Response to review of Atmospheric Chemistry and Physics, MS No.: acp-2015-923

Title: Investigating the Source, Transport, and Isotope Composition of Water Vapor in the Planetary Boundary Layer
Author(s): T. J. Griffis et al.
MS No.: acp-2015-923
MS Type: Research article

We thank the Reviewers for their thoughtful comments, criticisms, and attention to detail. All of the reviewer comments have been addressed below (review query in Italic; our response in blue upright Times New Roman).

Reviewer #2

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 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.

We thank the Reviewer for their positive comments.

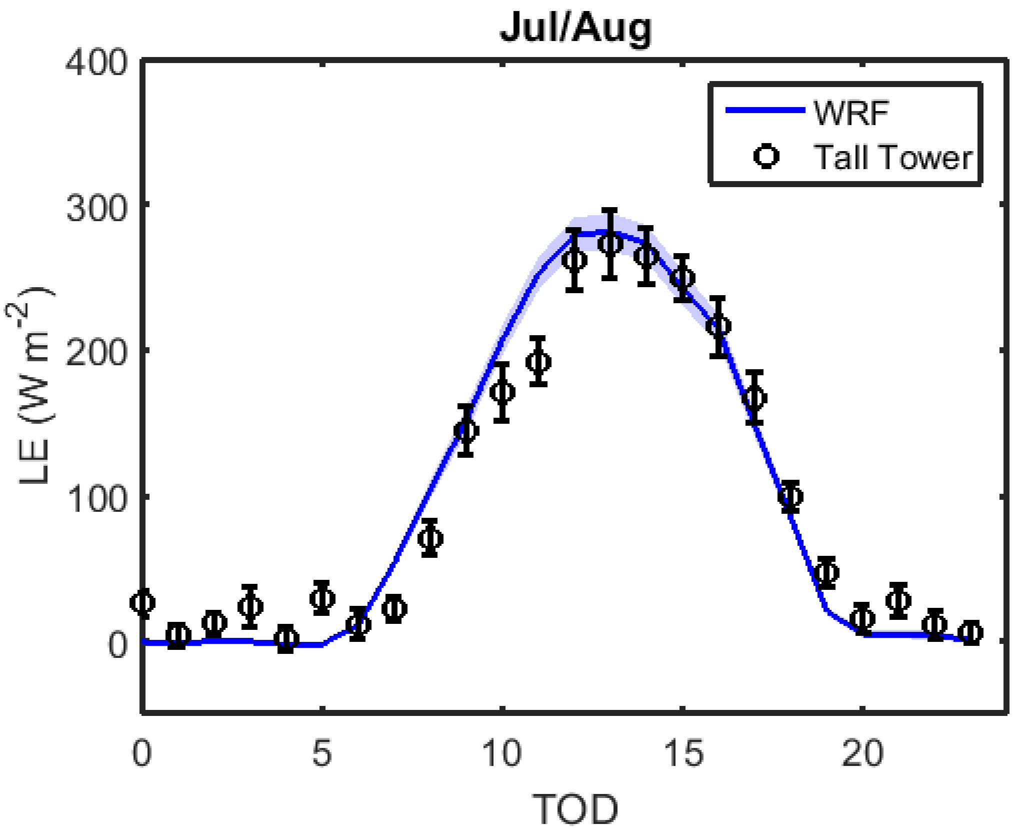
My comments aim to improve the presentation of this work, and are detailed in the following:

Ln 117&118, provide model # and manufacturers for tipping bucket rain gauge and snow board.

We have added this information as requested. The tipping bucket rain gauge used was model 6028-B, All Weather Inc., CA, USA, and the snow board was provided by the Minnesota State Climate Office (http://climate.umn.edu/doc/journal/snowboard.doc).

Ln179 what is rationale in choosing NOAH (a simple bucket) land surface model? Is your calculation sensitive to choice of land surface scheme?

Our group has been working with the WRF-NOAH system since 2012 and has been testing it as an evaporation/irrigation forecasting tool (http://www.biometeorology.umn.edu/research/etool). While more detailed surface scheme options could be used such as WRF-CLM (Community Land Model surface scheme option), they are computationally much more expensive. From our experience the WRF-NOAH system performs very well for our study domain. The Figure below (not shown in the manuscript) illustrates a comparison of modeled evaporation versus our tall tower eddy flux (185 m) estimates during the growing season. Based on these comparisons we are comfortable with the NOAH application in this current study.



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

Yes. The goal here is to simply provide an idealized case for comparison to the data. Please see response above to Review #1 who had a similar question.

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

Thank you. Reviewer 1 also caught this mistake and it has been corrected.

Ln236 be consistent with your definition of δET; evaporation and evapotranspiration are used interchangeably throughout the text
Thank you for catching this. As recommended by Brutsaert (1982) in his book "Evaporation into the Atmosphere", we believe it is best to use the term evaporation and have, therefore, eliminated our usage of evapotranspiration. This gives consistent terminology throughout the manuscript.

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?

To be consistent, we now use the tall tower flux gradient values for all calculations. The growing season value was -6.2 ‰ and in close agreement with other previous approaches at our site. Because this variable is difficult to quantify, and is inherently noisy, the other estimates from eddy covariance, field-scale flux gradient, and analysis of xylem water are described for context and to provide an independent constraint on this key variable.

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

This is simply due to the fact that the source strength is much smaller for deuterium. Therefore, for similar atmospheric mixing conditions, the gradient of deuterium in water vapor will be smaller compared to the gradient in the heavy oxygen isotope of water vapor. We have added a brief discussion to this effect on page 9 line 270.

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.

Over the three-year period the water vapor mixing ratio at 700 hPa was 3.9 mmol/mol. During the growing season (three-year period) the water vapor mixing ratio at 700 hPa was 5.9 mmol/mol. With respect to the power law function, these mean values occur before the function reaches its vertical asymptote (i.e. where it becomes hypersensitive). However, as shown in Figure 2, there are cases where the uncertainty in the background value will be large because of this sensitivity.

We have added the essentials of this discussion to Page 10 line 295.

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?

Here we made an incorrect reference. We followed the methodology of Kong et al., 2013, published in Tellus B, 65, Art. 19251. We have adopted their equation 7.

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

At relatively long times scales (seasonally) we would expect there to be isotope mass balance between inputs (precipitation) and outputs (evaporation, runoff, drainage). These data and analyses indicate that our atmospheric measurements provide a reasonable constraint on the isotope composition of evaporation, which is challenging to measure. We have added some addition discussion related to this point on page 11 line 335.

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

Here we are referring to the overlap of uncertainty in the flux and precipitation values. We have clarified this in the text.

Ln303 Shouldn't δET (-77.6‰ be relatively "enriched" compare to precipitation (-80‰ ln289)?

Yes. Thank you for bringing this to our attention. There was a typo in Table 1.

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.

We agree with the reviewer and have deleted this text.

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

As suggested, we have shortened this section and deleted the sentences noted below by Reviewer 2. Because this comparison represents one of the few ways to assess data quality, we wish to retain the essential elements presented.

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

Deleted as suggested.

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

We have re-worked this section and deleted "yielded a relation that" and deleted "inversely related".

Ln390 just say ET rather than "leaves and soil"

We changed this to "evaporation" as suggested.

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

We have modified this section and simply show that these relations are statistically different.

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.

Because the relationship is weak and not statistically significant we are reluctant to elaborate on a potential physical explanation for it at this time.

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

We now show the uncertainty bounds in Figure 9 and report the 1 sigma uncertainty range as 21 to 62%.