Response to referee (Matthias Sprenger)

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 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.

We appreciate that the referee found our study pioneering and timely as well as suitable to HESS readership. In the new version of the manuscript, we have now incorporated all his suggestions that we found very constructive and valuable. We are convinced that the manuscript has significantly improved after this revision. A revised version of the manuscript, with modifications in the text and the figures, will be posted after addressing the comments of the other two referees.

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.

We agree that we do not have an explanation for the observed isotopic depletion of twig water compared to all potential water sources. We could however rule out all the main methodological issues that could have caused such an offset, and also gathered evidence from the literature that this seemed to be a rather common phenomenon, although largely overlooked so far and still unexplained. To address the reviewer's concern that the title was not precise enough, we now propose a new and hopefully more accurate title:

"*Unexplained hydrogen isotope offsets complicate the identification and quantification of tree water sources in a riparian forest*".

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.

We agree that correcting xylem δ^2H with the SW-excess has limitations. In fact, this correction is not proposed here as a definitive solution for cases where depletion of δ^2H is suspected. It is rather used as an exercise to illustrate how sensitive the estimated source contributions are to the isotopic inputs. Bayesian models using $\delta^{18}O$ and SW-excess-corrected δ^2H led to water source contributions that compared well with those obtained from models using $\delta^{18}O$ alone. Also, the variability in source contributions explained by environmental conditions was similar between the two types of models, and with a higher predictive power than with MixSIAR models using just δ^2H or uncorrected δ^2H and $\delta^{18}O$ together. We interpreted these results as further evidence that deuterium offsets are at the origin of the problem and that they need to be accounted for to robustly identify tree water sources. We have now added the following text in the Discussion to acknowledge this important limitation of the SW-excess correction:

"Based on these correlations, such a correction using the SW-excess appeared to improve the predictive power of the dual-isotope approach. However, systematically correcting xylem data with the SW-excess may also be problematic because the SW-excess could carry other important information, especially when other water sources, not belonging to the soil water line, contribute to the xylem isotopic signal. Finally, spatiotemporal dynamics in the soil water isotope profile could also complicate the concept of the soil water isotope line and thus the SW-excess. The fact that we sometimes observed positive SW-excess indicates that we do not only correct for one single fractionation factor, and demonstrates the limitation of the SW-excess correction proposed here."

Specific comments

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

The sentence was not correctly worded. It is not the climate refugia that get buffered, but instead, they buffer the impact of climate variability in a particular site. The revised text reads now as it follows:

"This may also help understand how climate refugia facilitate the persistence of important biodiversity hotspots (McLaughlin et al., 2017)."

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.

We have now modified this sentence according to the referee's comment.

"Processes underlying the variability in source water isotopic composition include the temporal variability in the isotopic composition of rainfall and mixing processes of water in the subsurface (Allison and Hughes, 1983; Brooks et al., 2010), the evaporative enrichment of water in surface soil layers (Allison, 1982; Sprenger et al., 2016), the seasonality of groundwater and rock moisture recharge (Oshun et al., 2015) or isotopic processes during fog water droplet formation (Scholl et al., 2011)."

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

We added "and can be sampled"

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

To be precise, Oerter et al. (2014) found that cations adsorbed to clay minerals affect the $\delta^{18}O$ of CO_2 equilibrated with soil samples, compared to the $\delta^{18}O$ of deionized water initially used to moist the clays, and that the effect depends on the nature of cations (Mg^{2+} and Ca^{2+} deplete the equilibrated CO_2 $\delta^{18}O$, K^+ enrich it and Na^+ leave it unaltered). They explained their results by hypothesizing that "cations adsorbed to the clay surface form isotopically organized hydration spheres of water around them and thereby sequester these water molecules away from the bulk water" and thus from CO_2-H_2O equilibration reactions. Because the study of Brooks et al. (2010) was conducted on soils with high clay contents, we agree that the isotopic effects reported by Oerter et al. could have complicated the interpretation of the results of Brooks et al. We now refer to this possibility in the following sentence:

"Oerter et al. (2014) showed that cations adsorbed to clay minerals create isotopically organized hydration spheres of water around them and thereby sequester these water molecules away from the bulk water. However, even if the majority of the water contained in small pores is adsorbed water that does not interact with the more mobile water (the TWW hypothesis), in summer, when only water in small pores is accessible to the trees, there should be an isotopic match between soil pore and xylem water, unless (H1) is not true and isotopic fractionation occurs during root uptake. In this context, a recent controlled experiment…"

L 96: This is unclear. If there is only pore water in small pores, then there cannot be any interaction with mobile water, as it is not present.

We agree with the referee that if in summer there is only water in small pores, there is not any possibility of interaction between mobile and bound water and, should the TWW hypothesis be valid, such interaction would not even happen in spring although mobile water is more readily available. We think that the sentence, reformulated as mentioned above to refer to Oerter's study, clarifies this point.

L 145: How is the climate defined?

We have now added the definition of the climate of the area following the Köppen-Geiger classification:

"The studied area has a temperate oceanic climate (Cfb in the Köppen-Geiger classification). Daily meteorological data..."

L 229: How was that tested?

We are not sure if the referee refers here to the pooling of stream and groundwater or the exclusion of fog and rock water as a potential tree water sources. In the first case, stream and groundwater were not statistically different, so it was not possible to distinguish between them in the mixing-models. It could either mean that they are connected or that they are recharged by the same winter rain events. In the case that we had found a strong contribution of stream/groundwater, our data would not be able to clarify if this water pool accessed by the trees was connected to the stream or not.

If the comment referred to the fact that we excluded rock moisture as a potential source, this was decided based on the observation that rock moisture was very enriched compared to xylem water but also soil and stream/ground water. This is now shown in the dual isotope plot (Fig. 2), where rock moisture falls in the upper (enriched) part of the LMWL. Regarding fog, it is not just that we only started

collecting any fog in our collector by the end of summer (so obviously it could not be a source in spring or early summer), but also, as for rock water, fog had a very enriched signal that was never close to xylem water, neither could be involved in any sort of mixture. This is because a contribution of fog would move xylem water samples upper and to the right of the other potential sources in the dual isotope plot, which is the opposite case of what we observed. This is now clearer in the revised manuscript where we explain our choice of potential water sources only after showing their different isotopic composition for the different sampling campaigns (Fig. 2), i.e, lines 227-231 are now moved to the Results section, at the beginning of section 3.4:

"The potential tree water sources that we considered were restricted to the top and deep soil water and stream/groundwater. Stream and ground waters were pooled together as they were isotopically indistinguishable (Fig. 2). Fog and rock moisture were not included as potential water sources because their isotopic signatures were very enriched compared to xylem water but also soil and stream/ground water (Fig. 2), so that their contribution would have moved xylem water samples above and to the right of the other potential sources in the dual isotope plot, i.e. the opposite of what we observed. Also fog water could only be collected at the end of the summer, so is unlikely to have been a significant source of water in either spring or early summer. The first set of isotopic mixing models were only run for the dominant trees of F. sylvatica and Q. robur using…"

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

The volumetric water content of rocks was obtained by estimating that limestone rock had a density of 2.5 g cm^{-3}. This is now clarified in the text:

"Using a rock density of 2.5 g cm^{-3}, we estimated the mean volumetric water content of limestone rocks to be around 12%, which is comparable to that of deep soil."

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

Rock water content was measured in every sampling campaign since the second half of July and until the end of the growing season (November). We weighted the rock samples before and after cryogenic extraction, to calculate gravimetric rock water content. We ensured that all water content was removed by the cryogenic extraction by oven drying the material immediately after the extraction and re-weighting it.

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

In most of the cases we used generalized linear mixed models, or just general linear models when there were only fixed factors. Although we already mentioned this in the methods, we have now added the following text to emphasize it:

"The spatial, temporal, species-specific and size-related statistical comparisons between the isotopic compositions of grouped samples were analysed using linear models, or where plot and date were necessarily set as random factors, we used linear mixed models from the package lme4 in R. For instance, for comparisons between groups across several dates, the date of sampling was set as a random factor."

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.

Rain water was collected in every sampling date, so the water in the rain collector represents an averaged value of the water precipitated since the previous sampling. The collection was not done based on rain events. We have now clarified this in the revised manuscript, at the end of section 2.1:

"Each rain and fog water sample corresponds to the average (amount-weighted) value of the water that precipitated since the previous sampling date."

As correctly pointed out by the referee, rock moisture was not shown in Fig. 2, but we have now added it for consistency.

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

Yes, both top and deep soil samples were used, see L216-217:

"The slope and intercept were computed by performing a linear regression on all the soil water isotope data from the surface and deep horizons collected at a given plot and date"

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?

Yes, the higher the amount of rain fell during the period of collection (ca. 15 days), the more depleted was the top soil water composition.

L 272: This explanation is not clear to me.

We thank the reviewer for pointing this out. In fact, there was a confusion in the interpretation of the results, since it is top soil δ^{18}O that is negatively correlated with top soil water content, not δ^2H. We have modified the sentence and the interpretation of the results accordingly:

"In the top soil, δ^{18}O was significantly (P < 0.05) and negatively correlated with soil water content, but not δ^2H. This is surprising because isotopic fractionation occurring during soil water evaporation and water vapour and liquid diffusion should affect both water isotope signals in the same direction. The fact that these water signals respond differently to top soil water content but similarly to rainfall amount (see above) indicates that observed changes in top soil water isotope signals are primarily governed by precipitation input rather than soil water evaporative enrichment. It may also be that hydrogen isotope of soil water are reflecting extra fractionation processes (e.g. root uptake) compared to their oxygen isotope counterparts."

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

We have now included the rock moisture samples in this plot.

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.

We agree with the reviewer that "evaporation line" is not the correct term here. We have modified this sentence:

"Xylem water from the first campaign on the 19th of April (i.e. just before or during budburst), was exceptionally enriched (Fig. 3) and fell in the upper right part of the dual-isotope space (top left panel in Fig. 2), except for those trees that had already flushed their leaves"

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".

We have replaced "had a negative effect on" by "were negatively correlated with".

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.

Water extracted from above-ground coarse roots is also considered as xylem water. The δ^2H offset and their spatio-temporal patterns still persists after excluding the water extracted from (outcropping) coarse roots. In order to ensure that those trees did not drive all the statistical results, we selected "plot" as a random factor, to control for any effects related to sampling (root xylem samples were all from the same plot). We now clarified this point:

"The four trees (all on the same plot) in which xylem water was extracted from outcropping coarse roots (rather than from twigs) showed a significantly more depleted δ^2H over the whole season (P < 0.001), but no significant difference in $\delta^{18}O$, compared to all the other trees (Fig. 4). The δ^2H offset still persisted after excluding these coarse root samples, demonstrating that xylem water δ^2H exhibited different patterns than $\delta^{18}O$."

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

We only sampled non-dominant trees for *F. sylvatica*, not for *Q. robur*. Consequently, we preferred to compare only the water sources of dominant trees for this species comparison. This is now clarified:

"The first set of isotopic mixing models were run only for the dominant trees of F. sylvatica and Q. robur using both $\delta^{18}O$ and δ^2H data. Because non-dominant trees were only sampled for F. sylvatica, and not for Q. robur, we preferred to exclude them when comparing the two species."

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?

We agree with the referee. We now moved Table S3 to the main text. We chose 5-day averages and amounts because they gave the strongest correlations with source contribution overall (better marginal r-squared).

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.

We do not understand this comment. Vargas et al. do report a deuterium offset between xylem and soil water, with more depleted xylem samples compared to soil water. They conclude that this is caused by fractionation during root water uptake. They then elaborate on the effect of such a fractionation on

the remaining soil water pools, and conclude that this should cause a progressive enrichment of soil water via Rayleigh distillation effects, at least when soil water is scarce (i.e. in summer). Such mechanisms would completely support our results. In any case, here, we do not refer to one specific mechanism, but rather to an array of mechanisms that can occur in the soil-root interface, including those suggested by Vargas et al (2017).

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

This is a very good point. Indeed, similar to our study, Meißner et al. had to evoke either positive ($\delta^{18}O$) or negative (δ^2H) offsets between xylem and soil water in an attempt to reconcile both isotopes at identifying a common water source. They also performed extra lab experiments by manipulating clay and carbonate contents to try explain these putative offsets. They report that HCl-treated (i.e. carbonate-free) soil samples have a cryogenically-extracted water $\delta^{18}O$ in agreement with that of input water whereas the $\delta^{18}O$ of cryogenically-extracted water from carbonate-rich soil samples is depleted compared to input water. On the other hand, they find no effect of carbonate content on deuterium isotopes. They thus conclude that "H isotopes probably reflect the plant water uptake […] whereas O isotopes in extracted water are shifted to lower values as compared to [soil water and thus] plant water uptake". They suggest that the $\delta^{18}O$ depletion of extracted water was caused by temperature-dependent oxygen isotope exchanges between soil water and carbonates during the extraction, but they did not perform tests at different extraction temperatures to verify this was the case. Also the carbonate-induced O isotope effect that they report is about -1‰, which is only half of what would be required to fully reconcile their $\delta^{18}O$ and δ^2H data from the field. In our case, the presence of carbonates in the C horizon would be responsible for a $\delta^{18}O$ depletion of extracted water from the deep soil samples of about 1‰, i.e., the "true" soil water in this horizon should be shifted by about +1‰. This would slightly modify the SW-excess values but would not cancel the observed isotopic offset between soil water and xylem water. Therefore, the results of Meißner et al. (2014) are relevant, but do not explain the isotopic offset observed here. This is now stated in the discussion of the revised manuscript, in the second paragraph of section 4.1.

"Another possibility is that fractionation processes occur during water extraction. Meißner et al., (2014) reported that treating soil samples with HCl to remove carbonates prior to water extraction led to a cryogenically-extracted water $\delta^{18}O$ in agreement with that of input water, whereas the $\delta^{18}O$ of cryogenically-extracted water from carbonate-rich soil samples was depleted by about 1‰ compared to input water. On the other hand, they found no effect of carbonate content on hydrogen isotopes. They suggested that the $\delta^{18}O$ depletion of extracted water was caused by oxygen isotope exchanges between soil water and carbonates during the extraction, a process that should be temperature-dependent. Meißner et al. (2014) did not specify their extraction temperature but we expect it to be > 60°C, i.e. close to our extraction temperature of 80°C, so that we could expect a carbonate-induced isotope effect of comparable magnitude. If the presence of carbonates in the C horizon were responsible for a $\delta^{18}O$ depletion of extracted water from the deep soil samples of about 1‰, this would mean that the "true" soil water in this horizon should be shifted by about +1‰. This would slightly modify the SW-excess values but would not cancel the observed isotopic offset between soil water and xylem water. Therefore, although the results of Meißner et al. (2014) are very relevant to our study, they cannot explain the isotopic offset observed 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).

We agree that, in the conceptual framework of the TWW hypothesis, water in small pores (also referred sometimes to "bound" water, because assumed locked away from the more mobile water flow) is not necessarily more depleted, as it may come from isotopically-enriched rain events (e.g. during summer storms…). This said, in this part of the text we are not referring to "bound" water in the sense used by Brooks et al (2010), to cite an example. Note that we do not use in fact the term "bound", but the term "adsorbed". This is because we wanted to make a distinction between (1) bound water *sensu* Brooks et al. (2010), which means water that does not flow and remains in soil micropores and (2) adsorbed water, which is the physically adsorbed water on mineral or organic surfaces. This adsorbed water is held at higher tensions and should thus be less accessible by plants but is not necessarily disconnected from mobile water and can still exchange isotopically. This adsorbed water, at equilibrium, is expected to always be more depleted than mobile water (Lin et al. 2018; Lin & Horita 2016). Note however that we found clear depleted values of xylem water with respect to bulk soil water during the wetter periods, i.e., when the proportion of absorbed water relative to bulk soil water should be minimal and thus contribute little to plant water use. This seems to indicate that the isotope arrangement between adsorbed and mobile water within soil pores does not seem to be responsible for the hydrogen isotope offsets reported here.

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.

We agree that the term weak used to characterize the spatiotemporal variability in SW-excess was inaccurate, and unnecessary for the argument. This section has been re-structured and this expression was removed from the text.

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?

Cryogenic extraction retrieves all the water in the stem (after removing bark and phloem). Plant water storage pools connected to xylem vessels through a fractionating symplastic pathway with its own temporal dynamics could be behind the observed isotopic offset between bulk stem water and source water. It is technically challenging to design a non-fractionating extraction technique to separate these water pools in the stem. Vapor equilibration or probe-based sampling may produce similar results, but it remains to be tested. A possible explanation for the variability in the SW-excess is that the proportion of each of these water pools varies over time. We have reformulated slightly the end of the discussion on this topic (end of section 4.1):

"Interestingly, ray and axial parenchyma can account for around 31% of total xylem tissue volume in both F. sylvatica and Q. robur (Morris et al., 2016) whilst storage water in the stem can account for up to 16% of daily transpiration in F. sylvatica (Köcher et al., 2013), and contribute even more in some subtropical tree species (Oliva Carrasco et al., 2015). Thus future studies are now required to explore the role of symplastic water transport and storage as a potential mechanism leading to the depletion of bulk wood water $\delta^2 H$ compared to the actual source water signal. This mechanism may be quantitatively relevant for interpreting the isotopic composition of bulk xylem water in terms of source water and explaining the variability in SW-excess reported here."

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.

Following referee's advice, we have now included Table S3 in the main document.

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 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.

As explained in the text, in one of the Bayesian mixing models, all xylem δ^2H data were corrected for SW-excess. This means that positive offsets (very rare) were also corrected, although they clearly indicate that they were caused by multiple factors. We slightly modified the end of this discussion to warn about the limitations of the SW-excess:

"Based on these correlations, such a correction using the SW-excess appeared to improve the predictive power of the dual-isotope approach. However, systematically correcting xylem data with the SW-excess may also be problematic because the SW-excess could carry other important information, especially when other water sources, not belonging to the soil water line, contribute to the xylem isotopic signal. Finally, spatiotemporal dynamics in the soil water isotope profile could also complicate the concept of the soil water isotope line and thus the SW-excess. The fact that we sometimes observed positive SW-excess indicates that we do not only correct for one single fractionation factor, and demonstrates the limitation of the SW-excess correction proposed here."

Technical corrections

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

We revised the entire abstract accordingly.

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

We have replaced bi-weekly by fortnightly throughout the manuscript to avoid the possible confusion.

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

We reformulated the question to:

"Why is an improved understanding of tree water use needed?"

L 39: ecohydrological

Changed.

L 41: understanding

Changed.

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.

We introduce them before now.

L 68: No hyphens

Changed.

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

Changed.

L 74: TWW hypothesis holds, . . .

Changed.

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

Changed.

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

Changed.

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

Changed.

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

Deleted.

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

Changed to 813 mm yr^{-1}.

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

Changed.

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

We changed σ to Γ to avoid any confusion.

L 224: were run

Corrected.

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 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.

The referee is right regarding the coherence of the x-axis. We previously decided to simplify the figure by using a common x-axis. We have now redrawn the figure, with the correct and common time axis for all variable. They all belong to the same period, but there is daily (VPD and rainfall), monthly (streamflow) and fortnightly (GWC) data. We corrected the units on the y-axis and the figure legend accordingly.

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

We agree that the isotopic scale should be stated in the Methods section. We included a sentence there (at the end of section 2.2):

"*All isotopic data reported here are expressed on the VSMOW-SLAP scale.*"

The reference to VSMOW-SLAP on l. 260 was then deleted. On the other hand, we kept the scale on all the figures to make sure they are fully comprehensive on their own.

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. . ."

We have re-drawn this figure, in order to include rock and fog water for the campaigns that those were available. We also increased the marker size to improve the visualization, but preferred not to use different marker types given the large number of categories. The high-resolution figure published in the final version of the manuscript should be good enough to distinguish the already contrasting colors.

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.

We added cm in the figure legend as proposed but kept reference to the VSMOW-SLAP scale for reasons explained above.

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.

The brown color used here for roots is exclusive to this type of sample/plant organ. However, we agree that it may be hard to distinguish from the brown color used to indicate top soil water in the other figures. We thus decided to use only green colors for this figure and to distinguish between light (*F.

sylvatica) and dark (*Q. robur*) greens as in the other figures. The x-axis is clear enough to separate twig and root samples without a color scheme.

Fig. 6: Why hyphenated plant-water?

We have removed the hyphen.

References

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