**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.

**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?
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.
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.
4. This study is too short to examine synoptic variability with any depth.

**Specific comments:**

ln 31: citation missing
ln33-35: there are a fair number of dET measurements published, which you discuss later in fact.

ln 126-127: Welp et al. measured dET
ln 144: lat/lon
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?
ln 191: how long was the tubing and what was the flow rate in them?
ln 289: what modifications were made to West et al.?
ln 374: significant periods of the day were excluded to characterize a diurnal cycle.
ln 377-381: Is there any evidence that this much difference between soil water and the evaporation front could be real?
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.
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.
ln 485: typo? 'encroachment'
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.
ln 566-569: under what conditions was this observed?
ln 608-609: the two processes have very different fractionation factors as well
ln 632: Didn't you screen out nighttime dET measurements? Consider showing a plot of dET time series.
Fig 6: This figure needs more discussion.