Response to referee (Youri Rothfuss)

General comments

In their manuscript submitted to HESS and currently in its discussion phase, Barbeta and colleagues investigate the water sources of *F. sylvatica* and *Quercus robur* (L.) on basis of water stable isotopic measurements in sap xylem, stream/groundwater, and soil water in a vertical profile. For this, they opt for the most commonly used method, which is inversing the isotopic data with statistical modeling. They choose MixSIAR, a mixing model embedded in a Bayesian framework. A particularity of this study is that xylem water isotopic samples of both species very often not plot on the soil evaporation line in a ($\delta^{18}O$, $\delta^{2}H$) coordinate system, which is explained by the authors as being due to a significantly higher hydrogen than oxygen isotopic fractionation during root water uptake. The manuscript is generally well written (aside sometimes from the isotopic terminology – see my specific comments), easy to follow, and falls into the scientific scope of HESS. My general concern is that there is a major contradiction between the first part of hypothesis H2 ("it is essential that all potential water sources are identified and accessible") and the fact that the authors deliberately do not sample from the soil between 10 and 70 (or 110, "depending on the depth of the rocky layer") cm depth. In a review article (Rothfuss and Javaux, 2017), a simple synthetic experiment showed how much the non-fulfillment of H2 could lead to biased estimates of the relative contributions to plant root water uptake across a set of potential sources. If the authors can provide proof that the "mid-soil" is isotopically not any different than another source (e.g., deep soil), then it can be pooled together with this other source (e.g., "mid-deep soil"). If this is not the case, the authors' analysis might not be valid. For instance, we cannot say for sure that the observed hydrogen isotopic fractionation is not in fact partly due to the values of isotopic compositions in the missing soil water source.

We appreciate that the referee found our study well written, easy to follow and falling into the scope of HESS. We have followed his comments and suggestions to improve the general quality of the manuscript, notably regarding the "isotopic terminology". We have also addressed the concerns of the referee in relation to our sampling strategy and associated analysis. We fully agree that the identification and sampling of all possible water sources (H2) needs to be fulfilled in order to achieve realistic estimations of water source contributions. However, we have several lines of evidence that this was the case in our study, as explained below.

Our sample strategy of soil water samples was designed to capture the spatial variability of soil water isotopic composition as much as possible. We selected three plots that differed in their microclimatic and topographic conditions (that in the end did not imply differences in tree water uptake), and in each of these plots, we sampled three subplots. This allowed capturing both the plot- and tree-scale variability in soil water isotopes. We expected that the temporal and spatial variability of soil water isotopes would be large in the top soil and small in the deep soil, because of the spatially heterogeneous effect of soil evaporative enrichment that will vary with tree cover, sunflecks frequency, throughfall, stemflow, windspeed… The extent of evaporative enrichment of soil water isotopes is commonly limited to the top 20-30 cm of the soil (Sprenger et al. 2016). Thus, our sampling strategy of collecting only top (10cm) and deep (70-110cm) soil water was an efficient way to capture the two ends of the evaporative enrichment soil water isotope profile.

We recognize that soil water evaporative enrichment can sometimes extend to deeper soil layers (e.g. 50 cm) but only under prolonged periods of high evaporative demand and low soil water contents, typical of Mediterranean or semi-arid regions (Allison et al., 1983). In these situations, the evaporative front is usually located below the soil surface and the isotope profile displays a maximum value at the location of the front rather than at the soil surface. However, such situations are unlikely to occur at our studied site, especially during the year of the sampling (2017) that was dry only very early in the season when evaporative demand was low (as noted in the manuscript, the permanent wilting point was reached in the top soil only at one date in September).

Our expectation of low spatio-temporal variability of soil water isotopes in the deep layers (70-110cm) was confirmed by our data (Figs. 2 and 3), indicating that most of the variability in soil water isotopes came from changes in evaporative enrichment and recharge of the top soil layer by summer rainfall. We also noted that the deep soil and top soil isotopic composition often plotted in the dual isotope space on an evaporation line (Fig. 2), implying that the water isotope composition of middle soil layers must necessarily fall in between. Only when the sampling date followed abundant rainfall (e.g. July 4th, Fig. 2), the soil water line was somewhat altered and the top soil layer displayed a water isotope composition close to rain water and more depleted than deeper in the soil profile, i.e., the opposite of an evaporative front. We already reported in the manuscript that the estimation of source contributions in such situations may be misleading (Fig. 8).

Our conclusions are also confirmed by a more intensive sampling campaign performed at one date in late summer 2018 where soil samples were collected every 15-20cm from soil surface to below 70cm (Fig. S1, shown below), The water isotope profile in the soil from this campaign displays a typical evaporative enrichment profile with more depleted values at depth and no statistical differences among the soil layers below 35 cm for both water isotopes (Fig. S1). This reinforces our argument that below 35cm, the isotopic composition is fairly stable, and sometimes very similar to that of groundwater and stream water (Fig. 2). Although this "test" sampling was only done for one date in late summer when evaporation had time to shape this isotopic profile in depth due to dry conditions, we think that this was the most common case for our sampling campaigns in 2017, given the clear soil evaporation lines we could draw for each date (Fig. 2).

To sum up, our data seems to support the appropriateness of grouping several deep layers into one "deep soil" water pool, which was also considered the better option for balancing our sample processing capacity and the information provided by the data. In addition, we find it highly unlikely that the depleted δ^2H of xylem water is caused by water uptake from this middle layer. This would require a pervasive (over time and space) negative peak in δ^2H in this middle layer. We cannot think about any ecohydrological or physico-chemical process driving a -8‰ depletion in only one of the isotopes and in a targeted soil layer, without affecting the surrounding layers.

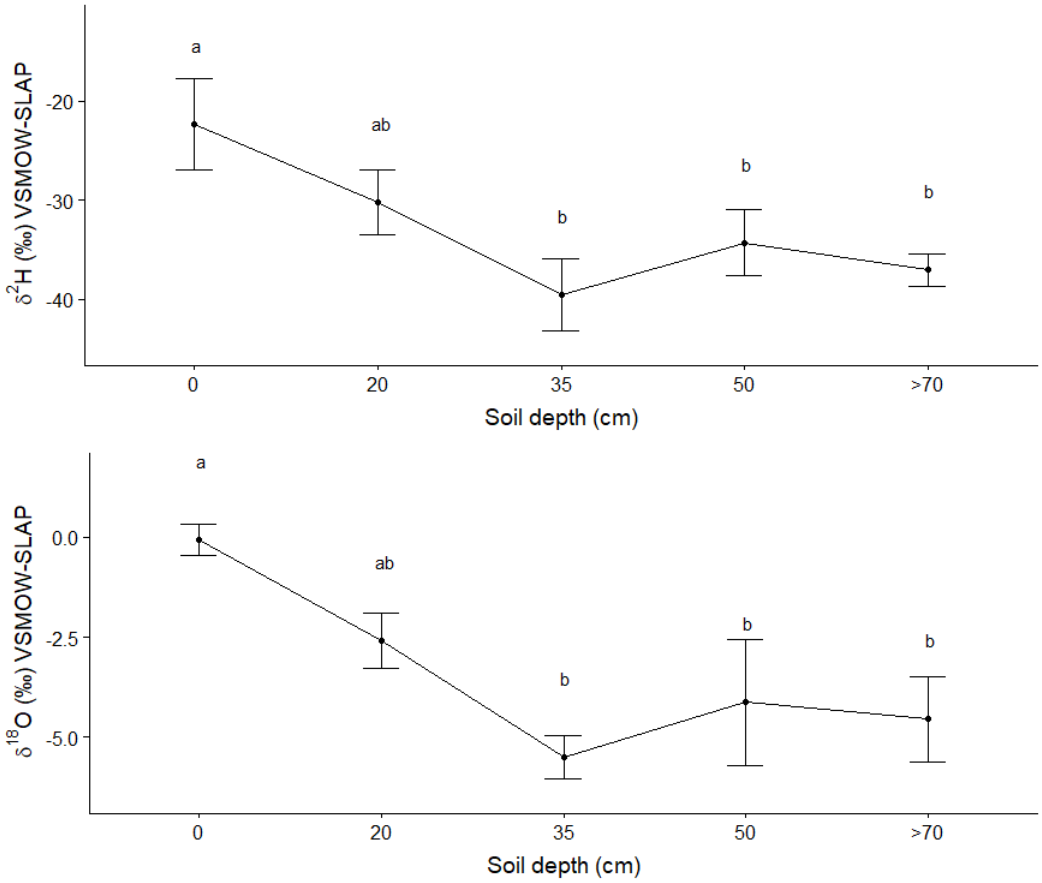

Fig. 1. $\delta^{18}O$ and $\delta^{2}H$ at different soil depths. Different letters indicate significant differences between depths (*P*<0.05).

Specific comments

L12. "The stable isotopes are powerful tracers. . ."

We have slightly modified the Abstract and omitted this sentence.

L15. "that xylem water isotopic compositions effectively reflect source water isotopic compositions." Same goes for the following sentence.

The first sentence was changed to "that the stable isotope composition of xylem water effectively reflects that of source water".

The following sentence was changed to "However, this assumption has been called into question by recent studies that found that, at least at some dates during the growing season, plant water did not reflect any mixture of the potential water sources."

L17. "In this study,. . ."

Changed.

L17-19. A biweekly temporal resolution does not qualify as "Highly resolved" (L16). It seems, however, that it is what the authors mean here.

We meant exactly what is said, i.e., that "Highly resolved datasets covering a range of environmental conditions could shed light on possible plant-soil fractionations processes." Techniques monitoring continuously (i.e. at a hourly time step) the stable isotope composition of xylem and soil water are currently under development and already providing interesting results, as mentioned by the referee below. Unfortunately, to the best of our knowledge, such methods have not yet been implemented to report data for extended periods of time (*e.g.* a full growing season) and thus over "a range of environmental conditions". This is the latter aspect that we tried to cover with our sampling strategy.

L19. "Using a Bayesian isotope mixing model (MixSIAR), we then quantified the relative contribution to root water uptake of . . .". It is important to emphasize the word "relative".

Changed.

L23. "Xylem water could always be interpreted as a mixture of deep and shallow soil waters from δ18O data, but the δ2H of xylem water was often lower than any other possible water source.". (1) Some isotopic composition value cannot be conceptualized as a water mixture and (2) cannot be depleted/enriched.

1. The sentence does not imply that xylem water can only be a mixture of deep and top soil water (or any other source), but rather that this is what we observed in this particular site and growing season (for $\delta^{18}O$). This is compatible with situations in which xylem water is not a mixture but it is sourced in just one soil layer or any other water pool.

2. We believe that the term "depleted" when used to compare the isotopic composition of two water pools is perfectly valid and generally accepted.

L25 and 27. "δ2H decrease" (or analogous) and not "depletion".

We have kept the term "depleted" that we consider correct, because it is explicitly said in the previous sentence that this depletion is relative to the other water pools (i.e. "more depleted than any other possible water source"). We felt unnecessary to repeat it again in this sentence, and did not want to use another word (offset, decrease…) that would only bring more confusion.

L26. "we found that the localization of plant-water source. . ."

Changed.

L43-60. There are other assumptions made for determination of relative contributions to RWU across potential sources than the authors H1 and H2, e.g., no sap tissue capacitance, perfect mixing in the sap and no xylem-phloem exchange at the output of the root system, etc. You say this, but the reader has to wait until the discussion of your results.

We agree. However, since this is not a review paper, we did not want to extend too much on the methodology and its underlying assumptions. We revised assumption H1 to include these other assumptions that the referee mentions without going into much more details:

"Firstly (H1), it is assumed that isotopic fractionation during root water uptake (and/or xylem water redistribution) does not occur…"

L46-47. “The water isotope tracing methodology is commonly used to study plant water uptake. . .”. Here you may cite our review paper on the subject (Rothfuss and Javaux, 2017). It better illustrates your point than the study of Dawson et al. (2002) (which is not in the reference list) as it focuses on RWU solely.

We now cite Rothfuss and Javaux (2017) here and have sorted out our reference list. Thank you for pointing this out!

L52. Why “However”?

We have removed it.

L54. “the temporal variability in rainfall water isotopic compositions. . .”

Changed to “temporal variability in the isotopic composition of rainfall”.

L56-57. “or the relatively higher isotopic composition in fog compared to rain water”

Changed to “or isotopic processes during fog water droplet formation”, as fog is not necessarily more enriched than rain water at the monthly scale.

L57-60. Not only. Laser-based spectrometers only allow for a retrospective (off-line) assessment of RWU. Do not forget the development of non-destructive methods for determination of δsoil and δRWU (Volkmann and Weiler, 2014;Oerter et al., 2016;Gaj et al., 2015;Rothfuss et al., 2013;Volkmann et al., 2016).

We fully agree but again, this is not a review paper. Here we just wanted to emphasize the fact that the development of laser-based spectrometers opened up the possibility to perform more extensive retrospective assessments of RWU, which is fully relevant for our study.

L59. “hydrogen and oxygen isotope”.

Changed to “hydrogen and oxygen stable isotopes”.

L81. You take too much liberty with terminology. How could “xylem water isotopes” match some “source”? You could write for instance “Plant water source studies in which the xylem water isotopic composition does not spread within the range of the sources’ isotopic compositions. . .”.

We appreciate the suggestion of the reviewer and have modified the text accordingly.

L83. “hypothesis (H2) is not met”

Changed.

L86. What does “carry an isotopic signal” mean? I suggest sticking with an “isotopic composition” which is “lower”/”higher”. . .

“Isotopic signal” is commonly used in the isotope geochemistry literature without ambiguity. We nevertheless followed the suggestion of the referee and modified the sentence, that now reads:

“Moreover, the water stored in soil rock fragments can have an isotopic composition distinct to that of soil water or groundwater, being either relatively more depleted (in the case of δ^2H in Oshun et al., 2015), or more enriched (Palacio et al., 2014; Rong et al., 2011) “

L90-91. It reads as if "the isotopic composition of rock water" is "a relevant, alternative plant water source". Please revise grammar.

We have corrected the sentence as follows:

"Thus, wherever weathered rocks constitute a large fraction of the soil volume, the isotopic composition of rock moisture should be measured as rock moisture could constitute a significant alternative plant water source."

L101. Do you mean "discrimination increased with decreasing soil water content"?

No, we meant that "discrimination increased with soil water loss" (or cumulative transpiration), which is what the authors of that study reported. In their study, the explanatory variable was not "soil water content", but "soil water loss", estimated by gravimetric methods. For different soil textures or soil moisture levels, soil water loss could be equal but soil water content could differ significantly.

L104-105. "In fact, a growing number of studies are observing lower xylem water isotopic compositions compared to those of the considered sources".

We rephrased it to:

"In fact, a growing number of studies are reporting xylem water with an isotopic composition that is depleted relatively to that of the considered sources".

L108. "The effect of deuterium fractionation on the quantification of sources' relative contributions to plant water uptake. . ."

Changed.

L109. "hydrogen isotopic composition only. . .".

Changed to "hydrogen isotopes only"

L114-121. This belongs to Material and Methods.

We do not agree with the referee. This section describes the ecological questions addressed by our study. Even if the focus of the study is more related to issues with the application of stable isotope techniques, we are still interpreting the results from an ecological point of view in the Discussion. Because this section is not merely a site description, we considered preferable to keep it here at the end of the Introduction rather than moving it to the next section in the Material and Methods.

L128. What do you mean by "extensive" here? Be more precise in the description of this objective already in the introduction.

We meant that our dataset is rather large in comparison with similar studies (not all, obviously). We have reworded this sentence.

"In parallel to the ecological focus of our study, the reported isotopic dataset spanning a whole growing season was also used to explore the potential effect of isotopic fractionation on the quantification of tree water sources."

L154. Of which two plots?

Thank you for noting this. Non-dominant trees were selected in only 2 of the 3 sampling plots. We rephrased it to:

"in two of the plots".

L155. "In order to measure the xylem water isotopic composition"

Changed.

L159-160. To "Each soil core was split into top soil (0-10 cm) and deep soil (from 70-80 to 110-120 cm depending on the depth of the rocky layer): even though you underline in the introduction the importance of identifying all water sources (i.e., hypothesis H2), you deliberately omit to sample the soil between 10 and 70(110) cm.

The full answer to this comment is provided at the beginning of this letter.

L178-179. I suggest to move this at the end of the §(L148).

We moved this section as proposed, before "The studied area has a mean annual temperature…".

L185. I propose: "the pressure in the extraction line. . ."

Changed.

L208. Which "significant difference"?

It refers to the isotopic composition of xylem water and its sources. This is now clarified:

"Because no significant difference was found between the isotopic compositions of xylem (or water sources) between the different studied plots…"

L209-214. A "mismatch" is not precise enough. You may write that you want to "assess if the isotopic compositions of the xylem water samples fall onto the evaporation line in a dual isotopic space". Also I would only present the concept of SW-Excess you are introducing and say that it is an adaptation of the LC-Excess.

The evaporation line is not the correct concept here. Still, we replaced "mismatch" by "isotopic offset" in order to improve clarity.

L211-212.To "However, because the source water for a tree is more likely to be made of soil water than rain water directly": it is a strange thing to say that trees would directly extract rain water just "less likely" than soil water.

We agree that rain water can sometimes be the source of water of trees, especially just after summer rain events. However, in this case, this would also be reflected, at least partially, in the isotopic composition of topsoil water. Thus, the distinction between rain water use and soil water use seems adequate to us.

L215. σ and Λ does not seem appropriate symbols, e.g., σ normally stands for standard error. I suggest other symbols like "c" and "d" or "A" and "B", "a"' and "b"'etc.

We have now substituted σ by Γ.

L218. To “δ2H separation”. Since by “δ2H”, you mean a numerical value, you shouldn’t use “separation”, rather “offset”, “difference” etc.

Changed by “offset”.

L223-227. Why “models” (plural)? MixSIAR is one single model that has different scripts interacting with each other. To “Models were ran in the script version of the package”: what other versions of MixSIAR are available? To “the number of Markov chain Monte-Carlo iterations was increased until convergence was reached”: did you do this yourself, or was the number of runs optimized automatically? To “Gelman and Geweke diagnostics”: what are these? To “residual error term in the isotope mixing models”: what is this?

We appreciate the comments of the referee, pointing out the lack of clarity in the description of our methodology. Although we only used MixSIAR, so the same model, every “run” is a differently parametrized mixing model. This is now clarified:

“The contribution of different water sources to that of xylem water was estimated using the MixSIAR package (Stock and Semmens, 2016) in R (R Core Development Team, 2012). Different mixing models were ran in the script version of the package…”

Yes, the number of MCMC iterations was not pre-set so we did it ourselves. Because the convergence can take more or less iterations to occur depending on the input data, we adjusted different number of iterations until we found that convergence was reached. This was checked for each individual mixing model. Gelman and Geweke are convergence diagnostics, so we based our decisions on the length of the chains based on a certain threshold value of these. This is now clarified:

“… and the number of Markov chain Monte-Carlo iterations was increased manually (by trial and error) until convergence was reached and the results for the Gelman and Geweke diagnostics were acceptable.”

In MixSIAR, there is the possibility to calculate the source contribution for individual trees or for a group. In our case, we ran the models for all the trees in the same plot and from the same species. The authors of MixSIAR recommend to include the residual error term when calculating source contributions of grouped individuals, as it allows to account for unknown sources of error on the observed data. More details can be found in the MixSIAR package manual, or the related publications (Stock and Semmens, 2016, Parnell et al. 2010, see manuscript’s reference list).

L228. Please define “top” and “deep soil water”.

This is already defined in section 2.1.

L231-241. You only test the sensitivity of the isotopic mixing model to the definition of the “product”, meaning the sap xylem isotopic composition. What about the definition of the “sources”? I am afraid that, since you do not fully characterize the ensemble of potential water sources, you cannot assess their contributions to tree RWU (see my general comment).

This point is discussed in the response to the general comment of the referee.

L257. To “3.2. Stable isotopes of tree water sources”: do the tree water sources have distinct “stable isotopes” than other sources:)? You might write something like “Stable isotopic composition distribution across tree water sources”. Same goes for 3.3.

We have modified this according to the suggestion of the referee.

L258-259. Belongs to M&M.

We have moved this sentence to section 2.1 of Material and Methods and added a shorter sentence here:

"The long-term (2007-present) local meteoric water line (LMWL) using the closest GNIP station (see Material and Methods) is shown in each panel of Fig. 2."

L266. A slope value of 9.99? Is this a typo? If not, what is the p-value of the linear regression?

No, this is not a typo. The sampling of July 4th was done following a 5-day rain episode that left more than 100 mm of rain. This caused that the top soil water became more depleted than the deep soil water, producing a soil water line with a slope steeper than that of the LMWL. The p-value of the regression was <0.0001. This can be noted in the corresponding panel of Fig. 2. But note also that the soil water line on this date is clearly not an evaporation line (otherwise topsoil should be more enriched than deep soil), i.e., the slope should be reported as -10 rather than +10…

L275. "in both hydrogen and oxygen heavier isotopes"

We have reworded this sentence:

"Finally, δ^2H and $\delta^{18}O$ of rock moisture were significantly more enriched than those of top and deep soil water"

L277. "The isotopic composition of rock moisture". In general, I suggest that you avoid use of "signal" throughout the MS. L281. No comma in ". . .budburst), was. . ."

We consider that the term isotopic signal used as a synonym of isotopic composition is widely accepted and understood by the community. In some cases and with the aim of improving readability, we consider it as the most appropriated term. However, we would change it all over the manuscript if the editor considers that it would improve the text or its comprehension.

L284. "had a lower δ2H"

In our opinion, stating that the isotopic composition of X is more depleted/enriched than that of Y is an appropriate expression. It would not be correct just using it to characterise a single water, but we consider it correct when used in relative terms to compare between two or more waters.

L289. "F. sylvatica presented higher δ18O values ($P < 0.05$)". Same goes for the rest of the MS: a value cannot be enriched/depleted.

Same as above.

L305-333. You are using the same Bayesian inference model with different parameterization, not a series of different mixing models. This might be confusing to the readers.

We find it appropriate to speak about different models, even if the type of model is the same. We believe that this use of the plural of models is commonly accepted and understood. For instance, when running Generalized Linear Mixed (GLM) models, one can speak about different models when using

different response and predictor variables, all they are all GLM models. If the editor thinks that our analyses can be confused by the readers with a comparison between isotopic mixing models (IsoSource, SIAR, MixSIR, MixSIAR), we could modify the text accordingly.

L338. "to identify plant water sources and quantify their relative contributions to tree RWU. . ."

We thank the referee for this suggestion. We have modified the beginning of the Discussion (see the following response).

L338-341. I propose something like "lower xylem water isotopic composition than any combination of those of the identified water sources would give".

We have modified the sentence as follows:

"Our results support those from recent studies reporting xylem water with an hydrogen isotope composition more depleted than any potential water source, and thus of any of their combination."

L341-342. To "The diversity of methodologies used for the extraction of waters and their isotopic determination in all these studies, including ours, rules out potential analytical bias.". Why could these methods not all be biased? Orlowski et al. (2016a) and Orlowski et al. (2016b) have shown discrepancies between the different methods for water extraction.

Here we meant that similar results have been reported using all sorts of water extraction and isotopic determination techniques. Issues associated with these techniques may produce a diversity of biases, e.g. extracted soil water being more depleted than input water (bulk soil), positive or negative effects of different types of clay-related cations on soil water, or organic interference during isotopic determination producing enriched/depleted isotopic compositions respective to the original sample. These effects can be of opposed sign, which minimizes the likelihood of a by chance commonly observed phenomenon. In addition, in our case, the results could not be explained by any of these biases, at least those that have been already reported in the literature. Still, we acknowledge that minimizing the likelihood is not the same as completely ruling out. We have thus modified this sentence:

"The diversity of methodologies used for the extraction of waters and their isotopic determination in all these studies, including ours, minimises the likelihood of a common analytical or methodological bias"

In addition, we have now added a full paragraph discussing the possibility of fractionation processes occurring during cryogenic extraction for our particular case. Notably, we argue that reported effects effects of carbonates in the soil water isotopic composition could not be responsible for the isotopic offset observed here and thus would modify the conclusion drawn.

"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"

L343 and after. If you mean by "offset" the "SW-Excess", then say so. Otherwise, define each time what are these offsets (e.g., is offset the same as in the study of Evaristo et al., 2017?)

We have now specified that we refer here to "δ^2H" offsets between xylem and source waters (see above). The SW-excess is an approach to quantify the isotopic offset between xylem water and soil water but we wanted to also include offsets with any potential source or their combinations.

L344. "Sternberg"

Changed. We also rephrased this sentence:

"Furthermore, isotopic offsets between soil and xylem water in potted plants (Ellsworth & Stenberg 2007; Vargas *et al.*, 2017) and botanical gardens (Evaristo et al., 2017) have also been reported and discussed these to some extent."

L380. How do you define a "similar offset" for δ2H and δ18O? Stating that both are linearly linked (e.g., δ2H = a*δ18O + b), would it mean that Δδ2H is similar as Δδ18O if Δδ2H = a*Δδ18O ??

We have completely rewritten this section:

"Thus, although empirical evidence for an isotope separation between bulk and plant-accessed soil water pools is growing, we do not think this is the cause of the isotopic offset reported here. Otherwise, we would expect both hydrogen and oxygen isotopes to be affected and the isotope separation between plant and bulk soil waters to be weaker when soil water content is large. Instead we found for all our trees a significant δ^2H offset between xylem and soil water sources (Fig. 5), even at times when soil water content was large (Fig. 1).

Similarly we de not think that branch evaporation is responsible for the reported isotopic offset (Martín-Gómez et al., 2017). If it were the case, we would expect to vary over the season with evaporative demand and to affect both hydrogen and oxygen isotopes, i.e., the opposite of what is found here.

We found differences in SW-excess when xylem water was collected from coarse roots rather than twigs (Fig. 4). Previous studies have shown that water in coarse or tap roots can exhibit significant depletion in δ^2H relative to source water pools for examples in *Populus euphratica* (Zhao et al., 2016) and *Prosopis velutina* (Ellsworth and Williams, 2007). Moreover, the δ^2H offsets reported between soil and root water were of the same order of magnitude (ca. -20‰ for *P. euphratica* and ca. -7‰ for *P. velutina*) as observed here for *F. sylvatica* and *Q. robur* (Fig. 4 and Fig. 5). Interestingly, Zhao *et al.*, (2016) analysed xylem water and what they called tissue water (*i.e.* bulk plant water) separately with the former not showing any isotopic depletion compared to source water. In this context, Ellsworth & Williams, (2007) attributed…"

L416. Rothfuss and Javaux (2016) is the discussion paper, not the actual article (Rothfuss and Javaux, 2017), please edit this.

We have corrected this throughout the manuscript.

L419. Define "SWexcessx" (i.e., already in the M&M).

This was a remaining from a previous draft. We are now using the term SW-excess throughout the manuscript.

L422-424. Using both δ18O and δ2H values does not mean you work in a bidimensional space. This is strictly speaking a 2x1D space since δ18O and δ2H across water sources are linearly linked. To be able to assess relative contributions to RWU in a 2D space, you need to break this linear bond (via isotopic labeling; e.g., Beyer et al., 2016), so that these water sources spread on some area rather than on some line in a (δ18H, δ2H) coordinate system.

We disagree because the fractionation factors during natural processes do not affect the two water isotopes similarly, i.e. they are not related in a mass-dependent fashion as for $\delta^{17}O$ and $\delta^{18}O$ for example. In any case, we do not understand how this comment relates to the lines 422-424.

L427. "deuterium fractionation"

Changed to "hydrogen isotope fractionation".

References

Beyer, M., Koeniger, P., Gaj, M., Hamutoko, J. T., Wanke, H., and Himmelsbach, T.: A deuterium-based labeling technique for the investigation of rooting depths, water uptake dynamics and unsaturated zone water transport in semiarid environments, J. Hydrol., 533, 627-643, 10.1016/j.jhydrol.2015.12.037, 2016.

Gaj, M., Beyer, M., Koeniger, P., Wanke, H., Hamutoko, J., and Himmelsbach, T.: In-situ unsaturated zone stable water isotope (2H and 18O) measurements in semi-arid environments using tunable off-axis integrated cavity output spectroscopy, hydrol. Earth Syst. Sci., 20, 715-731, 10.5194/hess-20-715-2016, 2015.

Oerter, E. J., Perelet, A., Pardyjak, E., and Bowen, G.: Membrane inlet laser spectroscopy to measure H and O stable isotope compositions of soil and sediment pore water with high sample throughput, Rapid Commun Mass Spectrom, 10.1002/rcm.7768, 2016.

Orlowski, N., Breuer, L., and McDonnell, J. J.: Critical issues with cryogenic extraction of soil water for stable isotope analysis, Ecohydrology, 9, 3-10, 10.1002/eco.1722, 2016a.

Orlowski, N., Pratt, D. L., and McDonnell, J. J.: Intercomparison of soil pore water extraction methods for stable isotope analysis, Hydrol. Process., 30, 3434-3449, 10.1002/hyp.10870, 2016b.

Rothfuss, Y., Vereecken, H., and Brüggemann, N.: Monitoring water stable isotopic composition in soils using gas-permeable tubing and infrared laser absorption spectroscopy, Water Resour. Res., 49, 1-9, 10.1002/wrcr.20311, 2013.

Rothfuss, Y., and Javaux, M.: Reviews and syntheses: Isotopic approaches to quantify root water uptake: a review and comparison of methods, Biogeosciences, 14, 2199- 2224, 10.5194/bg-14-2199-2017, 2017.

Volkmann, T. H., Kühnhammer, K., Herbstritt, B., Gessler, A., and Weiler, M.: A method for in situ monitoring of the isotope composition of tree xylem water using laser spectroscopy, Plant Cell Environ, 10.1111/pce.12725, 2016.

Volkmann, T. H. M., and Weiler, M.: Continual in situ monitoring of pore water stable isotopes in the subsurface, Hydrol. Earth Syst. Sc., 18, 1819-1833, 10.5194/hess-18- 1819-2014, 2014.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2018- 402, 2018.