

Interactive comment on “Field observations of Volatile Organic Compound (VOC) exchange in red oaks” by Luca Cappellin et al.

Anonymous Referee #2

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The manuscript of Cappellin et al. reports fluxes of volatile organic compounds (VOC) from branch enclosure and canopy profile measurements in a mixed forest. Moreover, the author performed branch-level fumigation experiments by employing some selected Oxygenated-VOCs to get new insights into the sources and sinks of those VOCs. The manuscript is generally well written and fits the scope of the journal. However, I suggest the author to address some major and minor issues (listed below) to get the manuscript ready for publication.

MAJOR ISSUES: - The author collected data over a period spanning 2 weeks (from 14th of August to 1st September) which can be defined as a ‘short-term’ period of time. Therefore, the author should take into account that emission/deposition rates of (O)VOC and their dynamics presented in this manuscript can change over a longer period of time (i.e. months). - Despite the concentration of several VOCs have been

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measured simultaneously by PTR-TOF-MS, the author reported in the manuscript only “the most important OVOC and volatile isoprenoids” (as written in line 1, page 16). The author should clearly define ‘how’ those oVOC and the isoprenoids have been selected/filtered from the multitude of protonated ions related to VOC (and/or fragment of VOC.) recorded by PTR-TOF-MS. - I remind the author that ‘concentrations’ of VOC shown in Figure 1, may represent a larger area possibly including other sources/sinks than the mixed forest investigated by the author. Indeed only through a ‘footprint analysis’, a model that considers the concentration distribution of VOC with respect to wind speed and the turbulent air conditions occurring at the site, it can be sorted out the representativeness of the measured concentration on the surface area (Horst and Weil 1992, 1994; see also Schmid 1994 for details). - The author discussed the results, i.e. of methanol fluxes (lines 9-24, page 30) by comparing VOC fluxes calculated in this manuscript from measurements collected at branch-level (and then scaled-up at canopy-level after considering the average LAI and inhomogeneity of the canopy; as written in lines 20-24, page 16 and lines 1-4, page 17), with results found in the literature where VOC fluxes have been measured at canopy level by eddy covariance technique. The author should discuss and make the point on how the different approach in calculating the VOC fluxes may have produced different results (i.e. eddy covariance flux measurements provide emission/deposition fluxes from complex sources/sinks also including the soil, while branch measurements provide indications of VOC emissions mainly from leaves as the only source/sink). - I am wondering whether the author can provide additional data on both the CO₂ and water vapor exchange from branch enclosure measurements. Indeed, the measurements of CO₂ assimilation rates would give information on the physiological performances of the leaves enclosed in the branch chamber; this would be particularly important to evaluate any damage eventually occurring to primary leaves metabolism following MVK absorption (in correlation with detoxification and thus MEK emission). In addition, measurements of water vapor exchange (even though at branch level) would provide basic information useful to both estimate the extent of stomata conductance and to partition the stomata and

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non-stomatal contribution to the uptake of (O)VOC (Fares et al. 2012) that has been mentioned several times throughout the manuscript. - Since the 'compensation point' may vary on the basis of both physiological and physicochemical factors (as reviewed by Niinemets et al. 2014), when discussing this issue it would be better to indicate, at least, the environmental conditions under which this point has been determined. - Material and Methods section needs to be re-formatted and dramatically shorten it (now it is almost 10 pages long!). I suggest the author to either delete or move to 'Supplementary Information' most of the description of PTR-TOF-MS technical details (which is now 6 pages long) and simply mention citations of the many previous scientific works where PTR-TOF-MS has been already described. In fact, subsections 2.4.1 and 2.4.2 belong more to a textbook than to a scientific paper, while much of the 'Spectral analysis' subsection could be summarized in a table (i.e. by comparing VOC analysis by PTR-TOF-MS set in H₃O⁺ and NO⁺ mode). Moreover, if not commercial, a schematic and/or a picture of the canopy-top branch enclosure employed for the measurements would be very much informative.

MINOR ISSUES: - Consistency I required throughout the manuscript! Please make use of either the term BVOC or VOC. - Is there any particular reason why fluxes are always expressed in "nmol m⁻² hour⁻¹", instead of "nmol m⁻² second⁻¹"? (in which all the measurements unit are indicated according to the International System of Units) - Lines 12-13, page 1: I suggest to replace 'atmospheric reactor' with 'atmosphere'. - Line 14, page 6: the author should describe the plant species composition of the mixed forest that has been investigated; this can offer information to improve the discussion of sources/sinks of (O)VOC emission/deposition. - Line 21, page 6: it is written 'Influence', without specifying 'of what'? - Lines 3-4, page 8: I wonder why the fonts of this sentence are in 'bold'... - Lines 14-15, page 16: This is not clear, and I suggest the author to rephrase it. - Lines 13-14, page 17: I remind the author that also Brilli et al. (2016) showed MVK+MAC emission fluxes at canopy-level from a poplar plantation. - Line 4, page 18: the author should quickly explain 'why' 'the actual presence of emissions could not be proved. - Lines 4-5, page 20: I suggest to remove both the

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words 'first' and to replace the last word with 'before'. - Lines 14-17, page 20: I remind the author that also Brilli et al. (2016) found emission and deposition fluxes of MEK through eddy covariance measurements at canopy level. - Lines 19-20, page 22: the author should specify which kind of 'correlation' (i.e. linear?) - Lines 1-3, page 23: I remind the author that emission of alcohols (i.e. belonging to the mixture of Green Leaf Volatiles) does not depend to the plant species, but to the occurrence of specific situation (i.e. the presence of herbivores inducing mechanical damage to leaves). - Line 16-17, page 24: the author meant 'benzaldehyde concentration', in air ? - Lines 5-6, page 27: These citations are missing in the reference list. - Lines 7-8, page 28: I am wondering if the Model of Emissions of Gases and Aerosols from Nature has been never applied to simulate acetone emissions. - Line 18, page 29: I remind the author that also Hüve et al. 2007 investigated the control of methanol release by leaf expansion and stomata.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-901, 2016.

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