Review of "Deuterium excess as a proxy for continental moisture recycling and plant transpiration" by Aemisegger et al.
Submitted to Atmos. Chem. Phys.

Summary:
The authors present continuous measurements of deuterium excess (dex) from a location in Northeastern Switzerland. They use Lagrangian back-tragectories to calculate the flux-weighted average humidity and temperature along the moisture source regions using a previously published method. They then do correlation analysis primarily between dex and humidity measured locally and from the Lagrangian remote source diagnostic. They find that only some periods show the 'expected' anti-correlation between dex and humidity (h) at either location (at low humidity, non-equilibrium kinetic effects are larger and dex of evaporated water is higher). They propose that the slope of the dex-h relations over the period of several days can be used to partition continental moisture sources into soil evaporation and plant transpiration. Shallower slopes mean more plant transpiration; steeper slopes mean higher soil evaporation. The ocean vs. continental moisture source partitioning is done using the Lagrangian moisture source analysis.

General comments:
This presents a novel approach to analyzing continuous water vapor stable isotope measurements in the surface atmosphere layer. The goal of partitioning terrestrial ET into components is important and challenging, especially at the landscape to regional sclaes. 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.

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.
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).
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.
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?

Specific comments:

P5, L17: And because the mean relative humidity of air at the evaporating ocean surface is <100%.

P10, L2: Actually, turbulent diffusion does fractionate. See Lee et al. 2009.

P11: 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.

P11, L11: 9 mL/min Typo? 9 L/min?

P13: Trajectory calculations and moisture source diagnostics. Can you comment on how representative the reanalyses are of the surface conditions (temp and h)? Citations?

P14, L10: Method ignores isotopic exchange independent of net flux. This is probably OK though.

P18: 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.

P19, L 10: I'd like to see thesis figures included as an appendix or online supplement since they are not previously published in peer reviewed literature.

P19, L11-13: The scale of Fig 5 is such that the reader can't see rapid changes in dex or tell if they are correlated with h or T.

P19, L13-17: Suggests that variability has been attributed. Please cite other studies that this conclusion is based on.

P20, L13: 24 h filtered data = daily mean? So the dex-h correlations were calculated based on 5 datapoints? Error/uncertainty analysis?

P27, L11-13: Cite recent studies on this topic.

P29, L21-25: Consider plotting visually instead of tables. Move tables to appendix.

P30, Eqn 3: Make clear this is for 18O/16O and D/H ratios.

P31, L11-13: 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.

P32, L5-8: Very sensitive is an understatement. Make clear what set of fractionation factors is used for the values listed in the text.

P37, L14-15: It's this kind of statement that needs to be qualified given the large uncertainties and limitations of this approach.

Fig 2: Inlet height appears to be 6 m, when it's in fact 1.5 m.

Fig 5: Notation is changed here. 'l' instead of 'm' for local measured values. All black would be fine here.

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?

Fig 9 and 10: Include error bars on the intercepts.

References:

Lee, X., T. J. Griffis, J. M. Baker, K. A. Billmark, K. Kim, and L. R. Welp (2009), Canopy-scale kinetic fractionation of atmospheric carbon dioxide and water vapor isotopes, Global Biogeochem. Cycles, 23, GB1002, doi:10.1029/2008GB003331.

Welp, L. R., X. Lee, T. J. Griffis, X.-F. Wen, W. Xiao, S. Li, X. Sun, Z. Hu, M. Val Martin, and J. Huang (2012), A meta-analysis of water vapor deuterium-excess in the midlatitude atmospheric surface layer, Global Biogeochem. Cycles, 26, GB3021, doi:10.1029/2011GB004246.