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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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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).

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.

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). 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' ($\sim$1.5 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 « -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 the-

ory. 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?).

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.

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).

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).

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.

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?

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."

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.

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. . .

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.

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.

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