" when distilling such 4 biggest problems. Actually, the system is underdetermined given just some SF and soil moisture sensors to rigorously conclude such a fixed list. However, we will revise the manuscript to convey these arguments in a stringent and clear manner.

I would also urge the authors to reconsider the framing and discussion around their 'Hypothesis 3'. It is currently framed as an open question whether tension gradients drive variation in root water uptake. And then Figure 11 is presented as evidence that this may not be the case. I think this is a misrepresentation of both where the field is at and what the confusing findings of Figure 11 represent. Plants can alter RWU via *changes to root properties (changing aquaporin expression to alter root permeability) and root distribuitons, but they cannot physically fight potential gradients as the authors seem to suggest with Fig 11 and in the Discussion. Plants can ONLY extract and move water by moving it down a potential gradient, and there is no physical way the plant can be extracting and transpiring water from soils with a matric potential 10s-100s of MPa below 'permanent wilting point' (âĹij1.5 MPa, or 4.2 (log10(hPa)). The general dogma (assuming +/- equivalent root resistances throughout the soil profile) that water uptake by roots should be proportional to the pressure difference and the root surface area/biomass should be used as a final test for the reliability of this method to estimate RWU, rather than using the data to test the dogma. In this case, I think it is painfully obvious that we have essentially no reliable way to convert water content to matric potential at the spatial and temporal scales that are relevant to these transpiration estimates. In fact, we're SO BAD at it, that it would appear that the Slate trees are extracting water from soil with a matric potential of Âń -10 MPa (when leaf water potential, the ultimate pressure differential driving water movement, is almost certinaly > -2 MPa). That tells me that there's a problem with the method, not the theory. However, recognizing this allows you to say something interesting about why we can't back calculate matric potential from these measurements (spatial heterogeneity in soil properties? Problems with rock fractions? Rock fractures that don't behave like soil samples used for dehydration curves?).*

Yes. We fully agree to this notion and clearly see the implications for our hypothesis 3. In a different study we exactly work out this point that it has been somewhat "forgotten" to intensify research about how to measure matric potentials but that both variables are essential to define soil water state dynamics because single retention functions render inconclusive – as in our example here. Likewise, measuring matric potential in general and in heterogeneous soils (like at the slate site) especially is challenging and could not be done in a similar manner as the soil moisture profile in this study. We will follow your advice and will reconsider if and how we should include a reference to matric potential inferred from soil moisture measurements in our study.

Specific comments: Pg 6 L11-14: Please explain a little more what you mean by 'NSE is a measure which is very sensitive to deviations from shape features" (perhaps you could add a day that does not pass this cuttof to Fig 4 to illustrate?), what cutoff of NSE you used, and how you arrived at that cutoff.

Thank you for the suggestion. We will add further reference candidates to Fig 4B to exemplify the differences and evaluation criterion.

Pg 7 L8-12 and pg10 L30 and Fig 7: I am very confused about what 'corrected' means. In the Methods, I interpreted 'Corrected' to mean RWU extrapolated from the linear regression through the nightly data (magenta line in Fig 4a). But in Fig 7 the 'not corrected' values (blue points) are higher than the 'corrected' values (colored points), which tells me I'm getting confused somewhere. Please clarify this in Fig 4, and Fig 7 and the associated Results text (pg 10 L30).

The difference between "corrected" and "not corrected" is if the slope of the nocturnal phase is extrapolated to form the reference or if simply the difference between the reference time stamps is calculated. Given our criteria for the step shape, the nocturnal phases can have slight negative slopes, which would lead to the observed situation that the not-corrected RWU is higher. This hints to phases when some diffusive percolation is taking place. With more description of the results in the revised manuscript, we will also clarify this.

Pg 7 L20-29: This paragraph about turning sap velocity into sap flux is very confusing. I did not understand it until I scrutinized Fig 5. Please rewrite/clarify. Also, in the Fig 5 legend/caption it is worth noting that the "5mm, 18mm, and 30mm" are depths from the outside of the tree (or inside of inner bark? Not sure which).

Again thank you for diving deep. Before installing the sap flow sensors, the bark is removed, so the depths are measured approximately from the cambium. We will re-structure the presentation of this step to make it easier to follow.

Figure 6: I had a very difficult time extracting the desired inferences from this figure. The shading (which varies per site, over time, and in different soil layers) is almost impossible to see and interpret (not to mention some of the colors become colors used for other soil depths when shaded) yet are referenced multiple times in the text. Also, the stacked bar plots make it almost impossible for me to interpret which depths are providing RWU, mostly I just take away total bar height. I would recommend 1) breaking out the information about how well the RWU estimation likely worked into another panel or method other than shading (filled versus unfilled bars/symbols, perhaps?). and 2) either finding a more holistic way of showing depth information (e.g. coloring whole bars by the weighted average depth of RWU) or just making a different panel that showed line graphs of uptake by depth through time. In fact, I would potentially advocate for breaking out the depth of extraction information into a new figure altogether.

We take this point and will rethink this figure. First of all, the shading will be dropped and given in a further figure. Using line plots instead does not help comprehension as this was our first try on this. We will reconsider how to make this legible.

Pg 12 L14-15: I don't quite understand what data are being compared in this sentence "Comparing RWU correlation between the two sites, applying the nocturnal correction improves Spearman rho form 0.42 to 0.52. KGE remains almost the same with 0.27 increasing to 0.3." All data from both sites (if so, why is this a useful comparison)? Or somehow site-level averages?

Obviously, we have to clarify this in the revisions, thank you for pointing it out. In the analyses before we have used the "corrected" data. Here we confirm that our findings about low correlation between RWU of the two sites remain valid with either approach. We see that the confusion might originate from this rather technical detail concluding

the site and process comparison subsection. This will be transferred to the discussion section.

Section 4.3 – I think this section is very cool, but I understood very little of the text. What does "a diffuse redistribution into the surrounding soil aggregates" mean and why can it be "seen as parallel declines. . .in the different depth layers"? Please explain more what "flashy transport through the macroporous soils and fill-and-spill mechanisms of subsurface pools" means, and much more importantly how this analysis influences our interpretation of the method for assessing RWU in this site. Clearly you learned something interesting and highly relevant (possibly that helps us interpret Fig 11?), but I do not understand what it is based on the current text. For instance, I have no idea what these sentences mean and how they relate to Fig 10b "Here, roots are likely to grow along joints and fractures, where event-water can be stored with little effect on the bulk soil moisture. As such, the measurements might miss parts of the active rhizosphere."

The hydrological jargon might be especially dense here - also because we expect quite some idea about the sites from the reader. We will explain in more detail in the revised version and reconsider if this might become part of the site description (again).

Section 4.4 – See above comments about interpretation and framing of these results. Also, the current Figure 11 is nearly impossible for me to interpret. I would recommend displaying SOME aspect of this information in multiple panels. (e.g. maybe splitting the soil columns up into three depths and displaying them as separate panels so you can color by SF). Also, the units/label on the x axis of this figure is confusing to me. And honestly, after reading the text of this section 4 times, I still don't have any idea what it means. I can't even decipher it enough to make suggestions on how to clarify it. I don't know what the referenced 'reactions' are and how I'm supposed to assess them in the figure. Moreover, I do not at all see the 'correlation of matric potential and depth' that *supposedly exists in slate site.*

We will re-evaluate Fig. 11. As you point out, we also need to rework the whole argument showing that it is not the plants sourcing at high flux rates against physiologically impossible tensions but the conversion of soil moisture into matric potentials, which does not represent the state around the roots. We will take care of this in the revision.

Pg 14 L14: This sentence "At the same time, we pointed out considerable limitations to the approach with respect to soil water state (no detectable signal during low moisture periods) and soil properties (high variability in heterogeneous soil profiles)" is the most interesting sentence of the discussion to me, but comes out of no where and needs much more explanation. In order for me to follow your train of thought, I require much more explanation. . .

Thank you for highlighting this lack of reference. We will build the links for clarification.

Figure 12 and associated text of Section 5.1: I had an extremely hard time interpreting this figure. Please 1) remove the red bars for total extraction to new panels (two axes y-axes with different interpretations is much more than my brain can handle). 2) Explain what the NSC cutoffs indicate, and what the larger blue bar for 'all detected' is and why the inset bars for different detection thresholds do not sum to it 3) Put panel A and B on the same axis (e.g. 0%-90%) and switch the big numbers to be % of days and little numbers to be of days. Also, how does Fig 12 show "The RWU derivation function appears to perform very well in general and can be used to evaluate a broad range of diurnal changes in soil moisture (Fig. 12)." (L1-2). Moreover, this sentence doesn't really make sense to me "Unlike the first impression in Fig. 6, the proportion of steps with higher uncertainty about the actual fit of the shape with the assumptions is higher in the slate site data, which is in line with the lower overall RWU detection there." Could you explain what you mean by "higher uncertainty about the actual fit"? Also,

how "uncertainty" and rate of "overall detection" differ? Throughout this section, please be much more explicit about the site, times, and layers you are referring to when, for example, you write "Under somewhat ideal conditions with soil moisture sensors and roots in good contact with a rather homogeneous soil matrix and sufficient soil water availability, the diurnal steps are identified and evaluated with great confidence." Finally, this feels like it should be in the Results, perhaps even near Figure 1, rather than in the Discussion.Pg 15- L5: I think it's worth explicityly mentioning the take-away from Figure C1: that flux amount is unrelated to how well the step function fits the daily soil moisture pattern.

Again, a big thanks for working out the twists which have made their way into our argumentation. Emphasising on the RWU estimation in the revised manuscript will make room to clarify on this as one of the main results. Thank you also for the suggestions on how to make Fig. 12 easier to understand. We will consider them for the revision.

Pg 16-L25-35: See my comments about Hypothesis 3 and Figure 11. Also, the sentences at L28 ("At the sandy site. . .") seem confusing and almost self contradictory to me.

We expected to find some evidence for a preference of cheap water (large fluxes at low tensions) by the roots. The sandy site somewhat depicts that. However the periods of implausibly high tensions have to be reconsidered. At the slate site, gravel content (which increases with depth) is a likely explanation for the "strange" picture. When we correct the available pore space with gravel content the whole distribution should shift to the left and make more sense. We will check both avenues: i) if the general argument around hypothesis 3 is fruitful and ii) how to clarify on Fig. 11 and the respective result description.

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