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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 #2

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The paper presents continuous time series of temperature, soil moisture and its isotopic composition in several depth of an evaporating soil column in the laboratory. This is a really nice data set. The build-up seems well done, the observational chain is sensible and the presented data seems to be of high quality.

The paper has two flaws in my opinion: 1. there is almost no discussion, only results and 2. it is overselling evidence for prove.

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Regarding point 1: This is such nice data so I am really missing the discussion. For example, it is reported that the upper soil layers exhibit different lines in d18O-dD space. But no explanation given why. There is already no meteoric water lines in the plots to compare.

What would you expect if you think about evaporation lines?

Gat has shown that the observed evaporation line is a combination of several evaporation lines at different temperatures. Could you do something similar?

I think that figure 7 does not add anything to the manuscript.

Another point of discussion could be the value of the kinetic fractionation factor. One has E and VPD so one gets the conductance. One has also theta at the soil surface and the atmospheric conditions. So you can estimate the enrichment at the evaporating front and hence can deduce the equilibrium fractionation that fits best. Are Mathieu and Bariac right, what seems to be suggested by recent studies? Or is it completely wrong because it neglects the resistance in the soil for the evaporation?

I guess evaporation E was deduced from the change of the soil moisture profile. This was not explained in the manuscript. So if you can do that, can you then also get the isotopic composition of the evaporation from the change in the isotope profile?

And if so, what for do I need the evaporating front?

I was also wondering if you can deduce soil moisture from the humidity in the tubes? I could not find a word about it in Rothfuss et al. (2013) but some sentence in this manuscript made me think that it should be possible.

Regarding point 2: The study is an application of the method developed earlier and presented in Rothfuss et al. (2013). But the abstract states: "In this study, we present a new non-destructive method ..."

The enrichment at the evaporating front is well known, often observed and explained at least since Barnes and Allison (1984). But the abstract states: "we could also show for the first time the increasing influence of the isotopically depleted ambient water vapour on the isotopically enriched liquid water close to the soil surface (i.e., atmospheric invasion)."

The same in the conclusions. It is stated, for example, "it could capture sudden variations following a simulated intense rain event." It is true that the method reacted to the watering. No attempt was done, though, to quantify if the observed change in isotopes fits to the isotopic water balance.

Also "followed quantitatively the progressive isotope enrichment" was not proven. The authors took the calibration done in another medium; at least it sounded different to the sand in Rothfuss et al. (2013). I have always wondered about the physics behind the calibration curve. For example, why is the observed offset not the equilibrium fractionation at 0 degC? So to me it was not proven that the same calibration curve is valid for different soils. There could be a dependency of the soil, which was also hinted in section 4.1.

I have a few other questions and comments: The method for determining the evaporating front is very flawed. I would have at least fitted some kind of spline to determine the front. Otherwise you are limited to the resolution of your measurements.

But in any case, the evaporating front does not have to be the maximum. This depends on the atmospheric value. It can be even seen in the measurements presented in deuterium on DoE 100-150: The profile is pretty vertical in the upper soil while the front is already a few cm down the soil. It is not the maximum of the curve but rather the point of a discontinuity in the first derivative that indicates the evaporating front.

Why is there no decrease in water content in the lowest layer. It seems like standing at

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0.3x and not moving for 300 days. This is impossible with a porous glass plate at the bottom. And quite contrary to Merz et al. (2014). Did you really have a porous plate on the bottom? If not, this is the reason why you do not see stage I evaporation.

So there in an argument in section 4.3 that the resistance increases with increased wind speed and this is why there is no stage I evaporation. This is unphysical. The exact opposite is true and also measured very often, even in the same institute.

The discussion about the atmospheric water vapour is strange. What does it mean: "These values were significantly lower than the calculated ratio of the equilibrium fractionation for 1H2H16O and 1H218O that characterises meteoric water bodies, which should have ranged between 8.32 (DoE 200–250) and 8.47 (DoE 0–50) at the measured laboratory air temperatures." Atmospheric air is long-range transport. It has nothing to do with "the measured laboratory air temperatures". Why does it has to be in equilibrium with some meteoric water bodies? What are these bodies? Did you check the weather patterns? Could have been air from the East instead from the West as normal.

Last but not least, the manuscript uses very often 'isotope' where it should be the adjective 'isotopic' such as in isotopic composition.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 3893, 2015.