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Discussions

Interactive comment on "Isotopic approaches to quantifying root water uptake and redistribution: a review and comparison of methods" *by* Youri Rothfuss and Mathieu Javaux

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General comments

The manuscript by Rothfuss and Javaux on quantifying root water uptake by the means of isotopic approaches aims to provide both an overview of methods and a comparison of the methods with regard to their limitations for the interpretation. They further propose to include modeling approaches to better estimate the root water uptake patterns.

The manuscript is generally well prepared, with mostly sufficient references, in depth information, and proper visualizations. Due to its nature as a review, it is pretty long and I am not sure if the sections 2.1 and 2.2 are really necessary. I agree that it is necessary to understand the soil water isotopic composition in order to interpret the root water uptake with the means of stable isotopes. The authors focus on evaporation fractionation as one process to alter the soil water isotopic composition. From my point of view, also the precipitation input (and its variability in time) would then need to be considered. However, this has been reviewed recently and would blow up the manuscript.

I think that the manuscript is a good contribution to current issues in ecohydrology and will be of interest to a broad readership. Therefore, I suggest a publication after a minor revision.

Specific comments

On page 5, where you introduce into the theoretical backgrounds, I do not think that the isotope depth profiles are solely a result of fractionation effects. It seems that you miss the importance of the variability of the isotopic signal of the precipitation input and its consequences for the spatial variability of the soil water isotopes over depth. As you write, the evaporation fractionation has been reviewed by Horita et al. (2008) and it was more recently reviewed by Soderberg et al. (2012). In order to streamline the manuscript and keep its focus on the root water uptake, I doubt that the section 2.1 and 2.2 are really necessary. However, I agree that it is necessary to account for the spatial and temporal variability of the soil water isotopes, but this is influenced by more than soil evaporation (see also my review Sprenger et al. (2016)).

I am wondering if the authors are aware of the work by Ogle et al. (2004) and Ogle et al. (2014), where they suggest to include biophysical conditions in a process-based mixing model ("root area profile and isotope deconvolution, RAPID"). I think it would be worth including this in the review of methods, since it goes in the same direction as the author's proposal of including more physical basis of the root water uptake estimates.

Please state that the recently published dependency on the carrier gas was found for a WS-CRDS (Picarro); I did not find this CO2 dependency for Off-axis ICOS (Los Gatos) (currently in review).

Technical corrections

P1 L12/13: I suggest using “studies” rather than “authors”

P3 L11: Insert “and” for “in space,” and replace “but also on the root’s” with “and their”

P3 L25: Not sure what you mean with “by reference”

P3 L32: I suggest “distribution of S” instead of “S distribution

P3 L23: Introduce RLD here.

P4 L21: Why not stating directly that Zarebanadkouki et al. (2012) used deuterated water?

P4 L29: I suggest (or artificial enriched/depleted)

P5 L1: Please state here once again what “these methods” will be.

P5 L8: Is this not a bit too simplified at this point? You would always also need some kind of info about vegetation isotopes. I don’t think the first sentence is necessary here.

P5 L9: Are you referring to S profiles or isotope depth profiles? I do not think that the isotope depth profiles are solely a result of fractionation effects.

P5 L 16: Consider splitting this long sentence.

P6 L 30: This line is not black anymore.

P7 L5: I think it would be worth including the definition by Barnes, Allison (1983) for the vapor region to be of total water potential about 15bar, while at lower potential there would be little connected water.

P7 L8: This line is not grey anymore

P7 L15: Is this supported by data? I don’t see it like that in Rothfuss et al. (2015), where you have shown that the slope for the depths above the EF (max -0.06 m) is still clearly below 8.

P7 L16: I have shown that for several studies in Sprenger et al. (2016).

P10 L8: delete second “is”.

P10 L12: I summarized the uncertainty of the different methods to derive soil water isotope data in Sprenger et al. (2015).

P14 L25: I assume this is for d18O? Please clarify.

P14 L24: It would be interesting which rooting depth and density profile was assumed for the modeling. Please provide.

P16 L2: cases

P17 L28: What about the study by Dawson (1993) who provides valumes?

P18 L27: Why do you not include the direct-equilibration method by Wassenaar et al. (2008)?

P19 L7: I did not use in-situ in that study, but consider including a recent paper by Oerter et al. (2016).

P19 L14: Please not that this is the case for a WS-CRDS (Picarro); I did not find this CO2 dependency for Off-axis ICOS (Los Gatos) (currently in review)

P19 L25: I suggest referring to Farquhar et al. (2007). Table 1: I believe there are more studies than the listed ones. I suggest considering the following: Meinzer et al. (1999); Kulmatiski et al. (2010); Kulmatiski, Beard (2013); Evaristo et al. (2016); Goldsmith et al. (2012); Liu et al. (2011); Bertrand et al. (2012); Meißner et al. (2012); Dawson (1996); Bijoor et al. (2012)

Table 2: Why did you limit your analysis here to d18O, while emphasizing that dual isotope approaches would be preferable on page 12 L4?

Figure 1: Caption “negative towards the surface”

Figure 2: Update the caption according to the color of the lines (blue).

Figure 4: Caption “a detail is is presented for”

Figure 5: Why standard deviation and standard error for the different approaches?

Publication bibliography

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