Reply on RC #2

Review of “First Eddy Covariance Flux Measurements of Semi Volatile Organic Compounds with the PTR3-TOF-MS”, Fischer et al., AMT (2021)

This paper presents observations of concentrations and EC fluxes of several biogenic VOC above a boreal forest. Measurements are acquired with a new instrument that has much higher sensitivity than its predecessors. They also use a novel inlet design that minimizes contact of sample gas with instrument surfaces. Measurements from a 3-week period in the spring are analyzed to demonstrate instrument performance and highlight the potential of the new capabilities. The paper is generally well written and the number and style of figures is appropriate. Publication is recommended after consideration of the following, mostly minor, critiques.

General Comments

Sections 4.5 and 4.6 deal with data quality and uncertainties. Possibly it makes more sense to present these before the discussion of specific fluxes in Sect. 4.1-4.4.

The sections were moved to the experimental chapter and reorganized, as also requested by reviewer #2.

Specific Comments

L87: It is stated that the inlet mounting was meant to minimize flow distortion. And later on L145, it is stated that the “anemometer deviation” is < 2 cm/s. What does this mean, precisely, and how was it determined? Were any tests carried out to confirm that flow distortion does not influence fluxes (e.g., comparing heat and momentum spectra/cospectra with the inlet flow on/off)? Also, is there potential systematic bias associated with such large sensor separation?

We estimated the flow distortion at the sonic anemometer assuming that the flow is drawn into our inlet homogeneously from all directions. The air velocity through the surface of a corresponding sphere was calculated. We mention this now in the new version of the manuscript. We did not do spectral comparison with the inlet blower on and off, but since we expect the distortion to be a constant offset in the vertical component of the wind data, it is removed automatically by the wind tilt correction.

The flux attenuation effect of a purely vertical sensor separation is discussed by Horst et al. (2009). Their figure 11 shows measured flux deviation versus z/z’ with z being the anemometer height and z’ the scalar sensor height. Our inlet is 15m above the zero-displacement height with the anemometer positioned 0.5m higher, resulting in z/z’=1.033. Attenuation is smaller in this arrangement than with the anemometer situated below the scalar sensor, and neglectable for our z/z’ compared to other uncertainties discussed in the manuscript. We added a short reference to Horst et al. (2009).

Horst, T.W., Lenschow, D.H. Attenuation of Scalar Fluxes Measured with Spatially-displaced Sensors. Boundary-Layer Meteorol 130, 275–300 (2009). https://doi.org/10.1007/s10546-008-9348-0

L119 – 121: This sentence might be more appropriate for the introduction.

This section was extended and is now part of the introduction as proposed also by reviewer #1.

Figure 2: An actual photograph of the inlet/sonic setup, if available, would be helpful.

We added a picture showing the inlet tube and the sonic anemometer to figure 2.

L274 – 278 and 295 – 303: This is stated in the introduction and does not need to be reiterated here. Deletion or shortening suggested.

The sections were shortened and merged with the corresponding sections in the introduction as proposed also by reviewer #1.

L372: Is this a lower limit for the Bcary emission rate, since it does not account for deposition of the oxidation products?

The section was partly rewritten, since we initially oversimplified the estimation of the reaction time and neglected in canopy reactions producing oxidation products. In the revised version we reduced the discussion ofreacted Bcary. We see variations in reacted Bcary to total sesquiterpene flux ratio following ambient temperature. We speculate about in-canopy deposition losses, which could be a possible explanation of our observations.

L388 – 392 – all of this description is more appropriate for the figure caption. Discussion here should focus on what the figure means, not the visual elements.

The text was changed to describe more results and less figure details.

Figure 6: It would be helpful to see this type of plot for all the fluxes (and maybe diel average mixing ratio, too?).

We added a figure showing the discussed compounds (except of the unemployable monoterpene). It is placed in the appendix, since no new information is presented by the alternative representation of figure 5.

L423 – 426: The exact same sentences, or nearly so, appear earlier in the manuscript (295 – 303).

The duplicate was shortened here and in the monoterpene section to avoid repetition, since most is already covered in the introduction.

Sect. 4.5: It would also be instructive to see the power spectra, either in the main text or supplement.

*Spectral analysis of our mass spectrometer data shows white noise. We suspect its origin in the Poisson counting statistics of the detector for each sample and quantization noise due to the low count rates. Antialiasing due to the short residence time in the drift tube compared to the sampling rate could also be a potential source. In the plot of f*S against frequency, this noise contributes a lot to the mid and high frequency range of the power spectra depending on count rates, but it is uncorrelated to the vertical wind component. Cospectra and the covariances used for flux calculation are not affected by this out of phase noise. Therefore, power spectra will not aid in assessing high frequency damping of the covariance and are not shown. Here we give an example averaged from May 3rd to May 8th showing vertical wind, virtual temperature, isoprene, sesquiterpene and BCARY-O3. The spectra were normalized for better comparability.*

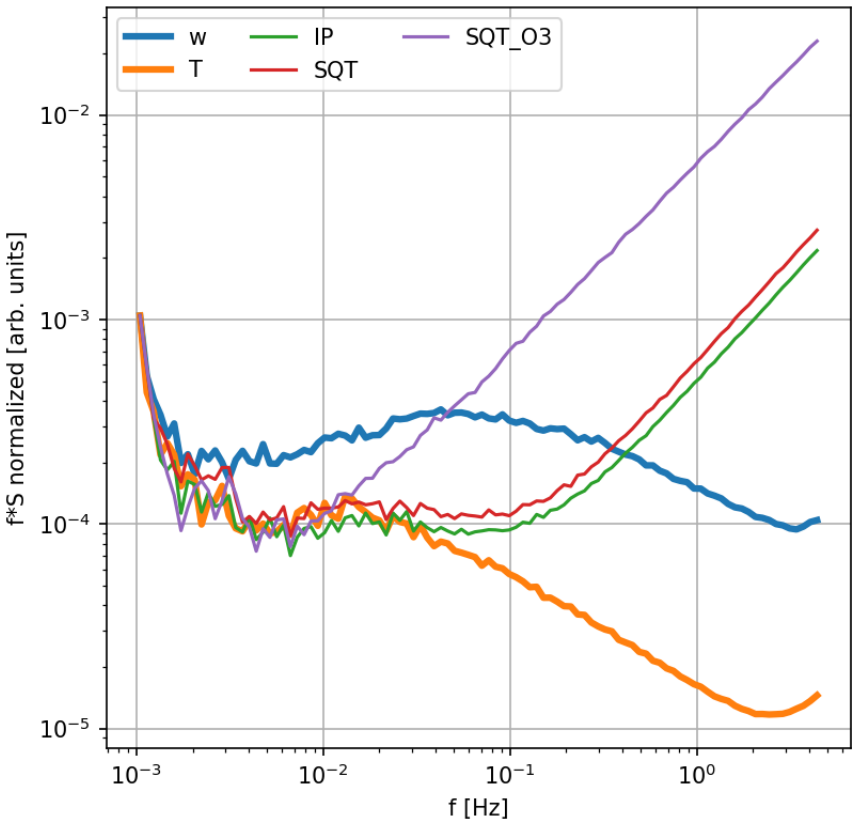

Figure 8: Would it be possible to add the w' – temperature lag correlations for comparison? These lag peaks seem very wide.

The requested w'T' lag correlation was added to the plot and it looks similarly wide. The only obvious difference is the missing lag time of the maximum correlation, which was to be expected since both w and T are measured by the same instrument.

Technical Comments

L33: "critical" ✓

L51: "predicted that" ✓

Figure 1: I am somewhat confused by the cartoons. What is the "measurement container" box meant to show? And the "inlet box", which seems to appear opposite of the stated inlet orientation?

The white boxes indicate the orientation of the measurement container and the attached inlet structure to show why the red wind sector had to be excluded from analysis. The figure caption was clarified.

L102: "empirical" ✓

L105: "expected" ✓

L149: What is the brand or manufacturer of the blower?

The manufacturer of the blower is unknown, it was scavenged from an older setup without its housing.

L351: it seems like pmol would be a more convenient unit here. ✓

Figure 4: why are concentrations shown in cps instead of pptv or similar mixing ratio units?

The histograms were re-calculated with calibrated time traces and show pptv now.

L421: "of the number of averaged samples" ✓