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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 #1

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General comments: This manuscript presents the isotope ratio data of atmospheric water vapor (d18Ov and dDv) above a managed beech forest in central Germany. Together with EC measurements, values of d18O and dD associated with ET fluxes (dET) were also reported for a full growing season. The primary objectives of the study are to assess factors that are responsible for the observed variation in d18Ov and dDv. The authors used a simple linear regression to seek for correlations between d18Ov and dDv variability and an isolated variable and interpret their results on the basis of regression statistics (R2 and p-values). As far as I can tell, the experiment was properly carried out and the data were carefully scrutinized and high quality. The topic is interesting to a broad audience especially to the stable isotope community and researchers

who study ecohydrology. The measurements will contribute to a growing number of water vapor data collection, though a mechanistic interpretation of water vapor isotope data in the surface boundary layer remains challenging. The biggest issues I have with this manuscript are on the structure, assumptions made for the proposed problem, its statistical analysis and the interpretation of linear regression. The authors should consider addressing these major comments before its final publication.

Specific comments on major issues: 1. This manuscript has two major purposes, 1) to demonstrate the ET fluxes do not dominate d18Ov and dDv and 2) to evaluate potential factors that control d18Ov and dDv variability on both diurnal and seasonal time scales. For the 1st objective, the authors treat the PBL as a box, and assume surface ET is the only flux component that contributes to volume of the box (or the diurnal evolution of boundary layer height) while neglecting horizontal advection and entrainment fluxes. They used 'isoforcing' associated with the ET fluxes, combined with PBL heights retrieved from ECMWF data product, to calculate dv/dt (for both 18O and D) over the course of a day (Eq 2) and compared the results to the diurnal pattern from the time series measurements (Fig 2). Applying Eq 2 in this context is flawed as the height of the PBL cannot grow without entrainment (even if horizontal advection can be assume negligible under certain conditions) . Assuming surface flux as the only flux component while applying a changing height (h) contradicts one another. This is a misinterpretation of the boundary layer budget, and it is no surprise that calculated and measured dv/dt diurnal pattern has nothing in common (Fig 2). Secondly, why would the authors even bother to do this exercise? As later stated by the authors (ln 189-190) "A discussion of the influence of local ET that is purely based on isoforcing IF overlooks the influence of boundary layer mixing processes." The authors later stated that "the concurrent trends in the diurnal cycles of CH2O and dv indicate, that entrainment dominantly influences dv from the forenoon to the afternoon:" and ". . . we observe this indication for a dominant influence of entrainment from the forenoon to the afternoon also in summer." If you can make these conclusions from the observation (which the authors did), why trying to prove (and did it incorrectly) something that the data have already shown? This whole section should be scratched in my view. 2. For the second objective, the authors identify 4 potential factors that influence seasonal availability of dv: local ET, Rayleigh distillation, selective water use by plants and temperature. The author applied a simple linear regression between dv and each of these factors to look for correlations. There are several problems in the statistical analysis used by the authors. First, the authors should distinguish processes from state variables. Secondly, these factors are not independent from one another, for example, ET and Rayleigh distillation are both temperature dependent. A simple linear regression ignores the interactive effect between processes and state variables. Ideally, one should carry out a full BL budget calculation with a numeric model that considers thermodynamic isotopic fractionation. At the very least, the authors need to consider a multivariate regression that considers the interactive effect among variables. A simple linear regression is inappropriate. 3. The authors use sloppy statistics. This manuscript reports incredibly small p-values (10ˆ-35) that are simply not meaningful. The p-value is calculated from the data and depends on the sample size (number of data points). It is possible to get p values to the -35 decimal points but that is simply because of we have the computing power to do this. More data points give you smaller p values. The bigger issue is, is the p value reliable? The p values shown in Table 3 are simply not meaningful. The difference in the p values between all times and period of green leaves is likely an artifact of sample size. Some statisticians have urged not to use p values but to use other alternative statistical matric because it is too often misinterpreted (see Halsey 2019 http://dx.doi.org/10.1098/rsbl.2019.0174). This study is another example of why. The authors should limit reporting p values to a more reliable estimate. 4. After redo the statistical analysis, the authors must re-evaluate their interpretation of the results and draw proper conclusion accordingly.

Technical comments: Ln 25. Do you mean a major driver of dv variability? Why the remove of precipitation only acts on seasonal time scales? Ln29. Your description of the amount effect is very crude and can cause confusion. Please be more elaborative on the amount effect. Ln37-38. It's unclear what 'different importance' means based on

R2 values; this sentence is hard to read. It's easier to see the effect by a state variable (such as temperature) but it becomes harder to visualize by a process (like Rayleigh rainout). Can you explain how Rayleigh process may differ seasonally that in turn affect seasonal variability of dv? Ln51-52, a correlation does not suggest a causal effect; maybe that was not what you meant to suggest but the writing makes it seem that way. Ln61-62, consider revise this sentence to "Only one of these studies performed direct dET measurements in a forest". Ln99 pls provide more details on how exactly dET was calculated. Did you perform a spectral analysis to examine potential loss of energy due to the differences in the sampling frequency between EC and isotope measurements? Ln123 More precisely speaking, VPD is calculated from temperature and RH data which were directly measured. Ln128. Can you give a brief description on how the rain sampler is designed to store its water to prevent evaporation? Ln135. avoid jargon; just say using ECMWF data product Ln145 delete this sentence; rework this paragraph. Rather than copying from the manual, it would be more useful to describe how you retrieve PBL h from IFS. Ln179 what is the time unit here? Is this 0.1 permil per second, per minute or per hour? Assuming 0.1 permil per hour, from 8am to 5pm, dv would've increased by 0.9 permil d18O and almost 10 permil dD. But Fig 1 shows a decrease in d18O by $\sim$ 1 permil while a decrease in dD by $\sim$ 5 permil. How do you reconcile the inconsistency between these results? Ln180-185 As stated above, this conclusion is flawed as the calculation was based on an invalid assumption of no entrainment while BL h is allowed to grow. Ln186-194 These remarks acknowledge the authors have known the answer from the observation but still decided to use a reverse logic to disapprove something they already knew could not be true. Hmmm interesting . . . Ln195-200 I found this section puzzling. TKE is a measure for the intensity of turbulence. h is most commonly defined by an inversion in potential temperature and dewpoint and is often estimated by radio sounding or lidar. It does not make sense to make direct comparison between TKE to PBL h (yes, they are both part of the boundary layer dynamics) because there is not a causal effect between the two. Simply presenting correlations without context is meaningless (if seeking covariation is the goal, why not presenting correlations with other meteorological variables? why do you choose to only present TKE? I would suggest removing TKE altogether. Ln204 -12 permil for dDv? Is this a typo? Ln206 Shouldn't selective water use by plants be included in ET? Ln209 some would argue 7m above the top of the canopy is pretty far out; it is likely outside the subsurface BL near the forest canopy. Since you mention TKE, why don't you show a profile of vertical wind speed and momentum fluxes? It will give you an idea if your sensors are within the canopy subsurface BL. Ln222-223, do you have an explanation of why you found a correlation between dET and dDv but not wit d18Ov? Ln225-234 these interpretations are based on flawed stats Ln238. Bowling et al. 2017 is not an appropriate citation here. Remove. Fig 7. Right panel: after leaf coloring - was that diamond or cross symbol? Ln243-248 & Fig7. Are GMWL and LMWL statistically different? I am skeptical that you can use GMWL and LMWL to contrast impacts by far-field v.s near-field factors. Ln254. entrainment is a diurnal process. Why would you expect entrainment be a factor on seasonal time scales in the first place?

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