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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Long-term and high frequency non-destructive monitoring of water stable isotope profiles in an evaporating soil column" *by* Y. Rothfuss et al.

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Dear Referee #1, thanks a lot for your review of our manuscript. You will find our answers to your general and specific comments below.

1) While the first half of the manuscript is well-structured, the second half is not. I would strongly recommend combining Results and Discussion sections or providing a clear separation between these sections with regard to contents. A major part of the actual results (including several figures) is introduced in the Discussion section, partially in fact without much discussion, context or explanation. This is not what a reader of this

article would expect.

ANSWER: the authors feel that to the contrary, figures presented in the Discussion section are not straightforward representations of experimental results but rather either statistical / analytical modeling results (i.e. δ2H-δ18O relationships / δS gradients dynamics). This is why the authors would like to keep Results / Discussion as separate sections.

2) The readability of the manuscript would clearly benefit from revising for sentence structure and grammar (e.g., avoid frequent use of very long sentences, use proper punctuation).

ANSWER: effort has been made in the revised text to reformulate and significantly shorten sentences containing more than 30 words.

3) It is not clear how the method described in the paper is new (Abstract P3894LL8; Conclusions P3906LL19). In the manuscript text (e.g. P3896), it is stated that a previously developed method is applied directly. As such, the declaration of methodological novelty seems inappropriate.

ANSWER: the present study is the first application of the method of Rothfuss et al. (2013) which provided the calibration coefficients for 2H and 18O and (Eq. 1 and 2) solely. This study is also the very first long-term application of the series of newly developed similar monitoring systems based on gas-permeable membranes (i.e., Herbstritt et al., 2012; Volkmann and Weiler, 2014). This has been clearly stated in the revised text (i.e., in Abstract, Material and Methods and in Conclusion).

4) It is not entirely clear how to interpret the ambient/atmospheric air measurements. Apparently, one measurement location was maintained in a room with a small soil column while an AC circulated air with unknown isotopic composition. It seems that the influence of air supplied by the AC on the vapor mixture that is assessed here would likely vary over time, but this issue does not become very clear. For exam-

ple, P3904LL12/P3905LL2, why was the deuterium abundance that high and why was there no dO-dD correlation then? Temperature is implicitly suggested as a relevant covariate, but no clear explanation is provided. It should generally be explained what the informative value and validity of atmospheric data collected during the experiment is.

ANSWER: through the significant statistical link between air temperature and atmosphere isotope compositions, it is inferred in the text that the laboratory air moisture partly originated from outside air moisture. However, as air temperature seasonal dynamics could not alone explain that of the atmosphere isotope compositions, it is also concluded that laboratory air moisture was impacted by column water evaporation. The fact that the atmospheric δ2H values measured during the experimental period DoE 125-155 were remarkably high is directly related to the high soil δ2Hliq values during that same period (see Figure 4d). The reviewer is right, this need to be explicitly written and will be in §4.2 Please also note that the AC system did not bring outside air into the laboratory whereas it cooled (without condensation) and re-circulated the laboratory air by the set of axial fans. This has been better explained in the revised text.

5) Throughout section 4.2 (PP3904LL7), significant results, existing and non-existing correlations are reported and further used to support conclusions without suitable statistical inference. I do not believe the conclusions are generally unjustified, but the statistical assessment and report needs to be improved. E.g., I would expect confidence intervals on slope estimates, and appropriate tests on difference between slopes.

ANSWER: F-statistics p-values have been given along with R-squared in the revised text. Note that p-values are almost exclusively lower than 0.001 apart from some regressions having $p < 0.01$.

Further (P3904L18), a (presumably) OLS regression using data from all soil depths (with expectedly differing slope) does not seem appropriate (regarding residual struc-

ture) or useful (a consistent slope would not be expected).

ANSWER: The authors agree with this remark. However this is typically what an experimentalist would do to check the extent of bare soil evaporation: plot δ18O against δ2H for entire soil profiles. The vertical resolution of our method was high enough to allow visualizing these significantly different δ2Hliq-δ18Oliq LRS in 01-03-05 cm depth. And this is why the authors finally differentiate these surface data from the rest of the soil profiles. Note that p-values of all estimated regression parameters (slope/y-intercept) are lower than 0.001.

Also, R-squared is the coefficient of determination (not correlation; e.g. Fig. 6). Finally, negative coefficients of determination cannot be found (P3905L4) and negative slopes do not point to lack of linear relation.

ANSWER: Thank you for these corrections. There was indeed a typographical error: only δ2Ha-δ18Oa regression slope is negative whereas R-squared equals 0.26 (p-value < 0.001). This has been corrected in the revised text along with “determination” instead of “correlation” in Fig. 6.

6) It is not clear if δSvap is calibrated to the reference scale. The contrary seems to be the case from p3899. In that case the values would, however, not be comparable (e.g. P3901LL15) with equilibrium vapor compositions of a known liquid water on the reference scale.

ANSWER: indeed, this was not clear in the text. This following sentence: “δa and δS values were finally corrected for laser instrument drift with time, using the isotope compositions of the two water standards, δst1 and δst2.” has been reformulated as: “δa, δSvap and δS values were finally corrected for laser instrument drift with time, using the isotope compositions of the two water standards, δst1 and δst2.”

Minor comments: P3896L18: "Average" should be plural and also more specific, presumably "Mean values".

ANSWER: thank you for this proposition. This has been adopted in the revised version.

P3897L20: Consider using, e.g., δSliq rather than δS. The use of δS and δSvap is somewhat confusing. Also, the latter seems to refer to partially corrected values, this could be made clearer.

ANSWER: δS has been replaced with δSliq throughout the manuscript in the revised text, the equations, and the figure captions.

P3899L7: Are the SDs mean or maximum values across samples?

ANSWER: the reported SDs are the maximal accepted standard deviations values. This has been specified in the revised text as such: "Measurements that did not fulfil the above mentioned conditions for δ2H and δ18O standard deviations were not taken into account"

P3899LL26: It is not clear which experimental measure was used here to achieve the declared goal.

ANSWER: the method with which water was applied to the sand surface has been further described by adding the following sentence: "Opening/closing the valve controlled the flow rate at which air entered the bottle headspace, which in turn controlled the irrigation flow rate."

PP3904LL12: A brief comment on some of the underlying concept (e.g. kinetics) leading to the stated conclusion could be helpful for interested non-expert readers.

ANSWER: Concepts on isotopic kinetic effects has been given in the revised text. The last sentence of 4.2 2nd §, i.e., "Therefore, it can be deduced that the laboratory air moisture was partly resulting from column evaporation." has been replaced with: "δ2H-δ18O regression slope typically lower than eight indicate that a water body has been affected by (non-equilibrium) evaporation process. A good approximation for the slope of the 'evaporation line' given by Gat (1971) (Eq. (3), based on the Craig and Gordon (1965) model) depends in addition on the aerodynamic conditions at the liquid/air interface, i.e., relative humidity and the so-called kinetic fractionation factor (Merlivat, 1978 ; Cappa et al., 2003; Luz et al., 2009). Therefore, it can be deduced that the laboratory air moisture was partly resulting from column evaporation."

P3904LL13: The reference used for inference of equilibration fractionation factors should be given.

ANSWER: the reference (Majoube, 1971) has been added to the revised version: "Majoube, M.: Oxygen-18 and Deuterium Fractionation between Water and Steam, J. Chim. Phys. Phys.-Chim. Biol., 68, 1423-1436, 1971."

Appendix to these corrections: ABSTRACT: The second objective was missing in the initial submission. The following sentence has been added at the end of the abstract of the revised version: "Finally, from simple soil isotope gradients calculations, we showed that the gathered data allowed to determinate the depth at which evaporation proceeded in the soil and how this evaporation front receded into the soil with time."

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