Anonymous Referee #2

Received and published: 17 August 2016

Summary:

The authors present an analysis of two weeks of atmospheric water vapor stable isotope measurements in a semi-arid environment. They focus on understanding the potential drivers of D-excess variability they observed in the near surface atmosphere. They use the short-term Keeling plot method to calculate the isotopic composition of the ET flux and find that under these conditions, ET cannot explain the increase in mid-day D-excess which has been observed in many other locations and studies. They use radon concentration measurements to constrain the influence of entrainment of moisture with a different isotopic composition from the free troposphere and don't find much support for an anomalous signal from the free troposphere. In the end, they conclude that the fact that mid-day D-excess is correlated with local RH, means that an oceanic evaporation signature is unchanged as the air mass passes over the dry land mass.

We thank the reviewer for their comments. Our responses are detailed below in italics.

Major comments:

This paper is appropriate for HESS, but there are major flaws in the discussion and analysis that need to be addressed before publication.

1. The authors should provide more details of their methods. They should discuss analytical uncertainty of their measurements, especially the dET calculations. Small ET fluxes make measuring the dET values difficult. Were the plexiglass chambers tested for isotopic effects?

As noted in our response to Reviewer 1, we can include comments about analytical uncertainty into the results and methods sections, in particular, the Keeling plot intercepts and CG for soil evaporation front modelling.

For the chamber measurement, whether the ET fluxes are small or not is irrelevant for determination of d_{ET}. Our method for determining d_{ET} was based on using flux chambers and the Keeling plot method, so the change in H_2O concentration during a chamber measurement and the difference between the isotope composition of ET fluxes and ambient vapour are the variables that influence d_{ET} uncertainty. As discussed in the text in lines 234-244, we used a quality control routine to ensure that the assumptions of the Keeling method were met.

2. Throughout the discussion of the results, the authors comment on how their results contradict previous studies. The results are in fact different, but I believe they represent very different environmental conditions and the discussion should be prefaced with that in mind.

In fact our results are not different, as we observe a very similar D-excess diurnal cycle as other studies (e.g. Bastrikov et al., 2014; Simonin et al., 2014; Welp et al., 2012). *So in this sense we do not contradict other studies. However, by adding d_{ET} measurements we are able to provide a more conclusive role for ET fluxes in the D-excess diurnal cycle. While we contradict the conclusions of Simonin et al.* (2014) *and Zhao et al.* (2014) *(as noted lines 588-589), they do not provide direct measurements of d_{ET}. Others have been more circumspect* (Bastrikov et al., 2014; Welp et al., 2008). *Regardless, our results are very similar, but are able to provide different (or more conclusive) interpretations through directly measuring d_{ET}.*

As reviewer 2 indicates, it is certainly possible (likely) that we are observing different environmental conditions to the other studies referenced above. We agree with this statement and provided context of our findings in the discussion (4.2) and also mention this in the abstract. We can further modify section 4.2 to make this clearer: in particular in paragraph 2 of section 4.2 where we can add more direct reference to the literature for context of our results.

3. The discussion of using dv as a tracer of RH of the oceanic moisture source region contains many errors and is a misrepresentation of Aemisegger et al. The original application is to use dv along with d18O and dD to solve for temperature and RH of the oceanic source region, not to assume that RH near the ocean surface in 100%. Ocean surface humidity is more like 75% on average anyway. A strong correlation between local dv and local RH does not necessarily imply a preserved signature of the oceanic moisture source region. This would require that local and source RH are tightly coupled. Or, that changes in local RH are driven by mixing with a constant isotopic source of moisture (e.g. the free troposphere). The authors do not describe the Aemisegger approach correctly. Their aim was to estimate terrestrial evapotranspiration based on assumptions about the oceanic moisture source informed by back-trajectories and climate observations.

Reviewer 2 is indeed correct that the main aims of Aemisegger et al (2014) was to estimate terrestrial evapotranspiration using d_v as a tracer. However, within their paper they use the precise methodology described in our section 4.1 to estimate the D-excess of the average liquid moisture source. We refer the reviewer to page 14 of section 5.1 and Appendix A in Aemisegger et al (2014). Please also refer to figures 7, 10 and 11 from Aemisegger et al (2014) where the methodology is applied.

Reviewer 2 appears to have misunderstood the application of our methodology, which was taken from Aemisegger et al (2014). This methodology does not assume the RH near the ocean surface is 100% and it does not model the vapour D-excess of the moisture source. Instead the method uses the closure assumption of Merlivat and Jouzel (1979) and shows that for RH=100% the C-G model reduces to $R_v = R_l/\alpha$ (R_v=vapour isotope ratio, R_l=liquid isotope ratio and α=equilibrium fractionation factor). By definition α for equilibrium processes is very close to 1, so that $R_v=R_l$ for RH=100%. Based on this derivation, Aemisegger et al (2014) use the relationship between RH and d_v and extrapolate to an RH of 100%. This reflects a weighted average of D-excess values for contributing liquid moisture sources.

As the reviewer points out, this implies tight coupling between local and source RH. Exchange between the ABL and free troposphere could impact upon this relationship. There is no way we can determine if this was the case from our dataset (which we discuss in the same section - lines 562-585). However, to produce the strong relationship we see between RH and d_v, the free troposphere source of moisture must have a relatively constant D-excess, otherwise the relationship would be weakened. Likewise, for multiple moisture sources from the surface, as reviewer 2 surmises, these are likely to significantly weaken the relationship between RH and d_v. So while we cannot rule out the influence of these effects, we conclude that the d_v during the day indicates a large remote moisture source: most probably a large reservoir such as the ocean.

To accommodate the misunderstanding and concerns of reviewer 2, we will provide some additional details of the methodology of Aemisegger et al (2014). In particular, reference to the closure assumption of Merlivat and Jouzel (1979) will be made. We will also make it clearer that we are not aiming to calculate the D-excess of the vapour at the remote moisture source, but the liquid source D-excess. Additionally, in our discussion of the methodology we will include details to address concerns about coupling between local and source RH, with direct reference to multiple sources and not accounting for ABL/free tropospheric exchange.

3. This study is too short to examine synoptic variability with any depth.

We have not examined synoptic variability in depth: we simply refer to synoptic conditions to provide context for our short study. As outlined in addressing reviewer 1's comments (lines 27-43 of that response), given the relatively short duration of the campaign, providing some synoptic context was appropriate. In doing this, we refer to the specific conditions evident during the campaign, but also examine what conclusions may be relevant in a wider context. This is the purpose of section 4.2.

Specific comments:

ln 31: citation missing

We prefer to leave references out of the abstract as we feel it infers we are directly evaluating the referenced paper, which we are not. Relevant references are included in the Introduction.

ln33-35: there are a fair number of dET measurements published, which you discuss later in fact.

There are a number of studies presenting d_v measurements, but only Huang et al. (2014) presents actual d_{ET} measurements, which is referenced in our paper.

ln 126-127: Welp et al. measured dET

They measured d_v (see abstract and methods) and modelled the D-excess of transpiration (see section 4.3). As we stated in the text, d_{ET} measurements were not made.

ln 144: lat/lon

Done.

section 2.2.1: Please comment on the non-linearity of the delta values with respect to water vapor mixing ratio of the LGR analyzer and the stability of the calibration before/after the field experiment. The Picarro calibration method does not correct for water mixing ratio dependence of the analyzer. At what water levels were the analyzer uncertainties characterized?

It may be that this section is not clear, as we explicitly corrected for water vapour cross-sensitives for both analysers, since this is one of the major contributors to measurement uncertainty. We have mentioned this on line 165 and line 175, but can attempt to make this even clearer in the text.

ln 191: how long was the tubing and what was the flow rate in them?

We have added this information – "Approximately 20m of tubing was required to connect the tower inlet to the analyser. A vacuum pump (MV 2 NT, Vacuubrand, Wertheim, Germany) was used to draw air through all inlets to the analyser at a flow rate of 10 $l.min^{-1}$."

ln 289: what modifications were made to West et al.?

Our modifications were minimal, simply using our own vacuum line. We will remove 'similar' from the text.

ln 374: significant periods of the day were excluded to characterize a diurnal cycle.

We agree that 'diurnal cycle' is misleading, so will change the wording to indicate that we refer to the transition between the stable nocturnal and convective boundary layers.

ln 377-381: Is there any evidence that this much difference between soil water and the evaporation front could be real?

We believe this difference is entirely possible and not at all surprising. Dubbert et al. (2013) observed a large enrichment in soil moisture $\delta^{18}O$ values near the surface in their soil profile measurements, as did the seminal work of Allison et al. (1983). Besides literature evidence, our 0-5 cm soil measurements showed low D-excess compared to the LMWL indicating evaporative enrichment. It can be presumed that moisture at the evaporation front would be much more enriched and D-excess much lower. We will add further reference to the literature to support our measurements and expand on the reasons for confidence in the modelled soil isotope values.

ln 401-406: Are you referring to Fig 7 here? It's very difficult to see these features in the data as it is plotted.

Yes, we are referring to figure 7, as indicated at the start of this paragraph. We believe the drier mixing ratios observed from May 5th are quite clear in the plot. However, we can attempt to make this clearer to the reader.

ln 458-460: I'm not sure about this. I think you have to make a stronger case that it's not entrainment of air from above the boundary layer.

Indeed. We discuss this precise issue later (lines 562 to 585) and the fact that we cannot rule out entrainment as a possible explanatory mechanism.

ln 485: typo? 'encroachment'

Encroachment mixing is common terminology used in boundary layer meteorology, referring to the process where the mixed layer encroaches upwards as the layer warms.

ln 537-546: This paragraph has major problems. See #3 above. The authors come to some unsupported conclusions here based on a misunderstanding of many of the processes controlling vapor isotopes.

We disagree that there are any unsupported conclusions in the text and refer the reviewer to the comments above (lines 59-89).

ln 566-569: under what conditions was this observed?

We will make this more clear by referring the reader to the correct figure (Figure 8) - ***Error! Reference source not found.*** *shows that following the morning transition, a drying trend observed during the day, indicating entrainment fluxes were larger than ET fluxes, which has been previously shown using large-eddy simulations (Huang et al. 2011) and observations (Davis et al. 1997)."*

ln 608-609: the two processes have very different fractionation factors as well

We have modified this passage to include the difference in fractionation factors – "Relative magnitudes of evaporation and transpiration fluxes are important for d_{ET}. The two processes draw on moisture from different depths within the soil column and have very different fractionation factors, so fluxes are likely to have different D-excess values.

ln 632: Didn't you screen out nighttime dET measurements? Consider showing a plot of dET time series.

Yes this is true. We will change the terminology to indicate more explicitly that we are referring to transitional periods between the stable and nocturnal boundary layers.

Fig 6: This figure needs more discussion.

We have discussed this figure across three separate paragraphs in section 3.2. If the reviewer could be more specific about their concerns we would be happy to address them.

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