

# ***Interactive comment on “Hydrogen isotope fractionation affects the identification and quantification of tree water sources in a riparian forest” by Adrià Barbeta et al.***

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## **General comments**

Barbeta et al. present a very interesting and extensive stable isotope data set studying the ecohydrology of riparian forests in SW France. Their experimental design of fortnightly sampling over an entire vegetation period is very timely and has pioneer character as most studies addressing similar research questions were limited to few sampling campaigns.

They use well-established methods, both for data gathering (e.g., cryogenic extraction) and data (e.g., MixSIAR) and assess both critically. Their findings regarding a potential

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deuterium offset and its consequences for plant water uptake studies is timely and will be of interest to a wide range of scientists working with stable isotopes to investigate soil-plant interactions. Their study therefore fits to the scope of HESS and suggest publication after a minor revision addressing the issues raised below.

I am not sure if the title is correct, when talking about "Hydrogen isotope fractionation", as you cannot prove that it is a (natural) fractionation process, as there could be also methodological issues causing a mismatch between potential sources and plant water stable isotopes (as you discuss). Maybe "Deuterium offset affects..." is more appropriate.

There is a need for a more critical assessment of the sw-excess correction of xylem data. You refer to Bowling et al. (2017) in the conclusion (xylem water as a potential result of mixing of enriched top soil water and depleted subsoil water), when discussing alternative explanations for xylem water samples plotting below soil water samples in the dual isotope plot. This should be extended as the presented deuterium offset cannot be explained with the current field experiments. Further, xylem waters can also plot above the soil water samples in the dual isotope due to mixed water uptake of soil water and recent infiltrated isotopically enriched rain water. Correction with the sw-excess would not make sense in this case for example.

### Specific comments

L 41: Unclear how a climate refugia gets "buffered".

L 54: "the soil depth reached by infiltrating water" reads as if infiltrating water has some kind of boundary beyond which it cannot further percolate. I suggest to refer to mixing processes of water in the subsurface instead.

L 51: To clarify that you refer to sampling and not root accessibility, I suggest to say "... and can be sampled" (or similar).

L 94: However, Oerter et al. (2014) that you quote in L 89 actually showed this fractionation for clay minerals.

L 96: This is unclear. If there is only pore water in small pores, then there cannot be

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any interaction with mobile water, as it is not present.

L 145: How is the climate defined?

L 229: How was that tested?

L 252: How did you get volumetric soil moisture? I only see gravimetric moisture described in the methods.

L 253: How, where, and how often was the rock water content measured?

L 263 and following: It is not clear which statistical tests were applied, as they are not specifically mentioned in the methods section.

Fig. 2: Which rain data is shown? Rain that fell on the given days? Rain that fell between the 14 days previous sampling? If so, are they weighted averages?

You mention rock moisture in the caption, but I do not find it in the legend.

L 266: Not clear if the SWL was calculated for only topsoil or all soil samples.

L 269: Negative effect means more depleted in heavy isotopes with higher antecedent rainfall? Does the isotopic compositions of the antecedent rainfall correlate with the top soil isotopic compositions?

L 272: This explanation is not clear to me.

L 276: I do not see the rock moisture isotope data in Fig. 2.

L 282: Please reword, as “evaporation line” is evaporation line is defined as the linear change in the isotopic composition in dual isotope space of a single water source through evaporative concentration of the remaining liquid. However, these xylem samples were taken all at the same time. I do not question that they underwent evaporation and kinetic fractionation, but “evaporation line” is the wrong word.

L 286: Needs rewording, as “had a negative effect” implies that actually DBH influences the xylem water isotope compositions. I suggest saying something like “more depleted with higher DBH”.

L 291: This questions that grouping xylem and root samples in Fig. 2, Fig. 3, and Fig. 5. You should then present each data set separately there already. Or do you not present any extracted root water there? Not clear to me.

L 306: What does “only for the dominant trees” mean? Why not all sampled trees?

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Table S3: I encourage the authors to add this Table S3 to the main manuscript, as this presents a major finding of the study. Why did you decide to take 5-day averages and amounts?

L 354: The “water isotope fractionation occurring at the soil-root interface” would not cause the deuterium offset, as Vargas et al. (2017) describe it as a Raleigh type fractionation.

L 360: You mention a carbonate rich C Horizon in the methods. Could processes described by Meißner et al. (2014) be relevant here?

L 362: I would disagree with this generalization of more tightly bound water being generally more depleted, as it seems to be depending on the time of sampling: During summer, mobile water will be more enriched in heavy isotopes as the rainfall is relatively enriched, as the more tightly bound water, which is likely to have a isotopic composition that stems from winter (more depleted) rainfall/snowmelt. I discussed this issue in Sprenger et al. (2018). However, as most field sampling takes place during summer, various studies showed more isotopically depleted waters in more tightly bound water than mobile water (Geris et al., 2015; Oerter and Bowen, 2017).

L 375: Why “weak spatiotemporal variability”? The variability is partly very high (see *F. sylvatica* on July 4). It’s the pattern that is weak, which the current study cannot really well explain.

L 385: In the light of Zhao et al. (2016) and your discussion in L 390 and following, do you then think that cryogenic extraction is then the best way to sample xylem water? Consider discussing potential impacts of not limiting the sampling to the actual transpiration water when using cryogenic extraction. However, why is sw-excess then so variable?

L 429: As you pick up the Table S3 again as one of the main findings, I highly suggest including it in the main manuscript and not having it in the supplementary material.

L 432: This is a very important point you raise. How did you deal with xylem data of positive sw-excess? This could be a result of a mixed source (soil water plus recently infiltrated rain water). I can also imagine a situation, where an enriched soil water

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source in the top soil with a depleted source (e.g., snowmelt) in deep soil results in a xylem water isotope composition plotting below the sw-line, but does not result from deuterium fractionation. Consider discussing the limitations of your sw-excess correction.

## Technical corrections

L 18: No delta sign (applies to the entire abstract)

L 18: bi-weekly could mean twice a week or every two weeks (fortnightly); applies also to L 154 and L 259.

L 31: This is not a question. You either need to remove the question mark or rephrase.

L 39: ecohydrological

L 41: understanding

L 46: Somewhere here you need to introduce “stable isotopes of water (2H and 18O)”, as you currently miss this, but use 2H and 18O it in L 67 without having it introduced.

L 68: No hyphens

L 69: ...rainless summer fills small soil pores first and does not contribute to river flow nor to mixing. . .

L 74: TWW hypothesis holds, . . .

L 78: ...stream water for riverside trees.

L 96: ...when only water in small pores is accessible. . .

L 97: ... between sampled soil pore and ... (important to mention “sampled”, as this match is solely based on H1 and H2)

L 122: “to the data sets” appears to be more confusing than clarifying.

L 146: 813 mm/year (no digit needed and the unit is mm/year)

L 191: composition (given in delta notation . . .)

L 214: I suggest to not use sigma, but a different letter, since it could be misinterpreted as standard deviation.

L 224: were run

Fig. 1: It seems that there is something wrong with the x-axis.

The VPD data is shown as a continuous measurement, but the x-axis is not really a

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time axis, as the x-axis ticks are not equally distributed (e.g., every 14 days). Not sure if the date format is correct according to the Copernicus guidelines, and it's actually different format to Fig. 2.

The streamflow data does not look like actual data per second, but seems to be an average value for the individual days. If so, this should be mentioned. Add mm/day for precipitation.

Thus, GWC should have a different x-axis as the other three subplots.

L 260: I suggest defining delta notation in the methods and then you do not need to refer to VSMOW-SLAP scale here.

Fig. 2: I encourage the authors to consider using in addition to different colors also different markers for e.g., soils, vegetation, precipitation,... just to make it easier to see the differences in the dual isotope plots. Please also consider increasing the marker size. I suggest defining delta notation in the methods and then you do not need to refer to VSMOW-SLAP scale here. Typo: "...soil at two..." and "The blue line corresponds...black line corresponds to..."

Fig. 3: Consider adding "cm" to 50-120. I suggest defining delta notation in the methods and then you do not need to refer to VSMOW-SLAP scale here. Not sure if the date format is correct according to the Copernicus guidelines, and it's actually different format to Fig. 2.

Fig. 4: Please use different colors, as this is misleading when similar colors are used in Fig. 3 even though there is different data shown (soil vs roots). I suggest defining delta notation in the methods and then you do not need to refer to VSMOW-SLAP scale here.

Fig. 6: Why hyphenated plant-water?

## References

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