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Dear Editor,

We thank the two anonymous referees for their critical comments. Based on comments we have re-written and edited the manuscript and hope it would now more clearly reflect the study aims and results.

Below are our responses to the specific suggestions and comments.

Sincerely Yours,
Hanna Lappalainen

Anonymous Referee #1

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(1) **The study is not clear enough about the distinction between concentrations of BVOC in the atmosphere on the one hand and emissions from the vegetation on the other, and it mixes these two at several coincidences. Not only does this neglect the importance of the atmospheric sinks (and their changes in response to the environmental factors that drive emissions as well), it assumes a constant volume of air for mixing the emissions into, and would require a much faster "cleaning" of the air by atmospheric decay than that related to the lifetimes mentioned to ensure that the integrated daytime emission rates are a measure for concentrations.**

In the introduction we postulate that in a boreal forest temperature is the main driver of seasonal variations in the biogenic synthesis process (photosynthesis) and the biogenic emissions and consequently the ambient air concentrations as long as anthropogenic sources can be omitted. It remains for each study to show whether the ambient concentrations represent the canopy emissions at the particular situation. In this study we used data filtered with the wind directions from anthropogenic sources (a saw mill; see Lappalainen et al. “Day-time concentrations of biogenic volatile organic compounds in a boreal forest canopy and their relation to environmental and biological factors” Atmos. Chem. Phys. 9, 5447--5459, 2009) and day-time medians of specific time windows of the BVOC concentrations. The time windows specified for each season represented the time when the sun is high enough to cause atmospheric mixing. In Lappalainen et al. 2010 we also studied the effect of the height of atmospheric boundary layer (hours scale) on VOC concentration but did not find any correlation during day time (unpublished data). We don’t downgrade the role of the

atmospheric sinks but we see that the studied concentration were inter correlated indicating the atmospheric life time had little influence on the changes in the concentrations. As a whole our fundamental approach was to test and prove a scare parameterized daytime VOC concentration model. To clarify this, we have now reformulated the text to bring our point up more clearly. We also clarified the use of terms: emission and concentration throughout the manuscript to be consistent.

(2)

The models rely on fitting of relatively simple functions to a set of observations, but the fitting procedure is not described. It is not straight-forward to fit a non-linear four-parameter model, and a better description is needed to judge the method. This description should also highlight the treatment of possible correlations (temperature, light (PAR) and ozone), e.g. for parametrizing A and B in equation (4).

For each model and compound we first used the data set of 2006-2007 to determine the model parameters and then tested how well the model performed to the test dataset of summer 2008. The parameter values were estimated by minimizing the residual sum of squares (RSS). The discrepancy between the data and a model and the model comparison were quantified by coefficient of determination (R^2). In order to clarify the fitting procedure we have reformulate the text in a more detailed manner.

(3)

The treatment of the data is somewhat odd. The authors rightly split their data set in a set used for curve fitting (2006-2007) and an independent set for testing (2008), but in their reporting they focus primarily on the results for the first set (e.g. in the Abstract and the Discussion), whereas it would be more fair to report here the results from the independent data set, as these provide the best judgment of the model's performance.

The distribution of the dataset was based on the practical reasons. We already on the way modeling the concentration with the 2006-2007 data, but got the summer 2008 data much later. We have added a more detailed analysis of the summer 2008.

Minor remarks:

- **page 20038, line 2: Do not use "complex number" here, a complex number is something completely unrelated- page 20040**
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- Word **"complex" has removed.**

- **line 21: Explain the unit amu**

We have added the following clause**:** Methanol was detected at 33 protonated mass [amu] (M33), acetaldehyde at 45 amu (M45), acetone at 59 amu (M59), isoprene and methylbutenol at 69 amu (M69), and monoterpenes at 137 amu (M137) (Taipale et al. 2008)

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- **page 20041, line 5: How is the percentage derived? As a cover fraction, or from the biomass?**

  It was based on the biomass.

- **page 20043, line 13: Clarify A and B in the equation, and the method to derive these**

parameters.

We edited the text to clarify the A and B in the Trigger model.

- **page 20045, line 27: The reasoning that the independent data set is less representativebecause of the low amount of measurements is awkward. The authors are free to distribute their data in a different manner between fitting and independent data if they consider the independent data set too small.**

We would like to comment that the distribution of dataset is based on practical reasons. At the time we started this study the quality checked dataset 2006-2007 (see also Lappalainen et al. 2009) was available and the summer 2008 dataset was available much latter.

- **page 20046, line 8: Are the references to Figs. 7 and 8 intended here?**
  The correct reference is Table 1.

- **page 20046, line 11: Is ressum the same as RSS (equation (5))?**

  This part of the text (2.4) has been deleted and is reformulated in the first chapter of 2.3

- **page 20046, line 23: For which compounds is the positive correlation significant?**

We have not performed a statistical analysis and we use an expression “indicating”.

We have responded to minor remarks and spell out the abbreviations and explain the used expressions.