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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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Authors responses to Referee Dr. Matthias Beyer

RC| 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:

AC| Dear Matthias, we thank you for the time you spent in carefully revising our manuscript! We hope that we have sufficiently addressed the issues you raised in our revised version.

RC| - 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.

AC| This is indeed an important part of the discussion. In the revised manuscript, we will clarify in section 3.2.2 that all measurable signs of hydraulic redistribution are positive (local increase of soil water content, local enrichment of water isotopic signature) and converge with independent simulated results (water exuded at the same time and location, at a rate compatible with measurements) to yield a robust "yes we think that hydraulic lift was happening at that time at that depth". Our approach will also be strengthened by evaluating the impact of the observational error on our predictions.

RC| - 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.

AC| We will discuss in further detail (please see our answers to your specific comments) the problematic of meaningfulness of our isotope data, i.e., whether these differences of isotopic compositions are the result of given processes or the mere translation of e.g., soil lateral heterogeneity.

RC| - 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).

AC| We will strengthen the discussion of the experimental experiment (see answer to your specific comment to L247-249)

RC| - 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?)

AC| For clarification, we will systematically add "oxygen" before "isotopic composition" throughout the manuscript as well as in the "List of variables with symbols and units" (Page 2).

We only measured the water δ18O with our IRMS ("Isoprep 18 - Optima, Fison, Great-Britain") and not water δ2H and δ18O simultaneously with, e.g., a laser spectrometer for two reasons: (1) to the contrary of laser spectrometers, IRMS are not affected by the presence of volatile organic substances which should be present in the distillated water from soil and plant samples. (2) The added information on δ2H profiles should not be discriminating for determination of RWU profiles as δ2H remains constant in the lower half of the soil profile (mostly contributing to RWU) which is influenced by labeling.

RC| - 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)

AC| We are indeed willing to make the code open source, as it may be useful to the scientific community working on such data. We will upload it as soon as the MS is accepted.

RC| I wish the authors good luck and look forward to the final publication. Greetings and best wishes, Matthias Beyer

Detailed comments:

RC| - Abstract is very well written

AC| Thank you :)

RC| - 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

AC| We agree! If soil water (and eventually groundwater) is replenished by rain events of which the isotopic compositions is highly dynamic in time, it can generally lead to issues of identifiability. This will be added in the revised version.

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

AC| Consider it done!

RC| - 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.

AC| Water from plant (i.e., tillers and leaves) and soil samples was extracted by vacuum distillation (applied vacuum: 10–3 mbar) at temperatures of 60 and 90°C, respectively.

In addition, complete extraction was assessed based on the comparison of sample weight loss during distillation and mass of collected distillated water. This information will be added in the revised version of the manuscript.

RC| - 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]

AC| Yes, thank you. It will be clarified, i.e. "transpiration (m d–1)" will be replaced by "evapotranspiration rate loss (in m d–1)" in the revised text.

RC| - 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)

AC| This is true! We have already received a comprehensive review from referee #1 on the modeling aspects of our work which we hope to have properly addressed in our answer.

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

AC| We make reference to Fig. 1 at in Chapter "2.4 Modeling of RWU and δtiller"), thus before citing Fig. 2 (Chapter 3.1.1).

RC| - 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).

AC| There was 1 profile taken per sampling time, thus four profiles are shown in Figure 2: DaS 166 - 15:45 (orange line), DaS 167 - 07:00 (blue), DaS 167 - 15:45 (red), and

DaS 168 - 05:00 (black) We agree with the reviewer that evaporation could have been partly the reason of the observed differences in water content at the soil surface across sampling times, the other reason being the lateral heterogeneity. We can only make the assumption that evapotranspiration = transpiration, assumption that we carefully mention, based also on the high computed value for soil water surface tension.

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

AC| We will strengthen the discussion at carefully discuss these options, thank you.

RC| - L. 250: minimal minimum instead maximal maximum

AC| Thank you. It will be done!

RC| - L. 252: delete level

AC| We propose to replace "level" by "value". Thank you.

RC| - 3.1.2: Again, if results/discussion is mixed here, those differences and diurnal patterns should be discussed and explained here

AC| We agree that section 3.1.2 (as well as 3.1.1) stays rather descriptive. It is the case because we choose to discuss both soil and plant isotopic data in section 3.1.3 by cross-comparing them with soil and plant hydraulic data.

RC| - 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?

AC| We will add two references to these corrections and how they should be applied: "Galewsky, J., Steen-Larsen, H. C., Field, R. D., Worden, J., Risi, C., and Schneider, M.: Stable isotopes in atmospheric water vapor and applications to the hydrologic cycle, Rev. Geophys., 54, 809-865, doi:10.1002/2015rg000512, 2016." "Dansgaard, W.: Stable Isotopes in Precipitation, Tellus, 16, 436-468, doi:10.1111/j.2153-3490.1964.tb00181.x, 1964."

RC| - 3.1.3: Well-written and explained

AC| Thank you!

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

AC| Indeed, we agree with this comment. We will mention that a difference of 2.9 ‰ between simulated and measured mean δtiller is notable, though relatively small compared to the datasets standard deviations (8.4 ‰ and to the isotopic ratio of the labelled water (464 ‰ non-labelled soil water isotopic ratio between –7.4 ‰ and 1.3 ‰. Statistically we could not systematically conclude that simulated and measured δtiller differed. By drawing randomly simulated δtiller in 3 plants at each time step (as in the measurements), comparing the overall distributions of measured and simulated pooled δtiller with an ANOVA analysis, and repeating the random drawings for all 40 observation times 100 times, measured and simulated δtiller distributions were not statistically different in 92% of drawings (P>0.01). We will reformulate the sentence as: "The predicted versus observed δtiller distributions for the overall dataset differed noticeably but not significantly (6.6 $\pm$ 8.4 ‰ and 3.7 $\pm$ 8.4 ‰ respectively) when pooling 3 simulated δtiller randomly at each observation time, as in measurements (P>0.01 in 92 cases out of 100 repeated drawings)"

RC| - 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

AC| The observed δsoil at the first three observation times are -7.17 ‰ -7.00 ‰ and -7.21 ‰Ẇe confirm that it differs from -6.2 ‰ with an ANOVA analysis (P<0.01). The p-value will be provided in the revised version of the manuscript

RC| - L.327 depths instead heights

AC| It will be done. Thank you!

RC| - L.345 model instead models

AC| It will be done. Thank you!

RC| - L. 360 upper half instead first half

AC| It will be done. Thank you!

RC| - L.363-365: But couldn't this be implemented to the Bayesian approach via the construction of priors?

AC| This is a very keen remark. It is true that we decided to go for a "flat Dirichlet a priori rRWU distribution (i.e., rRWUJ=1/10)" and we were missing an explanation on why we did not implement the construction of priors. The outcome of the statistical model may indeed significantly depend on the definition of the a priori relative RWU profile. In the present study, we set it to follow a "flat" distribution (i.e., rRWUJ = 1/10, see Appendix E), in other word, each layer was initially defined to contribute equally to RWU. To the contrary of other studies (e.g., Mahindawansha et al., 2018), where the a priori rRWU profile was empirically constructed on basis of soil water content and root length density profiles, we decided not to further arbitrarily constrain the Bayesian model for the sake of comparison with the physically-based soil-root model.. This will be added in the revised version of our manuscript.

RC| Figure 2: RC| - it's 18O data shown, could this be added to the title (instead of only delta)...OK it's in the figure description, still...

AC| We hope mention of "oxygen" in the title and now repeatedly throughout the manuscript will clarified this.

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

AC| ψsoil was calculated on basis of θ data, and not directly measured. We propose to clarify this confusion by moving the mention of soil matric potential to section 2.2. In addition, we will add "Measured" in Fig. 2's caption.

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

AC| This is true now that you mention it! The inset will be removed from Fig. 2 in the revised version.

Mahindawansha, A., Orlowski, N., Kraft, P., Rothfuss, Y., Racela, H., and Breuer, L.: Quantification of plant water uptake by water stable isotopes in rice paddy systems, Plant Soil, doi:10.1007/s11104-018-3693-7, 2018.

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