Anonymous Referee #1

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General comments:

The study "Response of water vapour D-excess to land-atmosphere interactions in a semi-arid environment" by Parkes et al. analyses the interplay between changes in atmospheric moisture isotopic compositions and the impact of local scale forcing of evapotranspirative vapor isotopes. As recently the isotopic composition of atmospheric moisture has been proposed as a tracer of large scale moisture recycling, this is an interesting topic. Also, apparently the impact of isotopic compositions was often studied using modelling approaches, which is rather surprising to me, given the increasing amount of isotopic ET studies recently! The overall quality of the paper notwithstanding, I see quite some space for improvement both technical and content related.

We appreciate the reviewer's comments and thank them for their time. Our comments and responses are shown in italics.

With respect to the reviewer 1's general comment "Also, apparently the impact of isotopic compositions was often studied using modelling approaches, which is rather surprising to me, given the increasing amount of isotopic ET studies recently!", *we are unsure of the reviewers meaning or the section of the paper they are referring to. Perhaps they refer to the sentence in the introduction (lines 113-115) that states "The studies of the dv diurnal cycles have largely relied on isotopic models to assess the contribution of ET fluxes, but a lack of dET measurements make it difficult to draw robust conclusions." This is true, as the papers we refer to (which study the diurnal cycle of* d_v *and the role of ET) have used process based models (Welp et al., 2012), empirical models (Simonin et al., 2014), or provided an interpretation of their data* (Zhao et al., 2014) *without providing any direct measure of D-excess in ET.*

1) In my opinion the study campaign was rather short, only 2 weeks roughly, and to my mind the significance of the interpretation is hence limited.

While the study period was relatively short, our study was completed in a remote semi-arid environment. These ecosystems are largely under-represented in isotope literature, especially within Australia, and also in using in-situ analysers. Given the duration, we do not make any major conclusions or claims about impacts on the hydrological cycles. Indeed, we deliberately focused the conclusions so that they relate to the specific meteorological conditions observed during the experiment (quiescent meteorology and extended dry periods, which are common to semi-arid environments) and what these might mean more generally for d_v *and* d_{ET} *variability over the longer term, as well as for other conditions and environments. We believe that this is an important and quite novel set of results.*

Beyond the ecological setting of our study, providing direct measurement of d_{ET} *is also novel, and shows that for our location and meteorological conditions, ET does not cause the relatively high D-excess values. We believe this is a useful and interesting finding, especially when in terms of providing context to other studies of longer duration and in different locations (e.g. Bastrikov et al., 2014; Simonin et al., 2014; Welp et al., 2012), we observed a very similar diurnal cycle. We have used section 4.2 in the discussion to elaborate on the context of our work: in particular, the long dry period leading to very low soil moisture D-excess values and how this may be applicable elsewhere. We will modify section 4.2 to include comments on the duration of the observation period and further emphasise the context of our measurements.*

2) I have some methodical concerns regarding laser spec calibration and chamber construction (see detailed comments).

Fully addressed below for relevant specific comments.

3) To my mind both results and discussion section are rather long and very detailed. Moreover, quite often results are repeated within the interpretation section, making the manuscript rather hard to follow at that point (very unlike the intro and M&M part btw.). I suggest to focus on the main results and shorten both parts to make it easier to follow.

Comments noted and we will streamline results and discussion sections where appropriate. We will ensure that results are not over-repeated within the discussion section.

Specific comments:

48ff: Be more specific! How?

Sentence modified – "Spatial and temporal variability of D-excess in ET fluxes therefore needs to be considered when using dv to study moisture recycling and during extended dry periods may act as a tracer of the relative humidity of the oceanic moisture source."

60ff: I think this is a bit overstated, there are surely some examples here!

We have clarified this sentence to reflect that datasets directly quantifying land-atmosphere exchange processes are rare – "To do this effectively, a range of datasets that directly quantify a variety of processes represented within these models are required. Unfortunately, datasets that directly measure land-atmosphere exchange at the process level are limited."

63ff: Shouldnt this be 2 sentences?

We are not sure what the reviewer is referring to here, as this is already two sentences.

73ff: how about transport processes? i.e. kinetic fractionation?

We have changed the sentence to include 'equilibrium and kinetic isotopic fractionation' i.e. "The utility of water isotope ratios for tracing sources of moisture derives from the characteristic equilibrium and kinetic isotopic fractionation that occurs when water undergoes a phase change, causing light water molecules to preferentially accumulate in the vapour phase."

81ff: Again, doubt there are so few. How about Berkelheimer, Simonin, Welp and others?

Our statement is that there are relatively few studies using vapour, relative to precipitation focused studies. The references mentioned by the reviewer do indeed discuss land atmosphere exchange for vapour isotopes, and we have referred to these in other sections of the manuscript. We have added (e.g. Aemisegger et al. 2014; Risi et al. 2013) to indicate some of these related studies.

98: Suggest to change "given this" to therefore

Noted and adjusted.

140: have has? omit has?

Noted and adjusted.

168: I f you indeed did not calibrate or drift check the LGR i think your values have a high uncertainty. I.e. the average difference to the Picarro might be small but you standard deviation suggests there was a high point to point difference. At the least it would be nice here to see the time evolution of the difference between laser specs throughout the campaign!

We are not sure if the reviewer is questioning whether we present raw LGR data (i.e. no calibration corrections applied) or whether there were no calibrations run during the campaign period.As stated in the text (section 2.2.1), we calibrated the LGR in the lab before and after the campaign to develop corrections for water vapour cross-sensitivity and linearity. This was completed simultaneously with the Picarro. During the campaign no calibration experiments were completed for the LGR, but to determine the importance of instrumental drift for our measurements, we regularly ran the two analysers simultaneously sampling ambient vapour (lines 180-188).

Reviewer 1 raises an important point regarding the drift for the LGR. While on average the Picarro and LGR agreed over the campaign, there was some shorter term drift that led to differences between analysers: most likely the result of the LGR's large temperature dependence. We did not include a comparison of the two analysers, as the differences between them is defined mostly by scatter with no clear trends. Interestingly though, there is no relationship between differences between the analysers and the major shift in H_2O concentration (i.e. wet vs dry period). This indicates that we have accurately characterised the water vapour cross-sensitivity of the two analysers and that this correction was stable throughout the campaign.

In line with Reviewer 1's comments we will include a figure showing the time series of differences between the two analysers over the campaign (see figure 1 below). So as not to detract from the main message of the paper we will place the figure and discussion of its consequences for our interpretations in the supplementary section. In constructing this figure we realised the biases listed in the paper were for the whole comparison period, which included nocturnal hours. This is not what we have indicated in the text (line 182) and is not a fair comparison for our measurements, as the LGR showed a very big temperature dependence that led to nonsensical values at night (hence our restriction of chamber measurements to 09:00 to 17:00). The comparison is more favourable during the day when the LGR cavity temperature was relatively stable and chambers measurements were made. We will update the bias statistics to the values shown in figure 1 below.

In some cases the differences between ambient d_v and d_{ET} were quite small, so I_{ET} calculated for the D-excess could be strongly influenced by LGR instrumental drift. However, this does not affect our interpretation, as for all chamber measurements that passed our QC requirements D-excess decreased during the measurement, illustrating that ET always had a negative forcing on d_v. To deal with the uncertainty caused by the relative instrumental drift of the two analysers, we will include statements in the text emphasising that while I_{ET} would be influenced by drift, our interpretation of negative forcing remains the same.

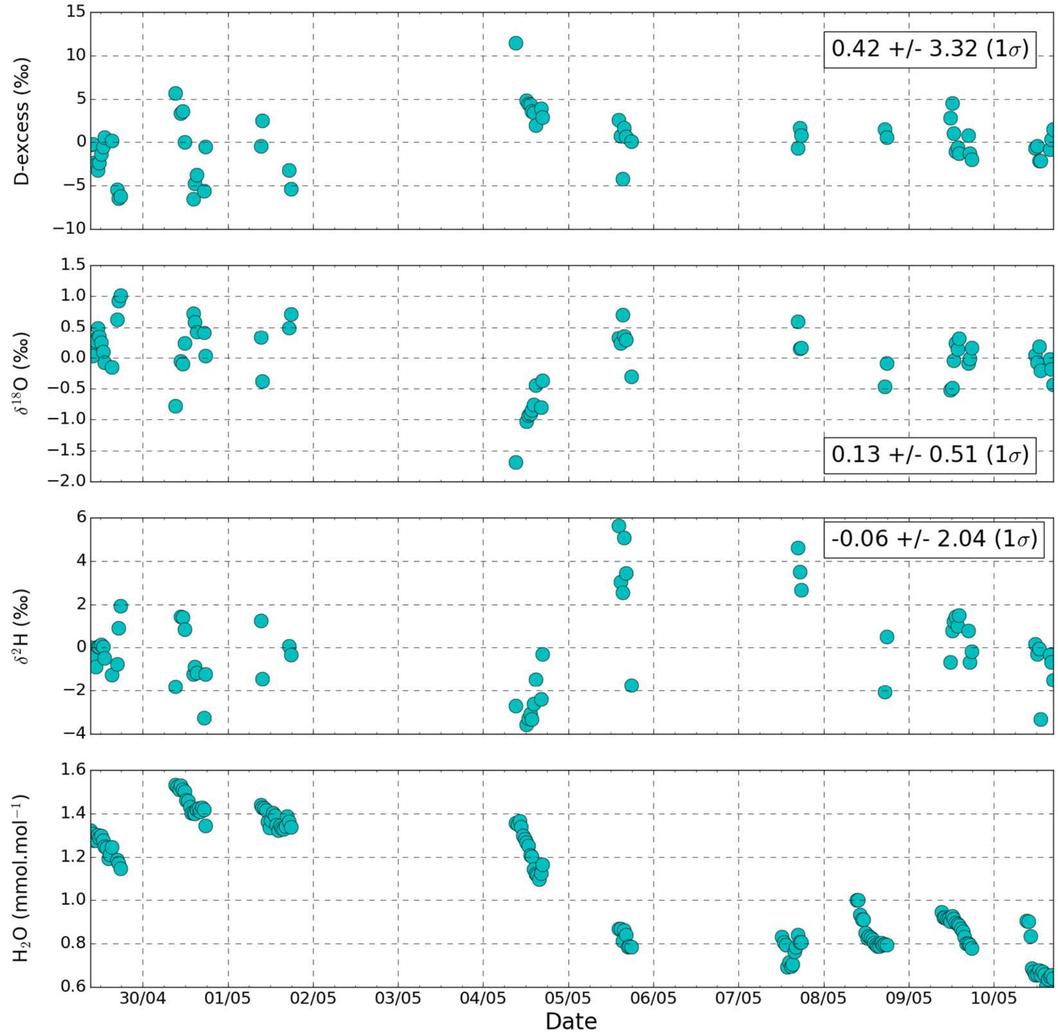

Figure 1: Time series of daytime differences between isotopic measurements of Picarro and LGR for periods when the LGR and Picarro were simultaneously sampling from the meteorological tower. The H_2O concentrations measured by the Picarro for these periods are shown.

207: Strong doubts concerning you placement of collars only 2 days prior to measurements! this will surely cut roots and there will be some affects in that direction.

We agree with the reviewer that collars were installed a relatively short time before the study period. However, the vegetation consisted of grasses with shallow roots (~5cm), so while near the edge of the chamber, roots may have been cut, the vast majority of the vegetation cover was unaffected. As with all chamber measurements, the apparatus can influence the environment and thus fluxes, but these would not change our interpretations here. We can add a sentence to include the reviewer's comment on this issue.

211: Did you coat the chamber in some ay? It is well known that Plexi exchanges water and acts like a sponge creating a smearing effect in background chamber and vice versa transitions. This could actually affect you keeling plots quite much.

No, we did not coat the chamber to reduce memory effects. We assume the reviewers comments are related to memory effects influencing ET isotopic compositions calculated from the Keeling plots of chamber measurements. As the reviewer correctly notes, memory effects could have a major effect on *the determined ET isotopic compositions. Indeed, we considered this and to combat memory effects we employed high flow rates, as the high turnover rates will reduce such memory effects. We also developed quality controls for Keeling plots, ensuring linearity and a significant H_2O concentration change was observed.*

While memory effects are unavoidable and can influence ET isotopic compositions, they do not change our interpretation. Memory effects are likely to attenuate the slope of Keeling plots, thus reducing the disparity between ambient vapour and ET isotope composition determined from the intercept of these plots. This is because chamber walls retain the isotopic composition of the ambient vapour being mixed with the ET flux. So, while memory effects would cause a high bias for the determined d_{ET} (i.e. Keeling plots for the D-excess always had a negative slope), our purpose was not to assess absolute d_{ET} values, but to determine whether ET could cause the d_v diurnal cycle: in particular the high daytime values. As such, this interpretation remains unchanged.

Regardless, it is an important point and we can include mention of memory effects in the methods and relating our methodology to how these were dealt with. In the results we will review the consequence of these for our interpretations.

230ff: Why did you choose the Keeling method? Why not a mass balance approach?

Studies comparing the two methods have shown they are comparable (Lu et al., 2016; Wang et al., 2013):, which is not surprising, as they are based on the same assumptions (i.e. that background concentrations and isotopic compositions of source and background water vapour does not change during a measurement). The main difference is that the Keeling plot requires extrapolation to determine the intercept of the $\delta_{chamber}$ vs $1/q_{chamber}$ plot. Comparisons in the literature have shown they agree well in practice. Considering the focus of our work was not to evaluate the two methods, we only present data using the Keeling model. In addition, as discussed in lines 237-256, we developed a filtering approach for the Keeling model focussing on ensuring linearity of our Keeling plots. We will add a comment to indicate that we considered mass balance, but based on literature findings, decided it would not have made a major difference on results.

256: Did you not measure soil water isotopes directly ? What is the uncertainty of the model approach?

Soil water isotopes were measured, as presented in section 2.2.5.

The uncertainty of the model approach is governed by the uncertainty of the chamber measurements of ET isotopic compositions and the parameterisation of Craig-Gordon (GG) model. While it is difficult to assess the accuracy of the CG model without direct observations, we did try different parameterisations (i.e. using Cappa et al (2003) vs Merlivat (1978) diffusion coefficients, and different values for the diffusion exponent). This had a large effect compared to uncertainty in ET isotopic compositions, but does not change the interpretation that soil water at the evaporation front was very enriched with very low D-excess values. While assessment of the CG model was not our focus, we can certainly provide some mention of the uncertainty of the model and how this may impact upon our results/interpretations in the discussion of water pool isotopic compositions (results section 3.2).

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