

Interactive comment on “Deep soil water ^{18}O and ^2H measurements preserve long term evaporation rates on China’s Loess Plateau” by Wei Xiang et al.

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Response to Anonymous Referee #2

We sincerely appreciate the Anonymous Referee #2 for your detailed, constructive, and very thoughtful comments on our manuscript. Our point-by-point responses are as follows.

1. The main issue of the presented study is the lack of information of the isotope ratios in the infiltrating rainfall at the individual sites. While it is unclear how the IAEA rainfall data were used in the study, the uncertainty introduced by not having site-specific rainfall isotope data are not assessed and thus not accounted for. If the actual rainfall isotope ratio differs from the IAEA data derived local meteoric water line, the

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lc-excess for the soil water ends up being offset over the entire depth profile. In such case, one would assume negative lc-excess results from transport of partly evaporated shallow soil water into deeper depths. For example, if the intercept of the LMWL is overestimated, the lc-excess values of the soil water will be underestimated. If the slope of the LMWL will be underestimated, then lc-excess values of deeper soil water might be underestimated. I now see that this aspect has been already addressed in the response to Reviewer 1. If I use some numbers provided in Fig. 2, assuming there is $d2H = -68$ permill and $d18O = -9.2$ permill in the subsurface (from reading the graph), the lc-excess could range between -4 and 9 permill depending on the assumption of slope being 6.11 or 7.56. Such uncertainty is not discussed yet in the manuscript.

Response: To address your concerns, we will add site-specific rainfall isotope data for six of our soil sites that are available. Indeed, the lc-excess method presented in this study requires the site-specific LMWL parameters (i.e. slope and intercept) for each soil sampling site, and it should be more accurately if the site-specific LMWL is used. However, for the method to be widely applicable, a method should not require data that are not widely available. Because LMWL may not be available at each site of interests, we constructed the regional LMWL based on IAEA data from five stations, and use it for all of our sites for evaporation calculation. If the site-specific LMWL is much different from the regional LMWL, this, as you stated, would introduce uncertainty.

To asses this uncertainty, we obtained the site-specific LMWL for six of our soil sites, and compared the evaporation estimates obtained from regional LMWL and site-specific LMWL. Figure 1 showed that the difference between the two methods are very small, with the difference ranging from -3.2% to -2.1%. This suggests that the regional LMWL may be acceptable if site-specific LMWL for a individual soil site is not available. We will add this important part in the method, result, and discussion sections of the revised version.

2. Further, according to Figure 4, the lc-excess at several sites is within the interquartile range of the rainfall lc-excess. Is rainfall lc-excess related to rainfall intensity? More

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intense rainfall would probably reach deep soils more likely than less intense rainfall.

Response: Thank you for your comments. Large rainfall events are more likely to be “lighter”, while small rainfall events tend to be heavier. Therefore, both $\delta^{18}\text{O}$ and $\delta^2\text{H}$ are negatively correlated to rainfall intensity. However, in the dual space, lc-excess or d-excess is not related to rainfall intensity. No matter what the rainfall intensity is, rainfall lc-excess, by definition, is zero. Notwithstanding individual rainfall events may have nonzero lc-excess values due to variations in moisture sources, air mass trajectories, and cloud processes (Dansgaard, 1964; Allen et al., 2018), such variations are very small compared to $\delta^{18}\text{O}$ or $\delta^2\text{H}$, and are also unimportant because they will be damped as waters from those events are mixed together in land scapes (Allen et al., 2018). As you pointed out, large rainfall events generally infiltrate deeper in the soil profile. Additionally, they also infiltrate faster, having less chance for evaporation fractionation. But during the infiltration processes, the “event” or “new” water mixes with pre-vent or the soil water that experienced evaporation. Consequently, the infiltrating water carries the evaporation signal (more negative lc-excess) into deep soil. This is also consistent with the “piston” flow model for water and chemical transport in soil. Therefore, the deep soil water would have the evaporation signal and lc-excess of deep soil can be used to calculate evaporation ratio.

Though there is mounting evidence that soil water flow on the China’s Loess Plateau is dominated by piston flow (Lin et al. 2006; Xiang et al. 2019; Huang et al. 2017). When preferential flow is dominant flow mechanism, the assumption that deep soil lc-excess keeps the evaporation signal, should be tested vigorously. Currently, we are using simulations to test this hypothesis and others, which is beyond the scope of this study.

Dansgaard, W.: Stable isotopes in precipitation, *Tellus*, 16, 436–468, 1964.

Allen, S. T., Kirchner, J. W., and Goldsmith, G. R.: Predicting Spatial Patterns in Precipitation Isotope ($\delta^2\text{H}$ and $\delta^{18}\text{O}$) Seasonality Using Sinusoidal Isoscapes, *Geophys*

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Res Lett, 45, 4859-4868, 2018. DOI:10.1029/2018gl077458

Lin, R. F. and Wei, K. Q.: Tritium profiles of pore water in the Chinese loess unsaturated zone: Implications for estimation of groundwater recharge, *J Hydrol*, 328, 192-199, 2006. DOI:10.1016/j.jhydrol.2005.12.010

Xiang, W., Si, B. C., Biswas, A., and Li, Z.: Quantifying dual recharge mechanisms in deep unsaturated zone of Chinese Loess Plateau using stable isotopes, *Geoderma*, 337, 773-781, 2019. DOI:10.1016/j.geoderma.2018.10.006

Huang, T. M., Pang, Z. H., Liu, J. L., Ma, J. Z., and Gates, J.: Groundwater recharge mechanism in an integrated tableland of the Loess Plateau, northern China: insights from environmental tracers, *Hydrogeol J*, 25, 2049-2065, 2017. DOI:10.1007/s10040-017-1599-8

3. Any information about soil moisture is missing. Ratios between infiltrated non-fractionated rain water and fractionated soil water storage are discussed, but remain speculation when no mixing volumes are considered. For now, the authors cannot really provide a process description of how fractionated shallow soil water is transported - without or with only little mixing with rainfall water - to deep depths. Without a description of a hydrological process based on observed data leading to $I_c\text{-excess} < 0$ in the subsurface, the interpretation remains speculation.

Response: Thanks. This is a very good question. From a large body of research on the Loess plateau, the piston flow is the dominant transport mechanism in soil. As we stated above, we are using simulations to test our assumption and obtain the process understanding.

Specific comments:

(1) 25: “f” needs to be clearly defined. Currently not given.

Response: Thanks. We revised as “Water loss from soil by evaporation is an essential component of the terrestrial water cycle (Good et al., 2015), and its ratio to the total

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precipitation (evaporation/precipitation, f) affects how efficiently precipitation water can be used for ecosystems (Good et al., 2017) because it is often considered as the undesirable component (Kool et al., 2014).”

(2) 39: typo: field

Response: Revised.

(3) 85: runoff, as surface OR subsurface runoff to streams (no rain reaches the stream) or do you mean “surface runoff”?

Response: Thanks. We revised as “surface runoff”.

(4) 93: provide temperature, vacuum, and time applied.

Response: Thanks. We added the missing information as “Soil water was extracted via the cryogenic vacuum distillation method (Li-2000, LICA, China; less than 0.2 Pa for system pressure, 95°C for heating temperature, and more than three hours for heating time)”.

(5) 110: It’s not clear which of the precipitation sampling stations has been used for constructing the LMWL of which soil sampling site. Please clarify. What are the time periods of the precipitation sampling? Did this overlap with the soil sampling? What was the sampling frequency of the precipitation sampling? Please provide the LMWL for each study site or was one LMWL used for all the sites?

Response: Thanks. We have responded in the comment 1 and will be added in the revised manuscript.

(6) 139: What “annual values” are meant?

Response: Thanks. We revised as “We estimated the α^* and ϵ_k using the long-term (1981-2018) daily temperature and relative humidity. The minimum and maximum temperature were used to consider the uncertainty associated with the conditions under which evaporation occurs (Allen et al., 2019); Consequently, minimum and max-

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imum f were calculated, and we reported the mean and standard deviation values.”

(7) 186: You miss showing the decrease in evaporation fractionation for your study sites. I suggest to replace the plot of $d18O$ with a plot of l_c -excess in Figure 2.

Response: Done.

(8) 188: What does this mean? Thick soil depths prevent surface soil evaporation? Please rephrase to clarify.

Response: Thanks. We revised as “However, the isotope fractionation signatures in deep soil water is not result of direct surface evaporation, because the evaporation front is generally above the depth of 2 m and thus deep soil water is not under the influence of evaporation.”

(9) 190: In the review by Sprenger et al. (2016) that you refer to, they also presented modeling results assuming no preferential flow and even under such conditions, the evaporation fractionation signal was gone with depth because of the “subsequent mixing of the evaporated soil water with nonfractionated precipitation”.

Response: Thanks. If one uses single isotope, “subsequent mixing of the evaporated soil water with nonfractionated precipitation” makes the signal disappear” can occur practically. But if one use l_c -excess, we think the evaporation signal should not disappear as a result of mixing. As we stated above, we are in the process of using simulation study to obtain a process understanding, which is beyond the scope of this study. In the manuscript, we added this as a limitation.

(10) 199: This interpretation remains speculation since no information about fractionated soil water storage and mixing with non-fractionated rainfall are shown in the results.

Response: Thanks. We add that as a limitation of our study.

(11) 224: Unclear how a ratio can have a residence time and how this residence was

derived.

Response: Thanks. We revised as “Assuming a piston flow mode, the mean pore water velocities (θv) are estimated as 11 cm yr⁻¹ ($\theta v = ((6.1 \cdot 100) / ((2015 - 1963)))$) and 19 cm yr⁻¹ ($\theta v = ((9.8 \cdot 100) / ((2015 - 1963)))$) at S11 and S7, respectively. Therefore, precipitation water infiltrated to 2 m and 10 m below of the soil surface needs a long time, about 11-17 years and 53-85 years, respectively. Under such scenario, soils between 2 m and 10 m comprised of water with the mean age ranged between 14 years and 69 years, and its isotopic compositions preserve the evaporation effects over this period. Thus, the f obtained from the average deep soil water isotopic compositions had a mean residence time of about 28 years.”

(12) 229: What do you mean of evaporation having long residence time? Do you mean the time water resides in the soil until evaporated or transpired?

Response: Thanks. as we stated in the response (12), we mean the age of the soil water (the time since the rainfall enters soil to the time at which we took a deep soil core and measured soil water).

(13) 261: Extending the data set for northern sites, Sprenger et al. (2018, doi:10.1002/hyp.13135, 2018) found similar to your results that rainfall amount could explain most of the lc-excess dynamics, and not ET.

Response: Thanks. We will add that in the discussion.

(14) 281: typo: 180

Response: Thanks. We have revised.

(15) 285: You miss to discuss the uncertainty introduced due to lack of LMWL at the specific site.

Response: Thanks. We will add that in the revised manuscript as we stated in our response to your comment 1.

(16) 473: Please add when was the “Deep profile” taken. Figure 2: Consider replacing d18O in sub plot (b) with lc-excess, since you refer to evaporation fractionation. Having lc-excess plotted over depth would underline your discussion, while showing very similar d2H and d18O profiles does not provide that much information.

Response: Thanks. Done as suggested.

(17) Figure 3: Since lc-excess is such an important measure in your study and you claim that the lc-excess changes over depth, it would be very valuable to see the lc-excess on a second axis in the plots shown in Figure 3.

Response: Thanks. Done as suggested.

(18) Figure 4: How about sorting the boxplot by e.g., total rainfall? (Figure 5 shows that there is a relationship there).

Response: Thanks. Done as suggested.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2019-679>, 2020.

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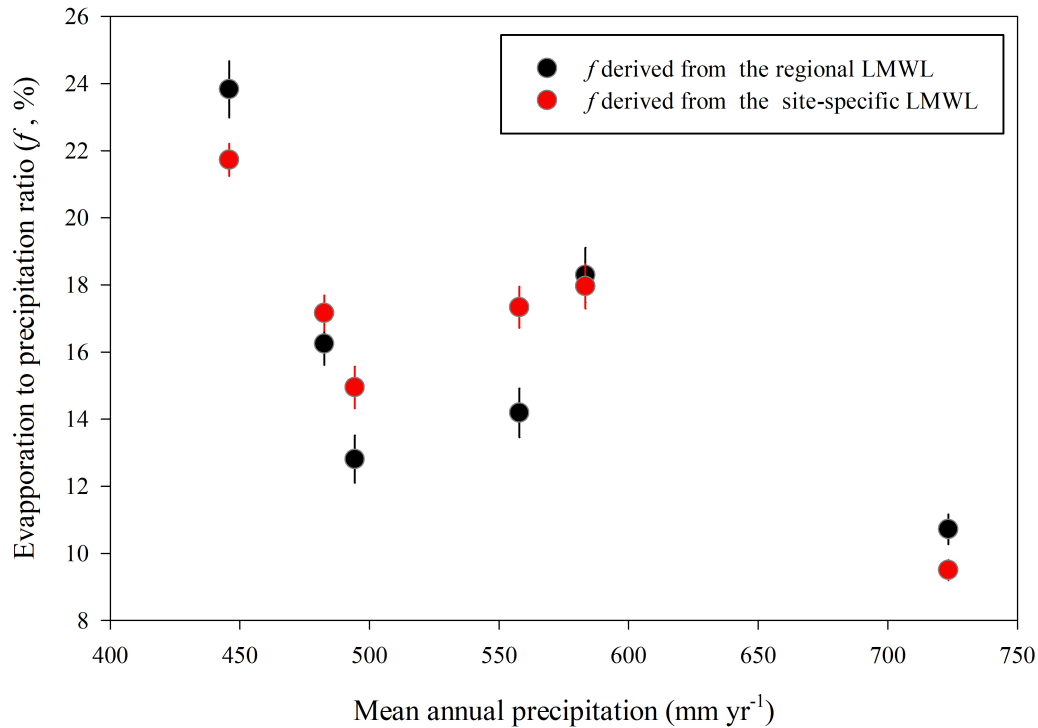


Fig. 1. Comparison of evaporation estimates derived from the regional LMWL and the site-specific LMWL.

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