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Interactive comment on “Investigating the Source, Transport, and Isotope Composition of Water Vapor in the Planetary Boundary Layer” *by* T. J. Griffis et al.

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This manuscript presents a 3-year (2010-2012) dataset of stable isotope ratios in PBL water vapor (δ) sampled from multiple inlets on a tall tower in a crop field in the Upper Midwest United States. Precipitation isotope composition (2006-2011) also was presented. The main objectives of the study are 1) to investigate water sources and fluxes that contribute to the variability in mixing ratios and isotope ratios of water vapor in PBL, 2) using a land surface model and wavelet analyses to discern the importance of PBL processes (Rayleigh distillation, evapotranspiration (ET) and tropospheric entrainment) on δ, 3) to quantify ET contribution to PBL water vapor content and 4) to estimate recycling ratio of precipitation. Major findings of this manuscript include: 1) a dominant

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influence of ET on δ and deuterium excess (dx), 2) a considerable contribution by local ET to PBL moisture content in the growing season (>40%) and 3) a 30% precipitation recycling ratio during growing season for this region.

The debate over uncertainties in the isotopic mass balance calculation for the global estimate of plant transpiration to continental water fluxes (T/ET) seems to come to an agreement (Jasechko et al. 2013, Good et al. 2015, Evaristo et al. 2015). However, uncertainties in the importance of ET to region/local precipitation and atmospheric moisture content remain high and field studies that aim to reduce this uncertainty is welcome and should be encouraged. This paper tackles this knowledge gap with numerical modeling and simple two source mixing calculation of isotopic information. Conclusions are drawn from multiple-lines of results. This work will be a valuable contribution to the isotope hydrology community. I recommend publication with minor revisions. My comments aim to improve the presentation of this work, and are detailed in the following:

Ln 117&118, provide model # and manufacturers for tipping bucket rain gauge and snow board. Ln179 what is rationale in choosing NOAH (a simple bucket) land surface model? Is your calculation sensitive to choice of land surface scheme?

Ln223 is the condensation temperature of -3 oC at the lifted condensation level site specific? Is it set as a constant for the entire study period?

Ln231 it reads awkward to say ‘condensation is evaporated’; replacing precipitation/condensations by raindrops/condensates?

Ln236 be consistent with your definition of δET; evaporation and evapotranspiration are used interchangeably throughout the text

Ln242 here δET is determined using tall-tower flux-gradient approach, rather than a value of -7.7‰ from eddy covariance measurements. Why? How different are the two estimates?

Ln245 please explain why atmospheric gradients are considerably small for 2H

Ln263 This is an interesting approach. What is the vapor mixing ratio at 700hPa for the entire study period? This is important, as the power law function is hypersensitive at low mixing ratios.

Ln278 The authors state that “Following the methodology of Gat et al. (1994)”, a two member mixing model approach (Eq 11) was used to estimate the recycling ratio of precipitation. However, the method described in Gat et al. (1994) was quite different than a two member mixing model. The authors need to clarify the claim, and explain the basis of Eq (11). Why is deuterium excess used in this calculation, instead of delta ratios?

Ln300 What does this agreement mean? What is its significance?

Ln302 Do you mean uncertainty or range in the precipitation isotope data?

Ln303 Shouldn’t δET (-77.6‰ be relatively “enriched” compare to precipitation (-80‰ ln289)?

Ln310 It is very interesting that the authors chose to compare measurements made in a crop field in upper Midwest of USA to measurements made in an urban setting in China. If local ET play a major role in regional PBL water vapor (as claimed here), why would you expect the two studies comparable? I do not see the relevance of this comparison.

Ln319-354 The whole discussion on the discrepancy between observed δv and that calculated from equilibrium assumption with precipitation weakens the manuscript in my opinion. Equilibrium only holds true when h=100%. As the authors correctly pointed out, reasons (mechanisms) behind this discrepancy has been well established elsewhere (e.g., the citations given). Measurements of isotopic composition of water vapor are rare before spectroscopy analyzers became commercially available. Hence, isotopic composition of water vapor was commonly assumed in equilibrium with pre-

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cipitation (readily available) in the old days. The manuscript is better off without this comparison. I suggest the discussion here be shortened.

Ln345-350 Cut out these sentences; equilibrium should only be considered when h approaches 100%!

Ln388 delete “yielded a relation that”. How was WVL and leaf/soil water “inversely related”?

Ln390 just say ET rather than “leaves and soil”

Ln395-396 Duh! equilibrium should only be considered when h approaches 100%! Consider deleting ln 390-398 for reasons stated above.

Ln406, the very weak positive relation is in contrast with the negative d-rh relationship found over seawater and at some continental sites. Discuss the possible causes for the discrepancy.

Ln510, Report errors for the 42% estimate. Calculated fv has a very large spread around the best-fit function.

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