

Review Hess Zuecco et al.

Much improved version of the manuscript, thanks.

Abstract. No comments

Introduction:

I think you greatly improved the introduction and I have only some minor comments:

Line 39-40: water representative of transpiration needed in ecohydrological studies. With your findings the method of choice would be SPC. Pick that thread up in the discussion again.

Line 80-81: Barbeta et al. observed this depletion, but the problem that it was related to CVD and not the plant fractionation was identified by Chen et al. 2020, and Allen and Kirchner 2021). If you could add these references (see below) and put them in context with Barbeta et al. 2019 that would be great.

Site descriptions:

Line 109 and 113 what do you want to tell the reader with the Engel et al. in review reference? To look for a better site description in there? I think it is irrelevant and I would delete it, especially because this has not been published yet.

Line 125 again, why is it important to have this reference here? I think your site description is sufficient as is, but if you insist on adding a reference here, please add what it is for.

Table 1 belongs in the Results section, but while looking at it, how do you deal with the tiny deviations from the LMWL (i.e. the small l_c values)? See (Landwehr and Coplen 2006) who indicate that l_c 's smaller than can be detected by the measurement precision are considered not to differ from LMWL (here, it looks like this could be the case for Alder CVD-WC, Chestnut CVD-T, Beech SPC and CVD-TwB and -TcT and maybe even more) could you add the calculations for S in regard to the measurement precision of the used instruments (as you write in the MM 2.5permil for δ^{2H} and 0.1permil for δ^{18O}) and then indicate in parentheses which values are and are not different from the LMWL in your case? It would give the reader the chance to directly assess differences.

Line 136 and again, emphasize what you want to say with adding these references here, or delete them if they are not giving additional information.

Material and Methods:

Line 234 the Landwehr and Coplen reference is dated to 2004 in your manuscript, however when I checked it again on Google Scholar and Web of Science the date is 2006, please change it to the correct date.

Generally, I am missing the explanation of how and why you weighted the means of the measurements? I know it says it in the header of table 1 but it should be part of the MM for completeness sake. What did you define as the mean extracted volume? Did that then differ for each method or each species or each tissue? And if so, what is the benefit of weighting them, the measurement device takes the same amount of sample for injection, right?

Also, I think I was misunderstood in the last revision round when I asked for a volume weighted isotopic composition, I meant if you could do a mixing ratio calculation where (in theory) you would mix the samples CVD_L and CVD_TWb together (weighted by each their volume) and get a mixed isotope signal that would in theory then represent the same tissue as when you extracted one twig with bark and leaves using the SPC. But since you do not see any co-extraction of water from the leaves in the SPC samples, I can agree how you would most closely compare them to CVD-TwB. However, that does not mean they are the same tissue type.

Results:

Figure 4: If possible, it would be great if you could change the pink line and text to e.g. grey but that's just a personal preference, I feel the pink is too much, maybe other readers think so too?

Also, it would be reader friendly if you could plot the leaf data as an inset or a separate column and therewith zoom in to the data plotting closer to the LMWL. Especially looking at the Laas data, I think the figure would benefit greatly from a higher resolution on the potentially non-enriched data.

Line 297: I think once you correct for the measurement precision this will not be distinctly different from 0, so I think it is important that you add this S (as standard deviation) according to Landwehr and Coplen 2006.

Discussion:

Figure 8 should go to the last page of the result section.

I would switch sections 5.1 and 5.2 as it is logical to first read your assessment about the differences and therewith the answer to your main question: do these methods yield different results, and then move on with the method of choice discussion in relevance to ecohydrological questions.

Line 333 -336: This could also be related to the contamination by phloem, as you did not peel it from the twig. Please add this information here.

Line 347: again, I think if you would "correct" the $\delta^{13}C$ -excess values by the precision of your measurement device, these values will be indistinguishable from 0 and therewith indistinguishable from the LMWL.

Lines 351-352: please discuss this also in relation to (Chen et al. 2020, Allen and Kirchner 2021)

Concluding remarks: no comments

References:

- Allen S and Kirchner J 2021. Potential effects of cryogenic extraction biases on inferences drawn from xylem water deuterium isotope ratios: case studies using stable isotopes to infer plant water sources. *Hydrol. Earth Syst. Sci. Discuss.*: 1–15.
- Chen Y, Helliker BR, Tang X, Li F, Zhou Y, and Song X 2020. Stem water cryogenic extraction biases estimation in deuterium isotope composition of plant source water. *Proc. Natl. Acad. Sci.* 117: 33345–33350.
- Landwehr JM and Coplen TB 2006. Line-conditioned excess: a new method for characterizing stable hydrogen and oxygen isotope ratios in hydrologic systems. *Int. Conf. Isot. Environ. Stud.*: 132–135.