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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modelling day-time concentrations of biogenic volatile organic compounds in a boreal forest canopy" *by* H. K. Lappalainen et al.

Anonymous Referee #1

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"Day-time concentrations of biogenic volatile organic compounds in a boreal forest canopy and their relation to environmental and biological factors", by H.K. Lappalainen et al.

The study by Lappalainen et al. attempts to simulate the concentrations of a number of BVOCs (methanol, acetaldehyde, acetone, isoprene, monoterpene) with relatively simple functions of environmental conditions. Model parameters were derived by curve fitting with observed concentrations from 2006-2007 at a boreal forest stand in Southern Finland, and tested against observations from 2008 for the same site.

The importance of these BVOCs in atmospheric chemistry makes this a relevant topic,

and there is room for these kind of simple models next to the more advanced types of simulations. However, there are a few major flaws that to my opinion make the study in its current state unsuited for publication in Atmospheric Chemistry and Physics:

(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 (e.g., page 20042, line 16 by comparing an emission model directly with the authors' concentration model, and page 20048, line 4, by using an additional factor that represents a tree's seasonal emission (not concentration) capacity). The statement that understanding concentrations is based on understanding emissions "as long as atmospheric mixing is high..." (page 20039, line 22), and that emission models can be used to simulate concentrations because of "[the] convergent behaviour of [methanol, acetone and terpenoids]" (page 20040, line 14) is too simplistic and doubtful. 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.

(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).

(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.

Minor remarks:

- page 20038, line 2: Do not use "complex number" here, a complex number is something completely unrelated

- page 20040, line 21: Explain the unit amu

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

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

- page 20045, line 27: The reasoning that the independent data set is less representative because 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.

- page 20046, line 8: Are the references to Figs. 7 and 8 intended here?

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

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

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 20035, 2010.