

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2
<https://doi.org/10.5194/hess-2021-333-RC2>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.



Comment on hess-2021-333

Anonymous Referee #2

Referee comment on "Revealing a significant isotopic offset between plant water and its sources using a global meta-analysis" by Javier de la Casa et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-333-RC2>, 2021

Review of 'revealing a significant isotopic offset between plant water and its sources using a global meta-analysis' by de la Casa et al

This is an interesting study that adds to several other recent meta-analyses on the topic, by exploring the potential role of various biotic and abiotic factors in determining the offset between plant and source (in this case: soil) water stable isotope composition. I recommend that the authors reconsider their title (main contribution). As it stands, revealing the offset is not the main novelty that this study brings. Indeed, the second line of the abstract already states that many studies have reported this, and this has also been demonstrated by recent other meta-analysis studies. This study does include several really interesting novel aspects, that I would recommend the authors put the focus more on, e.g. the outcomes of the specific hypotheses that were tested, the relationships with air T and VWC, combined use of LC-excess and SW-excess (but see comment below). The abstract and conclusion could be slightly edited to highlight the novelty a bit more as well.

My main concern relates to the approach towards the soil water data. Most studies used in the meta-analyses will have taken soil samples across a range of different depths. It is known that at many sites, the water stable isotope composition of soil water changes with depth (and the slope of the soil water lines, which is the basis of the analyses here, does as well); the variation with depth is also often used to determine proportional plant water uptake patterns of different soil depths, and Amin et al 2020 have shown that uptake occurs mainly from the upper part of the soil profile. The authors do not provide information on their approach to dealing with data from different depths, but it appears that they have not taken this into account and have used all soil water data available to derive soil water lines. The extend of the offset between the plant and actual source water could therefore be significantly over- or underestimated. It would be good if the authors provide a clear rationale for their approach and discuss the implications of this, and/or investigate how different the results might be e.g. for SW-excess of shallow and deep soil water as the source. Similarly, it is questionable that the authors currently consider VWC at a range of depths, but not the isotope composition.

Although there is only a small proportion of the studies that does not (only) report soil

water from vacuum distillation, there is a lot of literature that has demonstrated that there are strong differences in the pore spaces that are sampled via vacuum distillation versus lysimeters. Those results also show that the LC-excess of soil water obtained via vacuum distillation can be very different from soil water obtained via suction lysimeters. I agree with the authors that it is beyond the scope of this study to address this here as well, but I would argue that the data of the suction lysimeters should not be included in the analyses here. Alternatively the authors could indicate which studies have used which type of data and explore whether these are outliers/affect the results.

Following on from the comment above, I suggest the authors do not refer to precipitation, stream water and groundwater as the 'mobile' water, as this is often used to describe the freely draining or large pore space (i.e. not tightly-bound) soil water as typically sampled by lysimeters.

Finally, I wonder why the LC-excess for the soil water itself was not also included in the analyses to understand offsets. This could help with the interpretation of the relationship between plant LC-excess and SW-excess, and the role that climate or soil properties may have.

Regarding the abstract:

L 21: make it clear here that you are looking at offsets in plant water.

L 21: make it clear here that LC-excess is calculated based on the LMWL (not the GMWL)

L 30: I did not see direct evidence for the statement that offsets are caused by isotopic heterogeneity within plant stems. I'd leave this and the last sentence out of the abstract and keep it in the discussion as a possible explanation/implication.

Introduction:

L56-59: would be easier to follow if this sentence was split. Also in L 58 change 'that showed' to 'showed that'

L62: seems a bit strange to refer to Poca et al. 2019 as an 'early study'?

L116: it would be good to provide more information on the approach to combine LC-excess with SW-excess.

L146: here is a list of source waters. If I understand correctly though only soil water was directly tested. The other data were not even extracted, so this would be good to clarify here. I understand precipitation was analysed indirectly via LC-excess, but again the data were not used directly in this study.

Methodology:

It is not immediately clear what a 'sampling campaign' corresponds to. Is this a 'sampling occasion' at a specific site and within each plot? The word 'grouped' in line 168 might be why I'm confused, because if you group all dates and plots from a study site, it is unclear why you would have more campaigns than studies. Or does each study report on data from on average 5 study sites? It would be helpful to see the word 'sampling campaign' reappear as a heading in the right place in Table 1.

Soil water content is considered as an effect of climate, however this is also a strong function of the soil properties. Climate might explain the main variations in VSM over time at a specific site, but different soil types under the same climate can have vastly different

absolute values at one moment in time. In this study, VSM is simply a variable across all campaigns and time locally is not considered (e.g. in the way it is plotted in Figure 4b). I'd therefore suggest rewording the role VSM represents (not simply to look at climate effects only).

Results:

Figure 2: the caption seems to be incorrect/unclear. I don't see n reported in the figure. The small numbers on the left-hand side appear to refer to the ID in table 1. This is helpful, but several study sites appear more than once. Therefore, the statement in the first few sentences referring to 'for each study site/sampling site' might be incorrect. I also find the think bars in the figure misleading. They suggest a range rather than a mean value, and a range that either starts or stops at 0. It would be better to have the mean as a coloured marker, also so that the SE is clearly equal distance either side of the mean.

L307-312: this is very difficult to follow. Maybe split up or report the values in a table?

L335-339 and Figure 4: I wonder why only Tair and Soil VWC for the upper 1 m are plotted in the figure? It would be good to see the data for PET as well and equally it would be interesting to see if the slope of the relationship for VWC of different soil depths changes and how?

Table 2: is it correct that 'estimate' refers to the 'slope estimate'? If yes, I suggest to edit the heading as such.

Discussion:

L405-411: this appears to be misplaced as the authors report additional analysis here (would fit better in the methods and results section)

L 407: change 'run' to 'ran'