

Interactive comment on “Annual cycle of volatile organic compound exchange between a boreal pine forest and the atmosphere” by P. Rantala et al.

P. Rantala et al.

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We thank the referee for the review and the helpful comments. The referee comments below are bolded whereas our responses are written in normal text.

It would be valuable for the reader to see the typical diurnal cycle of monoterpenes at this forest site. For example, having a figure for monoterpenes similar to figure 7.

We plotted a figure for monoterpenes similar to figure 7 of the discussion paper. How-
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ever, a diurnal cycle of predicted results (algorithm) was left out from the figure.

Section 2.4. Please clarify the “pool” algorithm for the reader. Throughout the manuscript, authors talk about the “pool” algorithm, however this algorithm is not explicitly described in the text (e.g. does not even have an Equation number).

We added a sentence “The formula, $E_{pool} = E_{0,pool}\Gamma$, is hereafter referred as the pool algorithm” and an equation number for it.

Also, in Table 4 the “storage” name is used, which I guess is the same as the “pool” algorithm, but such a variety in names only confuses the reader..

The “storage” in Table 4 was replaced by the “pool”.

p9550 In15-17. Please clarify what the authors meant with this sentence

We clarified the sentence (p9550, In 15-17) in the manuscript.

P9555 In8-11. It is possible to roughly estimate the influence of humidity on formaldehyde sensitivity, because the proton transfer to formaldehyde and the backwards reaction with water have known reaction rates. Together with information about the ambient humidity level, this influence and the formaldehyde mixing ratios can be estimated. Have the authors tried this approach?

We tried to minimize the interference of water vapour using a normalization method

which takes into account changes in water cluster ions (Taipale et al., 2008). Other approaches were not applied in the manuscript.

P9555 In22-23. Do the authors mean hexanol or hexenol? m/z 85 has been attributed to hexanol in other works (e.g. the Buhr et al 2002 cited in the manuscript), while hexenol has been attributed to m/z 83, and Hakola et al 2001 also reported hexanol emissions from birch in addition to hexenols. Please clarify and, even better, provide some additional references to support the assumption of the identity of m/z 85.

There was a typo in the manuscript: we mean hexanol.

P9557 In8-13. What is the purpose and value of this "first step" of analysis of m/z 69? It is expected that isoprene and/or MBO fluxes follow light and temperature variations, as has been shown e.g. for MBO at the leaf (Harley et al 1998) and canopy (Kaser et al 2013) levels from *Pinus ponderosa*. This known relationships explain the good correlations with the algorithms. Anyway, given that authors talk all the time about having correlations with $p < 0.0027$, they should show the values of p in the corresponding tables (e.g. Table 3 in this case).

We agree that the algorithm is well-known. Our purpose here was to quantify the emission potentials of isoprene+MBO. Thus the correspondence of the algorithm and the data was checked. We defined that the correlation between the measured values and the algorithm was significant if $p < 0.0027$. We found significant correlations from May until August, therefore, we argued that measurements from those months are realistic. We will clarify the text in the manuscript. We also included p -values into Tables 3-5.

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P9561 In1. Maybe change "material" to "dataset"?

Changed.

Table 2. This reviewer has always seen the statistically significant results marked with an asterisk. The authors, however, chose to mark the non-significant results. Unless there is a very good reason for it, I suggest marking the significant results with an asterisk, otherwise the reader may be confused.

The significant values are now marked with an asterisk.

Tables 3-5. Please show the p values for the correlations and whether the authors considered the correlation significant or not.

We have defined in the table captions that statistically significant correlation has a lower p -value than 0.0027 (3σ), and only those correlations are shown. We included p -values into Tables 3-5 and clarified the text in the table captions.

Table 4. This table shows the "E0,hybrid" parameter. If this reviewer interprets correctly, the lower part of the table corresponds to the "pool" algorithm (please unify the name of this algorithm throughout the manuscript, and explicitly show the pool algorithm formula). If that is the case, I think that the relevant parameter should be in the caption of the column of this lower part, because the pool algorithm does not use the "E0,hybrid" parameter, but the "E0,pool" instead.

We have unified the name of the formula and added a parameter $E_{0,pool}$ in the caption of the column.

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Figure 4. The "E0,pool" and the fsynth symbols are easily confused when used with error bars in the graph. Please change the symbols to avoid confusion.

We re-plotted the figure with more unambiguous symbols.

REFERENCES:

Taipale, R., Ruuskanen, T. M., Rinne, J., Kajos, M. K., Hakola, H., Pohja, T., and Kulmala, M.: Technical Note: Quantitative long-term measurements of VOC concentrations by PTR-MS - measurement, calibration, and volume mixing ratio calculation methods, *Atmos. Chem. Phys.*, 8, 6681-6698, 2008.

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