

# ***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.***

## **Anonymous Referee #1**

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This manuscript describes the application of a non-destructive technique for measuring the stable isotopic composition of soil water and water vapor in a laboratory soil column subject to evaporation over an extended time period. The long-term applicability of the measurement approach is thereby demonstrated and insights on the isotopic evolution during evaporation from soil and interactions with ambient vapor are presented. The experimental approach is generally sound; however, the reporting of the research could be improved (see below). As such, I believe this paper can present an interesting piece of work that will be of relevance to the readership of Hydrology and Earth System Sciences, but also recommend that some aspects are revised.

This said, I kindly ask the authors to consider the following comments.

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.

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).

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.

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 example, 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.

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 sta-

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tistical assessment and report needs to be improved. E.g., I would expect confidence intervals on slope estimates, and appropriate tests on difference between slopes. Further (P3904L18), a (presumably) OLS regression using data from all soil depths (with expectedly differing slope) does not seem appropriate (regarding residual structure) or useful (a consistent slope would not be expected). 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.

6) It is not clear if  $\delta S_{\text{vap}}$  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.

Minor comments:

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

P3897L20: Consider using, e.g.,  $\delta S_{\text{liq}}$  rather than  $\delta S$ . The use of  $\delta S$  and  $\delta S_{\text{vap}}$  is somewhat confusing. Also, the latter seems to refer to partially corrected values, this could be made clearer.

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

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

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.

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

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