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Comment on hess-2021-456

Valentin Couvreur (Referee)

Referee comment on "Xylem water in riparian willow trees (*Salix alba*) reveals shallow sources of root water uptake by in situ monitoring of stable water isotopes" by Jessica Landgraf et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-456-RC1, 2021

In this study, the authors investigate the sources of water absorbed by Willow trees using both bulk and in situ monitoring of water isotopic signature in the soil and stems of two of these trees. A Bayesian mixing modelling approach to water sourcing suggests that the vast majority of water is absorbed from the top soil (above 40 cm depth), except at the end of the summer period when a fraction of the xylem water could originate from deeper soil layers (40 cm and 100 cm).

General comments:

From a general point of view, I believe the manuscript is sound, well-written and structured. It is also very well adapted to the audience of Hydrology and Earth System Sciences. On the one hand, I found the comparison of soil water content and in situ monitoring of water signatures particularly interesting and think the discussion could emphasize the related potential implications a bit more. On the other hand, the conclusion on the production of reliable 2-hourly resolution data for water signatures I think would need further support in the main text or should be tuned down. My main concern is about the quality of the figures, which would require substantial work. Yet, I think the manuscript is overall in a good shape and could be considered for publication in HESS after minor revisions.

Specific comments:

- Lines 81-84 (L81-84): Besides radial growth, doesn't the swelling / shrinking response of tree stem diameter reflect stem water tension, which results from the imbalance between canopy transpiration and root water uptake? This kind of formulation would seem more intuitive to me. Was any such swelling / shrinking fluctuation observed daily? If not, this part of the introduction could as well be removed to make the

manuscript more concise.

- L85: I think this part needs a bit of rephrasing too as transpiration is considered as the product of VPD and canopy conductance (often termed gs). Canopy conductance is considered to react to multiple factors like limited light, limited export of products of photosynthesis and limited water availability. Therefore, I think writing that sap flow usually reflects VPD is a bit strong. Maybe clarify "in absence of (…) limitations (…)".
- L88-89: Here I would suggest to re-order the terms. It could be more intuitive to mention decreased sap flow after low leaf water potential, turgor, and stomata closing, as it would come last in a temporal sequence of observations.
- Figure 1: There seems to be something wrong with the coordinates along the frame of panel A. Could you check if that is the case? Also, several elements in the figure are very small and hard to read. For instance, the labels in panel B, the dots marking soil pits and probes, whose colours are hard to distinguish for such small points. Could you modify the figure to facilitate its readability?
- L143-144: Can the eddy flux covariance measurement be considered as representative of the Willow trees transpiration? If not, could you clarify already at this point how the data is intended to be used?
- Figure 3 (and others): The use of the symbol "/" preceding units in the y-axis label is a bit confusing. Could you use another symbol, like the pair of brackets for instance?
- Figure 4: The right-side y-label would be clearer specifying "stem diameter variation" as it seems the stem size was not close to zero in May 2020. Northern and Southern Willow labels directly in the Figure would also be convenient. The legend indicates "Sap flow N" in both panels but it is unclear what "N" stands for. Could you clarify? Sap flow rates and ET would also gain at being overlain all in panel 4a for easier comparison, while stem diameter variation could be displayed in panel 4b.
- L290: The subtitle could also mention "and transpiration".
- Figure 5: The figure is particularly hard to read as symbols are small, several of them have similar colours and/or are hidden under geometrical elements. If the ordering of the legend is conserved in the bar plots, it is confusing that precipitation is associated to light-grey bars but black dots, surface water medium-grey bars but white dots, etc. Could you work on a new version of this figure that is easier to read?
- L307: Here and at other places in the manuscript, the use of the "-" sign to specify ranges of values while these values are negative is not ideal. Could you replace the "-" sign by the word "to"?
- L309: I am relatively new to water isotopic studies, so the concept of "lc-excess" I had to look up. I think for the audience of HESS it would be worth carefully defining the lc-excess with the associated equation and possibly a one-sentence example to make it easier to grasp.
- L3012: The expression "generally similar to" is a bit vague given the wealth of data available in this study. Were in situ and bulk measurements significantly different? Is there any indicator of their similarity (e.g. R-square, …)?
- L314: Please clarify the type of variability (space, time, …).
- L339: Could you comment on what could make responses more marked in covered pit A than pit B?
- Figure 7: In panel (a), the location of the "zero" differs between vertical axes, which complicates the visualization of the results. Could you fix this? Some of the panels (b) to (e) could as well be merged to facilitate the comparison of the results and the visualization overall as vertical axes are currently very small.
- Figure 9: Several of the box plots are hard to distinguish from the border of the panels. Could you make them easier to distinguish for instance with a lighter panel border colour? The legend for soil moisture lacks the dotted / dashed aspects. Could you make the legend more consistent with the content of the figure?
- L358-359: The prediction of water uptake almost solely from the top soil in the Bayesian modelling output is quite interesting. If you had to explain the remaining differences between water isotopic signatures in the stem and at 10 cm depth, what would be the other sources that you would consider as necessary complements? Could

you discuss the results also from this perspective?

- From a quantitative point of view, I think the authors could as well have argued (but don't have to) on the possibility to have root water uptake solely from the top 40 cm of soil by comparing the cumulative tree transpiration and rainfall to the soil water storage change. My estimations suggest that, even when neglecting water capillary rise from deeper layers, the water balance in the top soil seems reasonable if the tree roots extend about 10 meters away from the stem.
- L376: I found the phrasing "soil moisture availability was (…) not less than (…)" a bit odd. Do you mean that it is higher (as suggested by Fig. 9) but not significantly?
- L394-397: This part of the discussion is quite interesting. In the results I kept wondering why deeper water uptake occurred just as the upper soil was rewetting, which is counterintuitive for a specialist of hydrodynamics, as passive water flow from shallower soil layers to roots is supposed to increase as the water potential in these layers increases (a process called "root water uptake compensation"). So, your observations seem to go against the second law of thermodynamics (how exciting!). You do mention the possible explanation that the stem water signature could be the integral of past and present water uptake (thereby possibly reconciliating your observations with thermodynamics laws). If that is the case and if the volume of the stem water pool mixing with newly absorbed water is large enough, one would expect to see the signature of deep water in stems weeks after deep water uptake occurred. Obviously, this calls for more investigations of the mixing of soil and stem water pools ideally under controlled conditions and with labelled water, as well as for new versions of Bayesian mixing models that account for such a temporal integration of water signatures (or do they exist already?). The temporal integration of water signatures would also alter the ability to infer on water uptake profiles at high-temporal resolution, unless one was able to "deconvolute" the temporal dynamics of stem water signatures. Could you discuss this a bit more in depth in the manuscript?
- L418-440: Here diurnal fluctuations of the signals are discussed in depth, which I found a bit odd as I did not find supporting data and results in the main text and figures (I guess they are in appendices). I think if they are to be discussed in depth, they should be presented in the results and figures for the audience to have a grasp on what was observed at such a temporal resolution. It is striking in particular to conclude (L453-454) on the production of "reliable high-frequency (…) 2-hour resolution" observations while I think only daily-averaged data is presented in the body of the manuscript. I think it would be more consistent to present the high-resolution data and demonstrate confidence in its quality, or not to conclude on the production of such high-resolution reliable data.
- L463-464: This sentence is unclear to me. Could you rephrase it?

Typos:

L25: I think the expression "uptaking" is incorrect and the correct version is "taking up".

L326: The signs "?:" seem to be written in place of " for".

L413: I think N2 requires a subscript for the number 2.