Interactive comment on "Drivers of the variability of the isotopic composition of water vapor in the surface boundary layer" *by* Jelka Braden-Behrens et al.

Anonymous Referee #2

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In this manuscript, the authors present a dataset of the isotopic composition of water vapor over a forested ecosystem. They combine the measurements of the water vapor with eddy covariance derived estimates of the isotopic ratio of the ET flux. The goal was to test a fundamental hypothesis that the isotopic ratio of water vapor above an actively transpiring surface should respond to the ET flux. Over large scales (i.e. from satellite data) it has certainly been shown that the land surface fluxes of water vapor influence the isotopic ratio of the atmospheric water vapor. The authors conclude that the ET flux has minimal influence on the isotopic ratio of vapor. On diurnal timescales, entrainment drives a midday depletion in the water vapor isotopes that is in opposition to the flux of ET. On seasonal timescales, entrainment rate does not predict the isotopic ratio of the

vapor. Rather, it is some combination of processes (ET, rainout and temperature) that collectively influence the isotopic ratio of the vapor.

Firstly, I commend the authors on a very nicely developed dataset and some rather sophisticated analysis of the data. Secondly, I think the question that is posed is interesting and worthwhile particularly to the extent that using water isotopes to trace water fluxes and close hydrological budgets in the atmosphere has a lot of potential in diagnostic analysis of GCMs and transport models. This work contributes to these efforts. However, I found the analysis, on the one hand, to be unnecessarily complex at times (i.e. there were many competing correlations between derivatives) but also overly simple at others (i.e. trying to use a single linear regression model to predict d18Ov). In the end, I think the authors overlooked some simple tests that could have been useful and drew conclusions regarding why d18O/dDv correlated with temperature that are not correct. I would support publication after significant changes are made to the writing and perhaps some additional analyses.

The authors find that entrainment is the prominent driver of the diurnal cycle in d18Ov except in the morning when transpiration has more of an effect. This finding has been very clearly identified in previous works. See for example: doi.org/10.1002/jgrd.50701 as well as numerous other citations the authors provide. It would seem therefore that the authors should not have been surprised to find this to be true. It would have been surprising, in fact, to find the opposite to be true. I this comment is significant because it affects the entire tone of the paper. The authors should have begun from the perspective that entrainment is the primary driver of diurnal cycles and then sought examples where the effect of ET emerged.

The authors find that the correlation between entrainment rate and the seasonal cycle in d18Ov is weak. They therefore conclude that entrainment is not the critical driver of the seasonal cycle. However, they fail to identify that it is not just how much vapor is entrained but the isotopic ratio of the water vapor that is entrained. With synoptic scale changes in atmospheric circulation the isotopic ratio of water within the free tro-

posphere changes. It would seem quite clear, and maybe I misunderstood this from the manuscript, that it is the isotopic ratio of the free troposphere driven by large scale circulation that drives changes in the midday isotopic ratio above the canopy. Analysis using a lagrangian transport could be deployed (as with many previous isotope studies) to identify how the source of vapor changes and whether it is the source region that explains the seasonal changes.

The authors find a strong influence of temperature on d18Ov and call upon a rather confusing role for temperature influencing the fractionation of ET. I find this extremely unlikely. If this was the case, then there should be a very strong relationship between deltaET and temperature. I believe deltaET is more strongly influence by RH or VPD and or LAI. Revisiting comment #3, changes in synoptic circulation drive both changes in temperature and the d18Ov. The temperature of air masses affect how much rainout has occurred and give rise to a strong relationship between d18Ov and temperature. This is in fact the rationale for why ice core d18O values reflect temperature. I think explaining the relationship between d18Ov and temperature would have benefited from taking a more "first principles" approach and yielding to extensive research already done on this topic.

The calculation of isoforcing relied heavily on the estimates of PBL height from reanalysis. This concerned me somewhat because there was no good validation of these estimates and it seems the estimates from reanalysis would only be useful if the land cover in the area was homogenous. In other words, is the forested cover of the site representative of the conditions with the reanalysis grid cell? The authors discuss error estimates of PBL height but it was not clear how these error values were assimilated in the analysis. Secondly, the authors note that their assumption that the isotopic ratio of water vapor is well mixed is likely incorrect. This has been shown by other studies using gradient and flux gradient approaches. What are the effects of this assumption on the isoforcing estimate? What if the authors assumed a gradient with log form up the top of the PBL using previous studies? My point is that if the authors know this

assumption is incorrect it would be valuable to assess the impact of this assumption on their analysis using a sensitivity approach.

I was surprised come to the end of the paper and never see a figure or actual discussion on the estimates of delta ET. The estimates of delta ET were assimilated into numerous analyses but, after all, if the study is looking at how delta ET affects delta V, the readers should see delta ET. The authors need to present this data and analyse it directly before using it in more sophisticated approaches. How does delta ET vary through the season? Was it affected by soil moisture and VPD that might change T and E partitioning? Did delta ET relate to total ET rates or greeness/LAI? Does it change after rainfall events? An analysis of the drivers of delta ET are a necessary complement to the other analyses presented.

Small comments:

When a variable is introduce the correct grammar (I think) is like this... "Temperature, T, is related to latitude." Or "Temperature (T) is related to latitude."

28: Unclear why sublimation of snow was listed under "precipitation removal" processes. This would be a surface flux process.

54: The R2 value between C and d18O/dD were just listed in the previous paragraph so this sentence felt redundant.

60: Lots of other studies over forests not considered here: Continuous measurements of atmospheric water vapour isotopes inwestern Siberia (Kourovka) Stable Water Isotopes Reveal Effects of Intermediate Disturbance and Canopy Structure on Forest Water Cycling Response of water vapour D-excess to land–atmosphere interactions in a semi-arid environment I would say broadly that the literature available on this topic was under-cited.

145-155: This extended quotation from ERA5 manual is not appropriate. The authors should explain the process of error estimation in their own words. As noted above, it is

also unclear how this error was assimilated in the analyses that follow.

163: Missing "space" before the sentence begins.

170 "site" not "cite"

171: "However" is the wrong word here because this sentence does not contradict the previous one it supports it.

176: The comma should be after "h" not after "both"

178: if the nighttime data is not meaningful, I would recommend excluding it. As you note, when the value approaches 0, the equation becomes very unstable.

181: When you write ddv/dt is this dt_iso or dt_meas. Truthfully, I found the comparisons between the many derivatives quite hard to follow and perhaps not the most useful way to analyze the dataset. Figure 1: Standard error should be reported around composite diurnal cycles.

193: "being"

206: I was confused as to what the authors mean by Rayleigh distillation in this context. Is this condensation onto the surface such as through dew or is this the collective rainout of the air mass as it ages from its origin?

206: Also, because all of these processes are important to the hydrological balance, it would seem that linear univariate models are not really appropriate or useful. Perhaps multivariate non-linear models would be better suited for partitioning the relative controls.

207: "between"

208: missing closed parenthesis at end of paragraph.

209: Earlier you discuss the inlet being 10 m above canopy but here you say 7 m. Not a big deal but better to just be consistent.

Figure 6 and associated discussion on Rayleigh Distillation: The assumption that a single distillation model (i.e. a linear fit to d18O vs. log(C)) assumes that a common source but experiencing different degrees of rainout. This is not true. So you could really have multiple plausible distillation models that would give rise to "messier" scatter plot of your data.

253: How does delta ET relate to precipitation? This could give you some insight into the fractionation of ET relative to the source. Does it change during the year?

261-262: The authors write: "In general, the correlation between temperature and v might be linked to temperature dependent fractionation at the sites of evaporation." What are the sites of evaporation being referred to here? Local ET? Nearby lakes that might supply atmosphere? The ocean source?