Atmos. Chem. Phys. Discuss., 9, C1895–C1898, 2009
www.atmos-chem-phys-discuss.net/9/C1895/2009/

Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on “In-situ ambient quantification of monoterpenes, sesquiterpenes, and related oxygenated compounds during BEARPEX 2007 – implications for gas- and particle-phase chemistry” *by* N. C. Bouvier-Brown et al.

Anonymous Referee #2

Received and published: 16 June 2009

This manuscript presents ambient measurements of MT (monoterpenes), SQT (sesquiterpenes), linalool, and methyl chavicol using a modified GC-MS system. The dataset is valuable because ambient concentrations of SQT and oxygenated monoterpenoid have been rarely measured and reported. As significances of their roles in SOA (Secondary Organic Aerosol) formation have been speculated, the authors estimated their contributions in photochemical reactivity (OH and ozone) and SOA within the Blodgett Forest canopy, a ponderosa pine ecosystem. The results and discussion of this manuscript is pertained to aims of Atmospheric Chemistry and Physics. Therefore, I recommend the manuscript to be accepted with minor revisions, based on following points.

1. Some of the measured compounds such as linalool and bergamotene in the ambient air have been known as “herbivore-induced plat volatile emissions” (e.g. Kessler and Baldwin 2001 Science). Is there any indication that biotic or abiotic stresses were triggered at the ecosystem?

2. How different was the ratio of SQT/MT that you observed from branch enclosures and the ambient air? Authors’ argument that reactive SQT were quickly removed from the ambient air can be quantitatively discussed by comparing the ratios and fractions of reactive SQT emissions from enclosure measurements. Especially, the discussion about SOA formation potential from newly measured BVOC in section 4.4 can be supported by the discussion. In the section, the assumption for the amount of SQT contributing SOA formation is simplified although it can be more quantitative by comparing the speciation information between branch enclosure (Bouvier-Brown et al., 2009a) and ambient measurements.

3. Although ambient concentrations of SQT and other oxygenated compounds have not been reported, number of publications has reported the emission rates of SQT using various GC methods (Duhl et al., 2008). Is there any possibility of underestimation of emission rates by previous studies due to different GC configurations from this study? In the method section, authors described the modifications, added to Millet et al. (2005) but it is not sure that the modifications have not been tried by other research groups or authors adapted from other groups.

More specific comments and questions on the manuscript are listed below.

Page 10237 Line 18-23 Many other SQT other than β-caryophyllene and α-humulene are described in this study. Therefore, SOA yields from the chamber studies other than β-caryophyllene and α-humulene could be explained if they are available. If they are not, the fact should be also pointed out so that other researchers can conduct studies on SOA yields of other SQT.

Page 10240 Line 15 Please specify the sample residence time in the sampling line so that readers can get a clear idea about wall loss.

Page 10241 Line 9 How was the temperature of the sample pathway (50 oC) decided? Have you tested various temperatures and ended up the temperature? Please provide more information.

Page 10241 Line 22 Typo (I guess) Marin -> Marrin Page 10241 Line 25 to Page 10242 Line 5 What was the concentration range of liquid injection calibrations of SQT and other compounds?

Page 10242 Line 23-29 Kim et al (2009 AMT) reported that wall loss of SQT is a function of temperatures and concentrations. Please, therefore, describe the concentration range of each compound and the temperature range during the tests.

Page 10247 Line 23 Please specify “oxidant mixing ratios” in the text rather than referring from the previous study such as average daily variations of ambient OH and ozone if they are available.

Page 10248 Line 10 Please put more discussion on “ample evidences”. It is an important part of this study so readers may want to see extended discussion

10249 Line 1 The title is not clear enough. Authors need to specify what exactly “mass” means. In my perspective, it could be understood either the total organic compound mass in the air or detected masses by the GC-MS system.

10249 Line 19 Please provide more quantitative information on the importance of manzanita emission to the total BVOC emission of the ecosystem. Especially, the relative importance, compared with the ponderosa pine emission of the ecosystem will be helpful because most of readers including me probably regard Blogett Forest as a ponderosa pine dominant ecosystem.

10250 Line 1 The title also could be more specific. Most of discussion is about OH and ozone reactivity in the ambient air of the ecosystem, so please specify it.

10251 Line 1-4 In the discussion of OH and ozone reactivity calculations above, authors pointed out that the estimate rate constants using US EPA AOPWIN could cause large uncertainty. In the context, it would be desirable to specify the portion of OH and ozone reactivity that calculated with the EPA software suite. Table 2 The recovery of methyl chavicol looks too low by considerations of its chemical lifetimes with respect to OH and ozone. Please explain. Aromadendrene in the table has a very short ozone lifetime but indicates high recovery rates. Is this suggesting that the rate constants could have large uncertainty?

Table 3 Please edit superscriptions properly for notations of OH, ozone, and NO3 concentrations.

Table 4 Please specify how many data points were averaged.

Figure 3 Please specify how many data points were averaged.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 10235, 2009.