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Comment on hess-2021-333

Anonymous Referee #1

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-RC1, 2021

Review of de la Casa et al.,

This manuscript presents a straightforward, comprehensive analysis of dual-isotope offsets of plant water from soil water and precipitation. I can imagine this work being of interest and I commend the much hard work and careful thought that went into this manuscript. However, there are major issues that need to first be addressed.

The paper claims that "Globally, stem water was more depleted in 2H than soil water (SWexcess< 0)", but the study finds that "SW-excess was negative in 184 campaigns (out of 642 campaigns)". Thus, SWexcess values below zero are actually globally rare. When we look at Figure 2, which shows +/-1SE, it is apparent (e.g., imagine a doubling of the error bars to represent an approximation of a 95% CI) that very few sites would have mean SWexcess values significantly below zero. The presentation of the discussion, abstract, and conclusions are framed around a claim that is not entirely consistent with the findings. While I realize that the authors are referring to the mean SWexcess of -3.02 +/- 0.65 permil, this is a case where the average is not especially representative of the global behavior, but it is instead driven by outliers (again, see Figure 2). What is the median SWexcess? The authors need to also explain how the "+/- 0.65" was calculated, because it is unclear whether this uncertainty value only reflects the variation among the campaigns; does it also include the error from the calculation itself (eq. 3)? The manuscript needs to include a more nuanced interpretation of the findings, recognizing the wide range in values observed rather than over-relying on the mean value. The closing statement of the abstract, "Our results would imply that plant-source water isotopic offsets may lead to inaccuracies when using the isotopic composition of bulk stem water as a proxy to infer plant water sources" still remains true even if only 184 campaign support it. Perhaps more importantly, the inconsistency and non-ubiquity of negative SW excess values allows the authors to find the climatic effects, which may hint at ways to anticipate or predict plant-source water isotopic offsets.

Lines 30-31: It is speculative to suggest in the abstract that these findings "support the idea that these offsets are caused by isotopic heterogeneity within plant stems". No data were used to directly test this and thus this statement has the potential to mislead readers. I recognize that the authors use their data to argue that they can rule out alternative explanations, and I know that this message is one made and supported in other works by members of this research team, but partially ruling out alternatives does not automatically lead to support for one specific explanation. If the authors choose to keep this line, it should be appropriately framed, for example, as prefaced with a phrase that explains their logic, such as "Because of a lack of alternative explanations, we suggest that these data support". In my opinion, the abstract is more robust without this sentence at all, as the prior sentence is the one that can be more robustly defended.

Lines 124-125: "We expected that in the case where these offsets were the result of methodological artifacts, we would not find any correlation between the magnitude of this offset and environmental or biological variables." I appreciate the authors lay out their forthcoming interpretation, but I do not understand the logic here. There could be methodological artefacts but also biological or environmental controls that are strong enough that they show up despite the influence of methodological artefacts. It also might help to be more specific about which methodological artefacts are being referred to here, because effects of using a mass spec vs a laser spec are directly tested and discussed later. Also, how does this relate to the statement on line 164, "our database did not allow for robust analysis of the potential effects of the water extraction methodology".

Line 141: What would happen if the analysis was restricted to a larger "n" value? The uncertainties in "a-s" and "b-s" must often be very large when n is only equal to 3. Is the number of values that are not significantly different from zero a consequence of this analysis including studies where soils were under sampled? I am not suggesting that the authors change the threshold, but also doing the analysis with a higher threshold might support useful further insights into the meaning of the findings.

Line 147: Because "mobile water" is so frequently used to refer to soil water extracted by suction cup lysimeters in the isotope ecohydrology community, this term should be changed. Given that "mobile" is only used a few times throughout the paper, I suggest simply saying "precipitation, groundwater, and stream water" each time.

Line 176: Were the SWLs and LMWLs calculated by orthogonal least squares fitting? They should be because both the X and Y data have uncertainty, and fitting by standard least squares can artificially reduce slopes, which could have consequences for the findings.

Line 194: What are the criteria for plant and soil sampling to be considered "concurrent"?

Line 201: Is this equation missing a term for the product of sigma-as and delta18O? Also, please check your citations – it refers to a book review in Physics Today rather than the book itself. Another citation issue is on 670.

Line 282: The logic underlying this attribution of these results to "evaporative enrichment affecting stem water" needs to be more clearly explained. Evaporative enrichment could lead to positive or negative SW excess values, depending on the slope of that evaporative enrichment. More generally, the conceptual model and assumptions that are guiding the interpretation of the LC-excess values should be more clearly stated.

Line 304: Is this SE a pooled standard error of the by-campaign SWLs, or is it just of the variation among the individual values.

Line 377-385: As written, this statement is not true because plant water generally (by study or by campaign) did overlap with the corresponding soil water, even if the average value is significantly different from zero. It would be more accurate to say "the isotopic composition of stem water varied substantially in size and direction of deviation from soil water, but on average was slightly lower than soil water". Two sentences later, "sites depicting significantly negative SW-excess… are more ubiquitous" is also problematic because they are not ubiquitous, and "more ubiquitous" is a problematic construction because it means "found everywhere" and thus it cannot be used in a relative sense; use “widespread”. That said, I also think it would be appropriate to cite other studies here (e.g., Chen et al., 2020, Barbeta et al 2020, and probably others) because this manuscript is not the first to suggest that there are isotopic mismatches between soil and water that occur outside of arid or saline environments. One more issue occurs in the following sentence: as written it could be read as indicating that SW-excess was negative in 95% of cases because the parenthetical “95%” should either follow “majority” or “also negative”.

Line 417: I cannot follow the logic captured in the last sentence. How are transport and water exchange likely to be different in cold and wet places? This need to be appropriately laid out and explained rather than quickly inserted at the end. What is the conceptual model?

Line 427: Did drier sites have plants with lower stem water content? This would be a useful relationship to report.

Lines 462-464: I do not understand this sentence.

Discussion of Benettin et al 2018 (https://doi.org/10.5194/hess-22-2881-2018) is peculiarly absent. What may be problematic for the methods used here is that Benettin et al. show a hysteretic pattern that results from the fact that soil water “lines“ reflect a combination of mixing and evaporative fractionation processes. If only part of the annual pattern were sampled (e.g., just a growing season), it would not necessarily be first-order linear, but instead it could be curvilinear. Of course a linear relationship can be fit to curvilinear data, but this could be problematic where extrapolation is involved (e.g., in calculation stem water SWexcess). In this manuscript, cases where the SWL is defined by points with X-axis values that are dissimilar to the stemwater X-axis values, the method used would project the line to the stem water values along a first-order-linear slope. If the X-axis values of stem water and soil water are highly different, the consequences of using a linear versus curvilinear SWL could be large.