

Interactive comment on “Modelling atmospheric OH-reactivity in a boreal forest ecosystem” by D. Mogensen et al.

Anonymous Referee #2

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The authors describe the modelling of the OH radical reactivity for 2008 in the boreal forest station in Hyytiälä, Finland, and compare the output with the measured OH reactivity from August in 2008 at this site. Modelling has been performed using organic emissions from the MEGAN model and chemical transformation was described mainly by MCM. PTR-MS measurements revealed a good agreement between measured and modelled monoterpene concentrations. OH reactivity measurements have been performed in an indirect way by following pyrrole disappearance in competition to the OH reaction with the ambient air constituents. Modelled OH reactivity was found to be significantly lower than the measured data. The authors are discussing possible reasons. The paper is well structured and well written and within the scope of this journal. Before publishing, some revisions could improve this manuscript.

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1) Emissions of organics were taken from MEGAN. It would be nice to see a comparison measurement vs. model e.g. for isoprene concentrations and monoterpenes in total at ground allowing an impression how accurate concentrations of organics are described. Degradation pathways for alpha- and beta-pinene were explicitly taken into account by modelling. What was done with other monoterpenes, especially limonene? Nothing was said regarding sesquiterpenes!

2) NMHC account for 21% of total OH reactivity, cf. Fig.3. A couple (or the majority) of organic concentrations are directly available by PTR-MS measurements. What is the resulting OH reactivity using these measured organics with respect to the usage of modelled organics in a more comprehensive way. Is the OH reactivity using modelled organics definitely higher than the OH reactivity with the measured organics according to eq.(R1)? What is the improvement using the complex modelling by MCM?

3) The authors should discuss more in detail the other OH reactivity measurements given in literature, the comparison with model predictions and the resulting discrepancies.

4) A hot topic in gas-phase chemistry at the moment is the point that obviously the HOx chemistry especially under low-NOx conditions is not well understood. A series of field measurements for OH and HO2 cannot be explained by the current understanding of atmospheric degradation mechanisms especially for isoprene. Measured OH radical concentrations are significantly higher than data from modelling runs. Very efficient OH recycling is proposed, i.e. $\text{OH} + \text{isoprene} \rightarrow n \text{OH} + \text{products}$, $n > 1$ (Stone et al., Atmos. Chem. Phys. Discuss., 11, 10343–10401, 2011). The authors of this paper here should point to this topic. However, a possible underestimation of OH radical concentrations by models (also by MCM) does not help to explain the discrepancy of modeled OH reactivity vs. measured OH reactivity, it would strengthen the difference.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 9133, 2011.

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