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Interactive comment on "Vertical profile observations of water vapor deuterium excess in the lower troposphere" *by* Olivia E. Salmon et al.

Anonymous Referee #1

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The authors present the analysis of airborne measurements of the isotopic composition of water vapour over the continental US. The study will be an interesting addition to the literature, and contributes a valuable dataset. Aspects of instrument uncertainty and data calibration are thoroughly presented. However, some aspects of the interpretation need further clarification, and the structure of the manuscript could be improved. I suggest major revisions of this manuscript, as detailed in the comments below.

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Discussion paper

1 Major comments

1. I am not convinced by the way the Rayleigh model is used as an explanation in flights RAY and STC. Even though a Rayleigh model can match much your data, it does not necessarily indicate that the guiding processes are correctly represented. In particular, the presence of a quite homogeneous mixing line of water vapour below the inversion, topped by a pronounced inversion layer, and relative humidity below 10% is in stark contrast to the moist adabatic profile that would be associated with a Rayleigh model. I recommend to include the possibility of long-range advection of air with subsequent mixing into the picture, as well as considering the role of subsidence of air as a contribution to the inversion layer. Can several mixing processes in time and in the vertical combined look like a Rayleigh curve? This analysis should be placed in the context of the critique of Taylor (1984) on Rozanski and Sonntag (1982), and the very valuable study by Gedzelman (1988) to analyze Taylor's (1972) data set. Maybe a conclusion of your study could be a critique rather than a statement of consistency with the Rayleigh model?

2. The interpretation of the stratocumulus case study as evaporating cloud drops needs further corroboration. The literature cited on the kinetic fractionation of evaporating droplets, such as Stewart (1975) considers rain drops, which are orders of magnitude larger, and have a vertical downward motion. This provides the potential of partial evaporation, leading to a kinetic fractionation signal in the d-excess. Cloud drops, however, will rapidly evaporate completely as they become unstable at smaller radii, leaving no sign of kinetic fractionation in the d-excess. Furthermore, they are suspended in the atmosphere, thus allowing for equilibrium fractionation. Without the consideration of rain evaporation in addition, or other processes, the explanation of the STC case is thus not viable. What is the potential of the ocurrence of rain evaporation from these stratocumulus clouds? Can

you estimate how much cloud water specific humidity would have to be added to produce the negative d-excess signal, and is this consistent with the saturation specific humidity in the cloud layer? Could there potentially have been a potential for rain evaporation or ice processes in the airmass upstream earlier instead?

3. Section 3.3 (DBL case) describes and interesting case of BL development, but is particularly difficult to follow. Consider reorganising the material in a more logical order, and formulate a clearer take-away in the end.

4. The material included in the Supplement only seemed marginally useful. It is also confusing to have both an appendix and a supplement in a manuscript. Consider merging the supplement into the Appendix, keeping only Fig. S1.

5. It is difficult to follow the description of the cases without additional context of the flight situations. Maybe Fig. 3 could be split up for each case, and supplemented by a weather chart similar to Fig. S6.

6. The discussion sections 4.3 and 4.4 appear repetetive to what has already been presented earlier an can be deleted.

7. The second half of the first paragraph of the Conclusions section should be moved to the introduction/motivation.

8. Important studies that should be referred to are Taylor (1972, 1984), and He and Smith (1999) and Ehhalt (1975, 2005) and Tsujimura et al., 2007.

9. I found the structure of the manuscript somewhat confusing, in that first the flights around both Washington DC and Indianapolis are presented, and then almost all analysis focuses on flights from only one site, before returning to a wider overview in section 3.4. It would be more logical for the reader to move Sec. 3.4 ahead of the case studies, and then zoom into individual aspects. Consider adding a more

intuitive way to present an overview of all data than a comparison to Rayleigh in 7.

10. Several times in the writing, wording such as "Fig. x shows ..." is used. For conciseness, consider rephrasing this to sentences talking about what is shown in the figure, added by a figure reference "(Fig. x)".

2 Detailed comments

P3, L8: "However, relatively few...": As far as I am aware there is only one published dataset of airborne d-excess measurements. Why the citation of Schmidt et al., 2005 here?

P3, L34: Please state if the inlet or parts of it were heated.

P4, L9: How sensitive are the measurements to variations of cavity pressure and temperature during flight?

P9, L21: Worden et al. 2007 formulate their models for rain evaporation (falling condensate), not cloud evaporation, see their supplementary information.

P10, L10: "Rayleigh-consistent observations": Consider rephrasing in light of major comment 1

Fig.4: Consider adding a panel that shows more mixing lines, e.g. between the bottom and top of the inversion, or the bottom and top of the BL, how do these compare with the complete mixing line?

P12, L1: Consider using a standard symbol such as q or m for specific humidity or mixing ratio of water vapour, rather than H_2O.

P14, L8: "relative to Rayleigh" rephrase as e.g. "Rayleigh model predictions"

P14, L16: "d-excess switches to tracking" rephrase

P14, L13-22: hard to follow, rewrite this section for clarity

P16, L6: "Differences" - difference to what is shown? Are these not absolute values?

Fig.6: How are RL and INV defined? You could provide a quantitative comparison of averaged quantities for both layers that supports their distinction.

P17, L14: Not clear what the take-away from this section is. Revise paragraph.

P17, L17: "Figure xxx shows": revise according to major comment 11

P18, L12: Not clear what the take-away from this section is. Revise paragraph.

P18, L16: Why the citation of Lee et al. (2006)? Compare Gedzelman (1988) and the discussion of the Taylor (1972) data (major comment 1).

P19, L3: The thermodynamic characteristics of the profiles are not consistent with a Rayleigh processes, even though the isotope composition may be - what is the conclusion from that finding?

P19, L15: How would cloud droplets be lofted but not the surrounding vapour? If vapour and droplets move together, how can non-equilibrium fractionation result?

P20, L16-20: Consider the possibility of rain evaporation upstream (major comment 2).

P21, L10: What relative roles could vertical motion and entrainment of dry air from aloft play in the evaporation of the cloud layer?

P21, L20-29: Rephrase for clarity. Could ice-phase processes have been relevant further upstream, earlier in time?

P22, L12: What is the Rapid Refresh Model? Could be deleted here.

P22, L7-34: reorganize and rewrite this section for clarity

P22, L36: "so we look" rephrase

Fig. A1: What is the reason for the large scatter between both instruments in the mid-range of humidities? Does longer averaging of the data provide a better agreement? Was the scatter similar for each of the flight? How is the scatter for the downward profiles only?

3 References

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