The investigation by Dumont et al. at the ICOS Vielsalm station presents a unique dataset of long-term observations of concentrations and eddy-covariance fluxes for BVOCs and ozone. The study is unique in the scale and duration of BVOC flux measurements, spanning several years with observations both above and below forest canopy, and also thorough its presentation of methods and analysis used for preparing the dataset. Combined, this study and the dataset collected is a significant contribution to BVOC research, providing valuable data to the modelling community, and should be accepted for publication following minor revisions. Of my comments, the main area where the manuscript would benefit perhaps most substantially concerns the presentation in the text of VOC-related signals vs identified VOCs (for both concentration and significant ecosystem exchange), particularly in Figure 6 and Table 3. The distinction between detected ions and identified VOCs needs to be clarified (in both the Figure, Table, and their captions) to prevent ambiguity and potentially misinterpretation of the presented results.

Specific comments:

Line 7: While mentioned in the title, it would be a slight improvement to the Abstract if the type of forest - "mixed temperate" - investigated is mentioned earlier in the abstract text (perhaps near where the ICOS Vielsalm station is introduced), rather than at the end.

Line 27: There are many different types of forest ecosystems, it would be an improvement to have a more additional introduction material regarding what is known about the BVOC emissions from the type of temperate mixed forest that the study site represents.

Line 38: cite reference(s)

Line 41-42: For a long time MT emissions from coniferous trees/plants were treated as temperature-dependent only. Perhaps highlight this in the text or include additional references here regarding early research of temperature-dependent MT emissions from coniferous forests.

Line 50: The deposition loss of organic vapors lead to a reduced availability of these vapors to contribute to the formation and growth of SOA - not the direct removal of already-formed SOA particles themselves. I would suggest rephrasing this sentence. (double-check)

Line 54: You should cite multiple sources/examples for your claim that emissions are typically simulated by MEGAN - conceivably there are other model tools that have been created over the years

Line 60: This section neglects the role that turbulence and mixing has in in-canopy exchange processes.

Line 76: Gradient and profile -based flux estimates have also been used in the past as well (e.g., Rantala et al. 2014 and references therein).

Rantala, P., Taipale, R., Aalto, J., Kajos, M. K., Patokoski, J., Ruuskanen, T. M., and Rinne, J.: Continuous flux measurements of VOCs using PTR-MS—reliability and feasibility of disjunct-eddy-covariance, surface-layer-gradient, and surface-layer-profile methods, Boreal Environ. Res., 19, 87-107, 2014.

Line 91: two measurement heights? Please state them here in the text.

Line 103: It also provides an opportunity to investigate the role of turbulent mixing. mixing times would be particularly important for short-lived compounds such as SQT, which react quickly with ozone.

Line 114: In addition to this description, provide some information regarding the most common understory vegetation species.

Line 114-115: Add a sentence that states the typical forest canopy height h. This is important for footprint estimation and in-canopy estimation of sources and sinks from concentration+sonic profile data. I cannot seem to find the canopy height listed anywhere in the manuscript.

Figure 2: It would be useful if the setup schematic representation include some kind of indication of the forest canopy height. This would also illustrate which portions of the setup are above canopy and which are in-canopy. It would also be useful to add a height scale/reference of some kind to the schematic of the profile setup, as this would provide a point of visual reference for the vertical location of the various components of the tower setup relative to the forest's vertical canopy structure. You state the height of several components in the text but do not illustrate these heights in the figure.
Line 139: State the typical residence time of sample air in the tower's Teflon tubing (travel time from tower inlet to PTR-ToF). How does this time compare (though not necessary to state/add in text) with the lagtimes determined during EC flux analysis? (This is somewhat related to the comment at Line 303)

Line 157-158: As noted earlier, it is useful and relevant addition to provide some information regarding the most common understory plant species at the study site.

Line 163-164: As previously noted, this height information should be illustrated in the Figure 1 setup representation as well.

Line 177: To ambient atmospheric pressure or just into the operational pressure range needed by the T400 to function properly (not necessarily the same)? Please clarify

Line 203: Please provide a citation regarding the EVR.

Line 207: A frequent "internal" mass scale calibration. Be sure to distinguish in the text between internal calibrations and external calibrations (i.e., VOC gas cal. standard from a bottle).

Line 240-241: Is the transmission curve of this Ionicon PTR-ToF-MS linear with m/z in the range of 21.022 to 180 m/z? For Tofwerk's Vocus PTR-ToF-MS, the transmission curve can also be closely fit with a sigmoidal function. It would be useful to see the transmission results plotted for an example calibration run (perhaps such a Figure would be a useful addition to the Supplement). Is the transmission above 180m/z relatively close to unity?

Line 218: You should provide a software reference for the IDA software in your reference list.

Line 219: If one is available, you should provide a reference in the reference list for the PAP software as well.

Line 231: I assume steps i, ii, and iii relate directly to the description of DBSCAN in Ester et al., 1996)? Otherwise, please provide some additional information/background regarding these steps or provide references that describe it.

Line 258: Provide a reference for this statement here.

Line 277-279: You should cite relevant references for each of these methods/steps, or provide a general example reference at the end of the first sentence.

Line 280: I would suggest an alternative word choice here, using "by" to replace "under" or "under low", as the wording here may suggest that only EC fluxes during low/negligible turbulence conditions were kept - EC flux analysis typically performs most poorly when there is little mixing, such as during stable nighttime conditions.

Line 290: Why was the planar fit method not applied to both the TOP and TRUNK systems?

Line 303: As part of the VOC/ozone flux data collection, were the computer clocks for the sonic anemometer's datalogger and the PTR/ozone instrument synchronized in some way? This is a relevant consideration for lagtime analysis during long campaigns (weeks to months). If so, this should be noted in the text for this section. If not, given the multi-year length of observations, was clock desynchronization observed during the lagtime analysis (seems likely given duration of observations). How was clock-desync considered and/or handled? The latest version of innFLUX has a workflow step as part of the lagtime analysis where the clock drift can be fitted using regression and corrected for.

Line 311-314: Could you explain this more? The innFLUX paper seems to indicate that time variability in flow rates could create a problem for this approach in the case of very weak flux signals.

Line 322: Are there any references motivating this approach which you can cite? If so please cite them.

Line 351-352: last access date here may be required - "(https://github.com/icos-etc/RFlux; last access: day month year)". The toolbox that was adapted will also require a reference in the reference list.

Line 402: formaldehyde may not be the best example to include for a PTR, due to its back reaction with water vapor and humidity dependence. As a suggestion, if humidity variability was a significant factor during these plume events, perhaps "such as methanol and acetic acid" for this portion of text, with formaldehyde and additional complications discussed separately, may be more appropriate.

In Loubet et al. 2022 (cited in the text's flux analysis section with respect to WPL corrections), formaldehyde is noted to have large deposition, yet the authors choose not to further discuss it in detail since the uncertainties in its concentration measurement, especially in response to water vapor, were deemed too large to provide consistent measurements.

It seems that formaldehyde should also be handled here with caution as well.

Line 445: Why was the GEddySoft Software not used here as well for ozone fluxes?

Line 668: Sesquiterpenes etc are also, generally, emitted at lower rates than MT and Isoprene.

Line 659-660: The level of detection (3xSD) and level of quantification (10xSD) are different - did you detect any compounds with greater m/z than SQT which did not meet the level of quantification? i.e., did you observe any oxygenated SQTs, heavier photooxidation products, etc., in your measurements?

Line 670: While a general trend, not strictly true for all compounds.

Figure 6: *Note: See also comments for Table 3.*

Figure 6 needs to be revised to either report only identified VOCs (51 reported in the Abstract) or (alternatively) by only reporting ion signal on the y-axis (and by cutting the dot-size representation of EC signal strength or attempting to present it via alternative units, for example based on passing the cps timeseries through EC flux analysis). The former seems likely to be the more practical approach.

In the current version of the figure, mean VOC mixing ratios (y-axis) are compared to m/z (x-axis) for the 76 detected ions. Due to fragmentation and other related signals, this means that these 76 correspond to an inherently smaller number of VOCs: 76 VOC-related ions -> 60 VOCs + fragments (based on Table 3) -> 51 VOCs. In practice, only these 51 identified VOCs represent "real" ecosystem VOC concentrations and fluxes - how are the others converted from cps to mixing ratios here (and subsequently, to fluxes)? For the fragments (9), are they being used to determine the parent VOC concentration (with the figure essentially showing double-counting of certain VOCs?). What about the 16 other signals? This is not made clear in the figure nor its caption, which appears to host multiple instances of double-counting for the fragments (if converted to mixing ratios based on parent VOC ion mass). Of the eight ions featured as the most exchanged, at least two of them are double-counted (isoprene and MT). This ambiguity should be eliminated.

Table 3: One (potentially) significant qualitative issue with this table is the inclusion of parent ion fragments (9) with VOC fluxes and concentrations. The fragment ions are generated within the PTR instrument and are artifacts of the measurement. Just from a reading of Table 3 and the table caption, it is not clear whether you are reporting the parent VOC concentration & flux based on the fragment ion signal (i.e., simply providing another determination of the parent VOC conc. & flux based on the fragment), or for the fragment itself (i.e., converting from cps to mixing ratio based on the fragment's mass). The latter would be a significant issue (though from a reading of Figures 8 & 9, this does not appear to be the case). Therefore the reporting of fragments here, and how they are used to get to a mass mixing ratio (similarly for flux), needs to be clarified.

If the former (i.e., double-reporting certain VOC concentrations and fluxes), I would suggest reorganizing the table and clarifying in the caption text that the fragment rows are merely providing another estimate of the parent VOC's concentration and flux. While this double-accounting of parent VOC conc. and fluxes is quite interesting for its own merits (and may provide indirect qualitative information about the state of the PTR instrument and chemical speciation of a VOC - for example the MTs, due to the differing fragmentation rates among MT compounds, since the external mass calibration is based on a single MT compound), another (quick) solution would be to simply remove these fragment rows from the table.

*There is no ascending/descending ordering to the m/z values of the table rows. What is the logic behind the ordering in which ions are presented in Table 3, Eddy-covariance signal strength? The rows could also be sorted by m/z, or the property by which rows are ordered could be included as an additional column.

Line 680-681: These cycles also reflect changes in atmospheric conditions, such as diurnal changes in atmospheric turbulent stability and boundary layer behavior. Stable nighttime conditions and low boundary layer height can cause an emitted compound to accumulate to high concentrations (for example, can be the case for MT emission from needle-storage at night). This should be noted here as well.

Line 697: Also seen in the vertical ozone profile for boreal forest BVOC measurements presented in Petersen et al. 2023.

Line 821: Given the additional complications with formaldehyde, perhaps this should be pointed out in the methods section as well.

Figure 8: Panel labeling: there is no mention in the figure caption regarding the meaning of the figure labeling (a, b, c, d, etc.). Since a Figure caption outlines the visual elements of a figure, the general logic behind the order of panel labeling should be indicated as well.

To be efficient with caption space, I would suggest modifying the caption's first sentence to something like "Seasonal evolution of VOC fluxes measured by the TOP system over the years (a-e) 2022, (f-k) 2023, and (l-o) 2024.".

Small suggestion: If the m/z legend can be divided into two columns (6-by-2 or 2-by-6) and placed into the space available in the lower-left corner, the entire figure (including text) can be made larger (i.e., more reader-friendly)

Figure 9: Similar to comment for Figure 8, please indicate in the Figure 9 caption the meaning behind the panel labeling (a, b, c, etc.) - for example "(a-f) 2023 and (g-h) 2024".

Line :878 Given the discussed year-to-year differences in the select VOC fluxes presented in section 3.4.2 and the availability of tree species distribution data around the site, the text here (for example, at lines 889-895) can be improved by providing some information about the approximate percentages of the different tree species that fall within each of the yearly footprint climatologies. In the final paragraph of section 3.4.2, the use of species maps such as Figure 1 with flux footprint analysis to better attribute observed fluxes is discussed, but an attempt (even if very simplified) with the readily available Figure 1 data was never made here.

Line 973-975: Some brief additional description/info is required here to link the Abstract text with that of the conclusion. In the Abstract, it states that 51 significantly exchanged VOCs were identified. Yet, here in the conclusion it simply states the study identified "60 significantly exchanged ions, among 76 VOC-related signals". The intended meaning for the number of significant exchanges involved is somewhat unclear (i.e., a loose end) – ions, VOC-related signals or VOCs? Since Figure 6 indicates that there were 76 detected ions with significant exchange - I assume the 60 refers to the number of significant EC signals from the processed ion timeseries data. How does this 60 relate to the 51 described in the abstract - are some of the described exchanged ions not VOCs, etc? It appears so. Table 3 also includes 60 m/z ions with attributed formulae, but also includes ion fragments (9).

Since the number of significantly exchanged VOC compounds is one of the core results of your study, please clarify the final accounting regarding the total number of significantly exchanged VOC compounds (51) here in the conclusion via a sentence, etc.

Table S2: In the Supplement's Table S2 caption, for the gas standard calibration bottles used, please provide the name of the calibration standard manufacturer

Figure S14: Panel labeling (same comment as for Figure S15 and Figure 8 in the main text): Please indicate in the Figure S14 caption the meaning behind the panel labeling (a, b, c, etc.)

Figure S15: Panel labeling (same comment as for Figure 8 in the main text): there is no mention in the figure caption regarding the meaning of the figure labeling (a, b, c, d, etc.). To be efficient with caption space, I would suggest modifying the caption's first sentence to something like "Seasonal evolution of VOC fluxes measured by the TOP system over the years (a-e) 2022, (f-k) 2023, and (l-o) 2024.".

Table S3: The total number of presented VOC-related ions in the table should be mentioned in the table caption.

For Table S3, the Limit of Detection (LoD) should be presented as a table column if possible. Were the LoD values relatively stable throughout the multiyear campaign? If not, then please include min/max or mean ± SD, etc., for the LoD values if possible.

Technical comments:

Line 8: Commenter 2 suggests replacing "pipeline" with "software". I would suggest either this, "workflow", or "workflow software", as it cannot be confused with a physical object but still conveys the meaning that the analysis was carried out through a series of steps which carried out specific tasks/objectives as part of the full analysis.

Line 200: Analyzer or instrument? Here, combined with its use at Line 205, the use of the term is somewhat confusing/fluid. I would suggest "PTR-ToF-MS" or "PTR-ToF-MS instrument".

Line 205: Spectrometer? Here the ToF appears to be discussed independently of the whole PTR-ToF-MS instrument.

Line 207: Provide manufacturer information if available - company name + location, etc.

Line 283: I believe it is spelled "innFLUX". After reviewing the original paper and code repository documentation, it appears that the first letter of the term is only capitalized in the title (it's first word) of that paper.

Line 444: "larger" or "greater"?

Line 595: Requires a last access date and a reference in the reference list.

Line 627: phrasing should be checked here - benzene itself is not an isotope.

Line 610: Phrasing - all deciduous trees, or just European beech? "(i.e., Fagus sylvatica)" might work better here. Consider also using the common name, as the latin name has already been introduced in the site description section.

Line 685: Perhaps it would be more appropriate to say for example "reaffirms", etc. Also, it should be made clear that these observations apply to tropospheric ozone.

Line 689: "cessation of photochemistry" - with sunset (i.e., loss of solar radiation). The sentence text could be improved somewhat by adjusting the phrasing, since there is substantial time between mid-afternoon and sunset.

Line 750: "transport's"

Supplement section 2.1.3: The Dusanter reference appears twice in the reference list (the entries for Dusanter S. et al. 2025a & 2025b are identical). The first name initial should also be omitted when citing (i.e., "Dusanter et al., 2025").

Figure S15 caption: There is a font/text rendering issue with the symbols here - it states for emission ”other ¿ 0” and for deposition ”other ¡ 0”. I assume it is meant to be ”other > 0” and ”other < 0”, respectively.