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Discussions

Interactive comment on "Disentangling temporal and population variability in plant root water uptake from stable isotopic analysis: a labeling study" *by* Valentin Couvreur et al.

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The manuscript hess-2019-543 'Disentangling temporal and population variability in plant root water uptake from stable isotopic analysis: a labeling study' by Couvreur et al. present a lab-/field- and model-based study of root water uptake during an artificial tracer experiment, where the soil is wetted from below (as opposed to often, via irrigation). They support their isotope analysis by hydraulic measures in order to provide a holistic understanding of RWU.

The authors address the urgent and contemporary need for increasing the reliability of RWU models and improve the understanding root water uptake patterns. It has been

often proposed to combine hydraulic, water isotope and other information in order to do so, the presented study is in my opinion a holistic and promising approach. The rollercoaster vs. swarm hypothesis is also a good idea, though (as the authors state themselves) it should be validated further. It is also great that both data from an experiment and modeling are provided, rather than only one of the two. This manuscript is well prepared, and the topic is highly relevant. The figures are suitable and well-explained.

I highly recommend this manuscript for HESS, though I have a number of comments/questions that might help to improve this manuscript further. In brief, a few general comments, which are all rather minor:

- The discussion on hydraulic redistribution should be strengthened. Do the authors see a clear sign or not? I think strengthening this part would be of utmost interest for many people from the ecohydrological community.

- When reading the results and discussion section, I realized that there are very small differences discussed in the manuscript (e.g. 0,41 per mill, 1 per mil, etc....). I think it is necessary to think about uncertainties in that respect and really decide which of the differences are likely 'true' differences or simply within the variability/uncertainty.

- I find the discussion of the physical experiment slightly too weak compared to the results drawn from the modeling (I also indicated this in the detailed comments below).

- The authors use δtiller, etc. without providing the water isotope (e.g. δtiller18O). I think this is important to clarify (it was only 18O used, correct?) starting with the symbol description. Why was only oxygen-18 used? (and not 2H in addition?)

- Will the model be made publicly available? It would be very interesting to apply the model with other datasets (e.g. some in situ datasets of joint soil and plant water isotopes) I wish the authors good luck and look forward to the final publication.

Greetings and best wishes, Matthias Beyer

Detailed comments:

- Abstract is very well written

- L.78/79: depends on how deep the groundwater table is. In thick unsaturated zones, often mixing of old water is also a reason. Further, over short time periods a seasonal pattern might persist in the soil

- L.140: in oxygen-18 I guess? Could the authors please add this information?

- L.149: Can the authors please add specifics on the extraction? (Extraction temperature and time for soil and plant samples, how was complete extraction assessed?) The community has been asking in many occasions to provide more transparency of extraction procedures; hence it would be appreciable to add this information.

- L.152/153: the loss of mass would also include evaporation; was this neglected (please clarify) [I see that this is mentioned later in the text, but perhaps better to clarify here]

- L.163: literature

- Chapter 2.4: The explanation and equations make sense to me, but for a detailed evaluation and/or comments on the equations a true modeler might be considered (e.g. M. Cuntz, Wingate/Ogee group)

- Chapter 3.1.1: Figure 2 is mentioned first in the text, then Figure 1....hence, those might be switched Results/Discussion

- -l.243-247 and Fig. 2: There were two soil moisture profiles measured, but only one is shown in Fig.2 (or is that averaged over the two?) I am not sure if that justification that no evaporation was present is sufficient, as the moisture profiles oscillate greatly and over one or two days the effect of evaporation might be minimal (which on the other hand supports the assumption ET=T). Still, evaporation is probably occurring (though at a low rate).

- L.247-249: Yes, but these three options should be discussed by the authors

- L. 250: minimal minimum instead maximal maximum - L. 252: delete level - 3.1.2: Again, if results/discussion is mixed here, those differences and diurnal patterns should be discussed and explained here

- L.269: 'Rayleigh distillation corrections' – this is not explained in the methods. Could the authors provide details on these corrections and/or provide a citation?

- 3.1.3: Well-written and explained

- L.298: yes, but still: 3 per mill is notable for 18O...

- L.325: I am not sure if an 0,9 per mil increase is significant...were replicates taken for each soil depth? What is the std of those (-often this can be in that range already)....if no replicates were taken, this might be well within the uncertainty rather than a true increase

- L.327 depths instead heights - L.345 model instead models - L. 360 upper half instead first half - L.363-365: But couldn't this be implemented to the Bayesian approach via the construction of priors?

Figure 2:

- it's 18O data shown, could this be added to the title (instead of only delta)...OK it's in the figure description,still...

- why is matric potential 'calculated' shown if it was measured?

- Not sure if the inset graphic for the water content is helping the figure

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