

Review of

“Investigating the Source, Transport, and Isotope Composition of Water Vapour in the Planetary Boundary Layer”

by T. J. Griffis et al.

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1 General Comments

This paper presents and discusses a comprehensive and very valuable dataset covering a three-year period (2010-2012) of hourly measurements in water vapour isotopes from a tall tower (inlets at 3 m and 185 m) in the planetary boundary layer (PBL) as well as event-based precipitation samples in the Upper Midwest, United States. The isotope signature of evapotranspiration was also determined from the tall tower flux-gradient measurements. The authors analyse the data using several simple isotope models to describe the processes occurring in idealised airmasses (1) due to condensation of an initial airmass of oceanic source (called RM1), (2) due to rain out of the condensate (called RM2) and (3) due to mixing between surface evaporate and near-surface atmospheric water vapour (called EM1). Estimates of “regional” evapotranspiration and of the recycling ratio of precipitation during the growing season are provided using a simple two end member mixing model. One selected month of hourly data is analysed in more detail with respect to the coherence of behaviour of different variables (vapour isotopes from the tall tower predicted isotope composition of the PBL with a simple Rayleigh model, Evapotranspiration, boundary layer growth, temporal change of the isotope signals) using wavelet coherence spectra. Relevant questions with respect to the continental water cycle and the processes driving the isotope variability in the planetary boundary layer are discussed. I recommend publication of this manuscript after the following points have been addressed:

A Presentation of the large amount of data:

1. The water vapour isotope and precipitation climatology presentation would be easier to follow if a Figure with 3 different panels ($\delta^{18}\text{O}$, $\delta^2\text{H}$, d) would show the vapour, precipitation and ET annual cycles.
2. Many Figures and Table 1 should be made clearer, see my specific comments below.
3. Additional Tables might be necessary to help the reader particularly, when data from older studies is used or assumptions are made for the use of the simple isotope models (see specific comments below).

B Application of the simple isotope models and numerical modelling for determining boundary layer heights and evapotranspiration:

1. A short motivation for the modelling approach (use of the simple isotope models as well as for the numerical modelling to obtain PBL heights) should be given in the introduction. Why are these models used? (p. 3, L. 65)
2. It should be made clearer during the presentation of the models in the methods how the “BestFitRM” and the “BestFitEM” are obtained and what they represent.

C Regional recycling ratios: When discussing the “origin” of boundary layer water vapour throughout the manuscript it is not clear what is meant quantitatively/geographically by “regional” evapotranspiration. The same is true for the presented precipitation recycling fractions. The authors mention on p. 17, L. 567 that “As discussed by Trenberth, 1998 the calculation of water recycling using numerical models is scale dependent”. This is also true for empirical recycling ratios and moisture origin quantifications. A statement on what the term “regional” means should therefore be given in the paper.

2 Specific comments

1. p. 1, L. 9: Instead of “Models” I would suggest “Several simple isotope models”.
2. p. 1, L. 9: I would avoid to use the term “Rayleigh process” in the abstract. Maybe “rainout along an idealised air mass trajectory” or at least “Rayleigh distillation process” would be clearer.
3. p. 1, L. 13: “at the annual time-scale” is confusing, only hourly and monthly mean data are discussed here, maybe “when looking at the full hourly dataset” would be clearer.
4. p. 2, L. 50-51: An other study presenting 5 months of deuterium excess measurements in the continental PBL is Aemisegger et al. 2014, ACP.
5. p. 3, L. 63: “Rayleigh” should be explained here.
6. p. 3, L. 74: I do not understand hypothesis 2, why would one assume this a priori?
7. p. 3, L. 61: d_x should be changed into d_v for vapour, d_p for precipitation,... to be consistent with the $\delta^{18}\text{O}_v$ notation used.
8. p. 4, L. 98: What was the pumping rate and tubing type used?
9. p. 4, L. 103: The isotope composition of the span values should be mentioned.
10. p. 4, L. 103: The sampling time is relatively short, what was the response time of the system?
11. p. 4, L. 112: No parenthesis for the reference to Griffis et al., 2010b
12. p. 7, L. 206: The reference to Lee et al. 2005 seems inadequate here for the definition of the liquid water isotope ratio.
13. p. 7, L. 220: “Three simple models...”: it should be mentioned here that the parameters for the models are chosen from “best guesses” to compare their predictions to the measurement data and that best fits of the parameters to the measurement data are subsequently calculated. It should also be mentioned how the fitting is done (i.e. which parameters are fitted) for the best fit lines in Figure 4. Furthermore, it would help the reader if a Table would summarise the parameters of the models and their assumed numerical values.
14. p. 8, L. 223: How was the condensation temperature of -3°C determined?
15. p. 8, L. 231: “where precipitation/condensation is evaporated” I do not understand this. Shouldn't it say “where precipitation/condensate is removed”?
16. p. 8, L. 237: Why is $\delta^{18}\text{O} = -7.7\text{‰}$ used for δ_{ET} ? Is this just a best guess?
17. p. 8, L. 243: Is δ_v from 185 m height used here? How independent are δ_{ET} and δ_v
18. p. 8, L. 250: This approach neglects changes in δ by advection. This should be mentioned.
19. p. 8, L. 253: It should be explicitly mentioned that substantial changes in the background δ_v depending on the synoptic scale weather situation can occur.
20. p. 9, L. 265-277: I do not understand the role of this paragraph. On L. 237 a value of $\delta^{18}\text{O} = -7.7\text{‰}$ is used. And there are the tall tower $\delta^{18}\text{O}_{\text{ET}}$ presented in Table 1. This is confusing for me.
21. p. 9, L. 277: The reference is missing.
22. p. 9, L. 280: Equation 11, shouldn't d be used here instead of δ ?
23. p. 9, L. 283: What are the uncertainties of the d_{ET} estimated from the tower measurements?
24. p. 11, L. 227: “biased low”, shouldn't it be “high”? And I do not agree with the terminology “biased”, which I find confusing. As the authors mention on L. 332 below-cloud evaporation can strongly affect the isotope composition of boundary layer vapour and precipitation. See also other experimental studies combining vapour and precipitation measurements: Risi et al. 2008, QJRMS, Tremoy et al. 2012, GRL, Aemisegger et al. 2015, GRL.

25. p. 11, L. 331: “depleted” can be confusing here. “ $d_{v,e}$ were ... lower than d_v ” would be better.
26. p. 12, L. 367: “warm bias” with respect to what?
27. p. 12, L. 383: add “the” before “curvature”
28. p. 12, L. 385: which panel in Figure 4 are the authors referring to? This last sentence of the paragraph (L.281-285) is not very clear to me.
29. p. 13, L. 419: “near surface water evaporation” is “soil evaporation” meant here?
30. p. 13, L. 420-425: very similar observations were made using PBL vapour deuterium excess in Europe (Aemisegger et al. 2014, ACP)
31. p. 13, L. 427: It is not clear which Rayleigh model was used here.
32. p. 14, L. 458: The hourly time series of the relevant variables for this case study should be shown in a Figure.
33. p. 14, L. 464: What is meant by “the tall tower domain”?
34. p. 15, L. 483: Does this mean that if bare soil instead of crop was there we would expect weaker convective precipitation purely from a moisture availability point of view? I am not sure I understand what “enhance” convective precipitation means here. Compared to which alternative scenario?
35. p. 15, L. 490: It would help to have a table for the latent heat fluxes of the different land use classes.
36. p. 16, L. 505: The PBL vapour partitioning should be discussed more critically. The correlation between the fraction of local water vapour and the local water vapour mixing ratio is relatively low and the scatterplot in Figure 9 looks very noisy.
37. Table 1 would be much clearer if the units were indicated in the individual columns. It is not clear what the two $\delta^{18}\text{O}_p$ exactly refer to. They seem to cover different time periods but it is not clear which is which. The additional information added below the table should be placed in the legend. Standard deviations of the hourly data for each month would be informative of the isotope signals’ variability in the different seasons. Why is the water vapour isotope data at 3 m not shown?
38. Figures in general: it would help the reader a lot if more and clearer references to the Figures were given in the text and alphabetical numbering of panels was used.
39. Figure 4: the colorbar is missing, it is not clear how BestFitRM, EM are obtained, it would help the reader if the chosen colormap would be white in the low density domain. Some of the lines are not easy to distinguish.
40. The role of Figure 10 is not clear to me.