AUTHORS COMMENT: ANSWER TO REFEREE 2
'Drivers of the variability of the isotopic composition of water vapor in the surface boundary layer'

Referree comments: black,
Author comments: blue
Changes to the manuscript: green

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.

Authors response: We thank the anonymous referee for the motivating, detailed and constructive feedback to our manuscript. We understand these general comments on statistical analysis and on the conclusion about the influence of temperature. In the revised manuscript, we added a multivariate regression with fewer variables and drew conclusions based on this. We performed the multivariate regression to reduce the Akaike information criterion (AIC) using a stepwise backward-forward approach. Further, when analyzing simple linear correlations, we focus on fewer variables in particular regarding ET and separated the δD analysis from the $\delta^{18}O$ analysis in Table 3. However, we still include some correlations, e.g. correlations to IF, to compare with literature data. Concerning the conclusions, in particular about temperature, we tried to be clearer about what we conclude based on the data and suggestions for potential explanations that would need additional measurements. Below we answer the referee's comments in detail.

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.

Authors response: Thanks for pointing this out. We added doi.org/10.1002/jgrd.50701 to the discussion of our results:

'This is consistent to the predominant influence of entrainment on the diurnal cycle that has been found by other authors at different field sites (c.f. Lee2007, Griffis2010, Lai2011,Berkelhammer2013).'

We also changed the writing to make it clearer that an influence of entrainment might be expected and our purpose is to QUANTIFY the influence of ET compared to the measured changes in δ_v. e.g. we write:

- 'Our objective is to quantify the influence of local ET on δ_v in the SBL close to the canopy of a forest ecosystem.'
- '[We] use our direct measurements in combination with PBL heigt h to quantify the influence of ET on δ_v.'
- 'We quantify the influence of local ET on the isotopic composition of the boundary layer by making a quantitative thought experiment. How would local ET influence the delta value of the PBL (δ_v) if local ET would be the only process that (significantly) influences δ_v?. To answer this question, we use isoforcing values, that are based on EC measurements of the magnitude of ET F_{ET} and its isotopic composition δ_{ET} (see Braden-Behrens2019).'

We hope that this way it is a bit clearer. Please see also the whole section 'Calculation of evapotranspiration-related change in δ_v' of the revised manuscript.

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 troposphere 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.

Authors response: We agree, that the isotopic composition of entrained vapor is an important driver and added this to the discussion of seasonal variability:

- 'Concerning entrainment of isotopically lighter air from the free troposphere, the amount of entrained air can vary as well as the isotopic composition of the entrained air which can be studied using lagrangian transport models (see e.g. Aemisegger2014,Pfahl2008).'

We also agree that lagrangian transport analysis would be an appropriate tool to identify changing water sources but this would be beyond the scope of this study. However, we added this to the discussion.

- 'In general, the positive correlation with temperature-related quantities implies that changes in synoptic circulations might be relevant drivers of δ_v. We propose that such changes could be studied by using lagrangian transport models, as have been carried out at other field sites (see e.g. Aemisegger2014, Pfahl2008) and a further analysis of their relation to temperature at the field site might explain parts of the observed variability.'
- 'As the temperature effect is related to the origin and history of air masses (Dansgaard1964, Ambach1968), we propose that lagrangian backtrajectory models would be a useful tool to understand the processes that drive the temperature effect.'

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.

Authors response: Thanks for pointing this out. We agree that the interpretation of the correlation to temperature might have been misleading. In particular because changes due to the variability of δ_{ET} should be also included in $\frac{d\delta_v}{dt}|_{ET,est}$, the ET-related change in δ_v. Thus, the observed correlation to temperature cannot be explained with changes in T, RH or VPD that yield changes in delta ET. In the revised

manuscript, we removed the misleading interpretation of temperature influencing fractionation.

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.

Authors response: Thanks a lot for pointing this out so clearly. We agree that the interpretation of the correlation to temperature might have been misleading, in particular as that synoptic circulation potentially influence both, temperature and delta values. However, we want to point out, that the so called 'temperature effect' is a result of complex processes and our focus was to quantify the influence of local ET, whit we did by analyzing $\frac{d\delta_v}{dt}|_{ET,est}$. We agree, that the (complex) temperature effect is a better explanation for the observed correlation to temperature and include this to our interpretation of the correlation to temperature throughout the manuscript. We changed different parts of the manuscript and now write:

- 'We conclude that the observed seasonal variability of δ_v is neither dominated by Rayleigh processes, entrainment nor local ET but likely linked to other temperature-related processes such as changes in synoptic circulation.'
- Potential processes that could drive the observed seasonal variability of δ_v are local ET, cumulative rain-out (Rayleigh distillation) and changes in synoptic circulation.
- At the seasonal time scales the cumulative rainout of an air mass as it ages from its origin (e.g by Rayleigh destillation) is a major driver of the variability of δ_v. This is a complex process that influences δ_v via the origin of air masses (Ambach1968), the thermodynamic conditions during cooling (see e.g. Dansgaard1964), fractionation during condensation, isotopic exchange between rain drops and the surrounding air and evaporation from rain drops (see e.g. Dansgaard1964). These complex processes yield the 'temperature effect', a positive correlation between condensation temperatures and higher δ-values of precipitation (see e.g. Dansgaard1964).
- A large fraction of 50% of the observed seasonal variability of δ_v is linked to temperature, indicating a considerable influence of the complex processes that drive the so-called temperature-effect.

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?

Authors response: We thank the anonymous referee for this comment. This aspect was indeed missing from the manuscript, so we have now mentioned the landcover and the representativity of the grid cell for the study site in the revised text. Indeed, while the tower itself is situated in a forest, this forest patch is only a couple km wide, and the landscape at a larger scale is a mixture of such forest areas and agriculture. The spatial scale of the forest is such that the boundary layer formation will be routinely driven not only by the forest patch but by the larger mixed landscape. Comparing the land cover and the relative fractions of forest and agriculture for the ERA5 reference gridcell relative to the relevant area around the study, we find that the grid cell is representative for the study site. We have added this point to the revised text:

'The land cover within this grid cell is characterized by a mixture of forested patches and agricultural land, with a relative contribution of about 50% each. A similar mixture of forest and agriculture is found at the study site at a spatial scale relevant for the formation and growth of the PBL. Due to the proximity and the comparable land cover of the entire grid cell and the area surrounding the tower at the measurement site, we consider the boundary layer height estimate of the grid cell to be representative of conditions at the study site.'

The authors discuss error estimates of PBL height but it was not clear how these error values were assimilated in the analysis.
Authors response: Unfortunately, we do not have error estimates for all datapoints, but only for a subset of datapoints. Thus, we show the discussion of uncertainties and not propagate those uncertainties further. In the revised manuscript, we added more information about the PBL-heigt data source and its uncertainties for the uncertainty or delta_ET we refer to the previously published manuscript on https://doi.org/10.1016/j.agrformet.2019.01.035, that analyse the uncertainty of delta_ET as a function of the magnitude of ET.

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.
Authors response: This is a very good idea, but we do not have the necessary data/boundary conditions to perform such an analysis.

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.
Authors response: Our measurements of delta_ET is presented and discussed in a previous paper https://doi.org/10.1016/j.agrformet.2019.01.035. This manuscript focusses on using this data to better understand δ_v , so we do not want to present the same data twice. However, we agree that changes in delta_ET are important, thus we added a brief summary of the results concerning delta_ET of our previous paper to this manuscript and refer to it for further details.

- […] the diurnal cycle of δ_{ER} does not dominate the diurnal cycle of IF. These diurnal cycles are shown in a previous manuscript (see Braden-Behrens2019). In brief, δ_{ET} rose throughout the day, indicating non-steady-state conditions both δ values and over all seasons, except for δD_{ET} in summer.
- In brief, δ_{ET} spanned a range of -19 to 0 permil for $\delta 18O_{ET}$ and of -140 to -25 permil for δD_{ET}. with a complex seasonal shape and larger uncertainties for smaller ET, (see Braden-Behrens2019 for details).

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.”
Authors response: Thanks for pointing this out. We try to change this throughout the manuscript.

28: Unclear why sublimation of snow was listed under “precipitation removal” processes. This would be a surface flux process.
Authors response: We removed it.

54: The R2 value between C and d18O/dD were just listed in the previous paragraph so this sentence felt redundant.
Authors response: This sentence refers to the study of Griffis 2016 does not use R^2 values, but a different method. We changed this sentence to be more clear: ‘In particular, even at a height of 185 m above a crop/grassland canopy, (Griffis2016) estimate the relative contribution of ET to range from 0 to close to 100%,

with a median of 34% based on a two-end-member isotope mixing model (Griffis2016)'

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.
Authors response: We added these studies to the discussion, in particular to Table 2.

145-155: This extended quotation from ERA5 manual is not appropriate. The authors should explain the process of error estimation in their own words.
Authors response: This extended quotation from ERA5 manual has been replaced in a complete reworking of this section on PBL height.

As noted above, it is also unclear how this error was assimilated in the analyses that follow.
Authors response: Please see our comment above.

163: Missing "space" before the sentence begins.
Authors response: We changed this.

s "site" not "cite"
Authors response: We changed this.

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

176: The comma should be after "h" not after "both"
Authors response: We changed this.

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.
Authors response: Yes, we excluded it from the analysis. We added a sentence about this to the manuscript.

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.
Authors response: In the revised manuscript, we changed the notation to be more clear. We also added a more detailed description about the purpose and the underliing assumptions of the ET-related estimate $\frac{\mathrm{d}\delta_v}{\mathrm{d}t}|_{\mathrm{ET,est}}$. We now only use $\frac{\mathrm{d}\delta_v}{\mathrm{d}t}|_{\mathrm{ET,est}}$ for the estimated quantity and $\frac{\mathrm{d}\delta_v}{\mathrm{d}t}|_{\mathrm{meas}}$ for the measured quantity to avoid this confusion. Additionally, we changed the table that presents the different regressions to be more clear (using less parameters) and changed the analysis to a multilinear regression: We performed the multivariate regression to reduce the Akaike information criterion (AIC) using a stepwise backward-forward approach.

Figure 1: Standard error should be reported around composite diurnal cycles.
Authors response: Unfortunately, we do not have uncertainty estimates for all datapoints and quantities.

193: "being"
Authors response: We changed this.

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?

Authors response: We changed this sentence to: 'Potential processes that could drive the observed seasonal variability of δ_v are local ET, cumulative rain-out (Rayleigh distillation) and changes in synoptic circulation.'

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.
Authors response: Yes, we changed the statistical analysis a multivariate regression to reduce the Akaike information criterion (AIC) using a stepwise backward-forward approach. We are aware, that by this we cannot fully model the isotopic composition, but our goal is only to identify potential drivers. For this purpose, we decide for a linear model instead of a nonlinear model, because we want to avoid overfitting.

207: "between"
Authors response: We changed this.

208: missing closed parenthesis at end of paragraph.
Authors response: We changed this.

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.
Authors response: We changed this to 7m throughout the manuscript.

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.
Authors response: Yes, we added this to the discussion:

- 'However, it is worth to point out, that this discussion of Rayleigh destillation is based on the assupmtion of one single destillation model. Thus, some of the additional variability in the relationship between δ_v and log(C') in Fig. 6 might also be explained by multiple distillation processes.'

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?
Authors response: Please see our comment above: We added a brief description of the seasonality (and uncertainty) of delta_ET to our manuscript.

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?
Authors response: After reading your general comments about synoptic changes, we removed this part of the discussion, because we agree that this is unlikely. We removed this misleading interpretation from the manuscript and thank the anonymus referee for pointing this out.