

Replies to reviewer 2 acp-2013-741-discussions

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We would like to thank reviewer 2 for her/his constructive comments on the manuscript that helped us to improve our paper.

The reviewer's comments are repeated in normal case, our replies are given in italics. Specific comments line numbers are given for the online version of the manuscript. We address each comment point by point (see below).

1 General comments

The goal of partitioning terrestrial ET into components is important and challenging, especially at the landscape to regional scales. Unfortunately, the method relies on several assumptions, which are unlikely to be true, is limited in application, and is very sensitive to the non-equilibrium kinetic fractionation factors used for soil and ocean evaporation. The authors need to address these limitations and uncertainty in their revisions.

We agree that the approach we present in section 7 of our paper is a first attempt and feasibility study on a new methodology that may be useful to estimate the transpiration fraction of continental evaporation. We would like to stress the fact that this is only one aspect of our study and that our main aim in this paper is to better understand the mechanisms behind the variations of the continental deuterium excess signal at the daily timescale.

We added explanations on the assumptions and limitations of our approach to estimate the transpiration fraction using the $d - h_s$ relation:

- p.29747 l. 3: *"The theoretical slope of the $d-h_s$ relation of continental evaporation (Fig. 11a) can be computed using the Craig and Gordon (1965) model under the following assumptions:*

- 1. The only source for boundary layer water vapour is surface evaporation (closure assumption, see Appendix A for more details, Merlivat and Jouzel, 1979) A sensitivity study on how the closure assumption is applied (on each evaporation flux separately or globally) is shown in Appendix C. Furthermore, as mentioned in Sect. 1, the $d-h_s$ slope may be overestimated when applying the closure assumption (Jouzel and Koster, 1996). This implies that we may in turn slightly underestimate the transpiration fraction with our approach.*
- 2. Transpiration is assumed to directly transmit the signature of the soil moisture, which is assumed to be constant over the 5 day time period of an HRA or HLA event.*
- 3. h_s and R_c represent average conditions at the moisture source for each time step.*

- *In Appendix C we added a sensitivity study on the application of the closure assumption and on the dependency of the plant transpiration fraction estimates on the choice of the non-equilibrium fractionation factors.*

1. *I agree with the other reviewer and the author response that the different closure assumptions should be addressed in Eqn 3. Do they matter? I feel like global closure is more likely to reflect the real world continental environment. But they are also quite flawed assumptions based on what we know about the variability of vapor dex in the continental boundary layer. Humidity is likely different on the ocean and continental evaporating source regions. Atm vapor dex is almost never identical to transpiring or soil evaporating water. This is a very thorny problem.*

We now use the closure assumption over all the evaporation sources in the main part of the paper. The impact of the type of closure assumption is discussed in Appendix C of the paper. The effect

of using an average h_s over all moisture sources is difficult to estimate and will be investigated in a future study.

2. This analysis ignores entrainment, which likely has a strong dex-h relationship and the influence can vary greatly from day-to-day (Welp et al. 2012).

In the case of the HRA events the mentioned source of $d - h_s$ covariation should not affect our analysis, since moisture source points are identified where air parcels take up humidity. Strong entrainment rather leads to a drying of the boundary layer. However, for HLA events this could be a plausible interference factor. This effect would have to be studied in more detail. It is clear that entrainment of free tropospheric air affects the daily cycle of water vapour isotopes in the boundary layer. We changed the text as follows on p. 29749 l. 4:

“Entrainment of free tropospheric air may be a further confounding factor, by bringing dry air with low h and large d values into the boundary layer. This effect is however thought to mainly affect the d signal at timescales < 24 h and may be studied in more detail by analysing the driving processes of the daily cycle in d .”

3. This approach assumes the transpiration isotopic composition is the same as soil water (OK) but also that it’s in equilibrium with atm vapor and it’s constant over the 5-day correlation windows. This is not likely. Furthermore, the transpiration flux is likely h -dependent even if the dex composition is not.

We mention the fact that soil moisture d is assumed to remain constant over our analysis period in assumption 2 (see above). We provide an average partitioning over one 5 day event.

4. These high anti-correlation events should be driven by high soil or marine evaporation fluxes. Isn’t this analysis greatly biased to periods of low transpiration overall?

Yes, this is true, we mention this on p. 29749 l. 29 - p. 29750 l. 1:

“Furthermore, no estimates can be done for NRA or NLA events with our approach. For many of the NRA and NLA events, especially in summer, the high transpiration fraction is supposed to be responsible for the absence of anticorrelation between d and h_s .”

However, we would not say “greatly” biased, since the maximum transpiration fractions that we obtain for one specific event are around 90%.

2 Specific comments

1. p. 29725, l. 9: And because the mean relative humidity of air at the evaporating ocean surface is $< 100\%$.

We changed the text as follows:

“Craig (1961) found that the global mean meteoric water has a d of 10% , reflecting the fact that the ocean is generally out of equilibrium with the atmosphere and the mean relative humidity of air at the evaporating ocean surface is $< 100\%$.”

2. p. 29729, l. 7: Actually, turbulent diffusion does fractionate. See Lee et al. 2009.

We added the following sentence including the reference on p. 29729, l. 10: “For continental evaporation, small-scale turbulence may even enhance rather than suppress non-equilibrium fractionation (Lee et al., 2009).”

3. p. 29730: In the methods section: Mention the analyzer used (Picarro model #). Also comment on the instrument precision and accuracy. And how the calibration was applied to the data. Note that this calibration method does not span the full range of water vapor mixing ratios observed at this site.

We added the information on the analyser used, the precision and accuracy as well as how the calibration was applied to the data. The text was changed as follows:

On p. 29730, l. 23:

“The instrument used is a Picarro L1115-i cavity ring-down laser spectrometer. For hourly measurements, the precision of the measurement system for d is 0.3% and the accuracy is 3.1% (see

Aemisegger et al., 2012, for technical details).”

On p. 29731, l. 9:

“During the daytime calibration period only 2 calibration runs were performed at the ambient water vapour mixing ratio with the two standards. If variations in water vapour mixing ratios during the day were >1000 ppmv, the measurements were corrected for water vapour mixing ratio dependency (see Aemisegger et al., 2012). The water vapour mixing ratio corrected $\delta^{18}\text{O}$ and $\delta^2\text{H}$ measurements were then calibrated using the known reference standards with a linear relation between the measured standard δ -values and their reference value (Table 3) and normalised to the international VSMOW2-VSLAP2 scale (IAEA, 2009). The $d = \delta^2\text{H} - 8 \cdot \delta^{18}\text{O}$ was obtained from the calibrated δ measurements. The 5 s calibrated water vapour measurements were averaged to hourly isotope and water vapour mixing ratio data.”

The chosen calibration method does span the full range of the water vapour mixing ratio at this site as we calibrated at 3000 ppmv above and 3000 ppmv below measured ambient water vapour mixing ratios.

4. p. 29731, l. 1: 9 mL/min Typo? 9 L/min?

Yes, it should be 9L min^{-1} , we corrected the typo.

5. p. 29732: Trajectory calculations and moisture source diagnostics. Can you comment on how representative the reanalyses are of the surface conditions (temp and h)? Citations?

Both the COSMO analyses and ERA-Interim reanalyses are nudged to observations and are thus by construction in good agreement with surface observations. We added the the following sentence on p. 29733, l.4:

“In general there is a good agreement between ERA-Interim surface conditions and observational data sets (Simmons et al., 2010; Pfahl and Niedermann, 2011)”

Furthermore a short comparison of the surface h_s conditions at the moisture sources of the two reanalyses datasets is presented in Section 4.2.

6. p. 29733, l. 4: Method ignores isotopic exchange independent of net flux. This is probably OK though.

Yes it is true that our method ignores isotopic exchange that occurs independently of the net water flux. We do not expect that such exchanges are important for the analysis presented in this paper, because we are interested in air parcel uptake locations where there is a net water flux. But these net water flux independent isotope exchange effects may indeed play a more important role, when analysing the daily cycle of water vapour isotopes in the continental boundary layer.

7. p. 29736: If I understand this, water vapor mixing ratio was measured at 1.5 m and then h was calculated using 2m air temperature and 5 cm depth soil temperature? Is this even necessary to discuss if the soil temp version is the only one used in the analysis.

Yes, we think it is important to discuss, since it is not a priori clear which of the two relative humidities (with respect to soil temperature or with respect to 2 m air temperature) is the driving force for the water vapour d variability.

8. p.29737, l. 12: I'd like to see thesis figures included as an appendix or online supplement since they are not previously published in peer reviewed literature.

We added a figure with two panels showing 2 zooms into Figure 5. One zoom in summer shows the daily d cycle and one in winter shows the covariability of d and h_s^i

9. p.29737, l. 14: The scale of Fig 5 is such that the reader can't see rapid changes in d or tell if they are correlated with h or T .

See answer to comment 8.

10. p.29737, l. 16-25: Suggests that variability has been attributed. Please cite other studies that this conclusion is based on.
We added the following references:
On p. 29737, l. 18: "The drivers of these local processes have also been discussed in Lai and Ehleringer (2011) and Welp et al. (2012)."
On p. 29737, l. 23: "This variability component is dominated by processes at the remote moisture source (Pfahl and Wernli, 2008; Pfahl and Sodemann, 2013)."
11. p.29738, l. 11: 24 h filtered data = daily mean? So the dex-h correlations were calculated based on 5 datapoints? Error/uncertainty analysis?
We use 5x24 h moving average filtered data. We mention the trade-off between including as many measurement points as possible and keeping the time window for the correlation analysis close to the typical weather event time scale on p. 29738 l. 11-16. With longer time windows we obtain qualitatively the same results, meaning we identify the same number of HRA and HLA events and they cover the same time periods. Furthermore we tested the significance level for the $d - h_s$ slopes shown in Table 5 and 6 with a t-test (see captions of Tables 5 and 6).
12. p.29744, l 5-10: Cite recent studies on this topic.
This sentence was removed, see answer to comment 9 from reviewer 1.
13. p.29746, l 16-19: Consider plotting visually instead of tables. Move tables to appendix.
We moved Table 7 to the Appendix. The others are essential in the main text.
14. p.29747, Eqn 3: Make clear this is for $^{18}\text{O}/^{16}\text{O}$ and D/H ratios.
We changed the text as follows:
"Using these assumptions the $^{18}\text{O}/^{16}\text{O}$ and the $^2\text{H}/^1\text{H}$ ratio of boundary layer water vapour can be expressed as (see Appendix B for the derivation):..."
15. p.29748, l29-30: As long as the dex of soil water is constant over the 5-day analysis period. Otherwise it could change the dex-h slope, especially after a rain event moves through.
We now mention this assumption in the text:
"Transpiration is assumed to directly transmit the signature of the soil moisture, which is assumed to be constant over 5 day period."
16. p.29748,l. 24-25: Very sensitive is an understatement. Make clear what set of fractionation factors is used for the values listed in the text.
We state which fractionation factors we use on p.29748 l. 6. We added Appendix C with a sensitivity study on the use of the non-equilibrium fractionation factor. Furthermore in the text we mention the impact of the choice of the non-equilibrium fractionation factors on the average transpiration fraction value for all HRA events:
"The event-to-event variability of the transpiration fraction associated with the different HRA events is large and varies between 0 and 89 % with an average value of 62 %. These values are very sensitive to the non-equilibrium fractionation factors and increase with larger α_c^k (see Fig. 11b as well as the sensitivity study in Appendix C). The sensitivity of the transpiration fraction estimates obtained using Eq. 3 with respect to different non-equilibrium fractionation factors for soil evaporation is much more important than the way how the closure assumption is applied. When using the non-equilibrium fractionation factor for dry soils of Mathieu and Bariac (1996) we obtain an average transpiration fraction for HRA events of 73% with a minimum value of 31%. Particularly for low transpiration fractions, when soil evaporation is strong, the choice of the non-equilibrium fractionation factor becomes very important."
17. p. 29753,l. 11-14: It's this kind of statement that needs to be qualified given the large uncertainties and limitations of this approach.
We changed the text as follows:
"The characteristics of the $d-h_s^r$ relationship may be a useful proxy for attributing ambient water vapour to ocean evaporation, soil evaporation and plant transpiration, even though the uncertainties

associated with this attribution method are still large.”

and a few sentences later: “As the estimates from other isotope methods for the separation of the evapotranspiration flux, the transpiration fractions obtained in this study are associated with important assumptions and uncertainties that have to be addressed in future research.”

18. Fig 2: Inlet height appears to be 6 m, when it’s in fact 1.5 m.
We added a reference line for the ground and a double sided arrow for the 1.5 m height of the inlet.
19. Fig 5: Notation is changed here. ‘ ℓ ’ instead of ‘m’ for local measured values. All black would be fine here.
Was already corrected in the online version. We still use colours to provide the link to the new Figure including zooms of Fig. 5.
20. Fig 6: Explain 24 h filtered. Daily average? Is it just my imagination, or are there more datapoints in panel b than in panel a?
*24 h filtered means hourly data averaged over 24 h moving window averages. We changed the caption as follows:
“Time series of 24 h moving average filtered d signal...”
Panel a and b have the same number of data points.*
21. Fig 9 and 10: Include error bars on the intercepts.
We added error bars on the intercept.

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