Atmos. Chem. Phys. Discuss., 15, C3203–C3206, 2015
www.atmos-chem-phys-discuss.net/15/C3203/2015/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Contribution from biogenic organic compounds to particle growth during the 2010 BEACHON-ROCS campaign in a Colorado temperate needle leaf forest" *by* L. Zhou et al.

Anonymous Referee #2

Received and published: 3 June 2015

The authors describe the modelling efforts to predict new particle formation events (NPF) in Manitou Experimental Forest Observatory during the 2010 BEACHON-ROCS experiment. The goal was study the interaction of sulfuric acid and organic vapors arising from monoterpene (MT) and MBO oxidation in NPF. The applied the 1D column model SOSAA, although, as stated by the co-authors themselves, the terrain was too complex for the model to be able to catch all features, especially during nighttime "the drainage flows down the side of the mountain cause difficulties for the model to simulate the meteorological conditions". Nevertheless the model performed acceptable well in

reproducing the daytime meteorological conditions, at least at time when NPF was expected.

The daytime MT and MBO concentrations were described by emissions from MEGAN and extended MCM photochemistry. The diurnal cycles of the precursor VOC were also described qualitatively quite well. However with too high overall conc. of MBO (factor 2- 1.5) and very high nighttime concentrations of MT. The proposed explanation for the latter is a too high night time temperature predicted of the model. But this hypothesis could be tested by testing the T-dependence of the main emissions in the MEGAN emission algorithm. I suggest to do that in order to convince the readers that this is indeed the explanation.

Amazingly the model fails substantially in predicting the daytime sulfuric acid concentrations and the afternoon OH concentrations. The argument that a JNO2, too low by about 20% around e.g.16:00-17:00h in the model compared to the measurement leads to a factor of two too low OH concentrations at that time period seems not too convincing to me. The question arises is if the model has missing OH sinks, and if these are organic vapors which are oxidized. How would this affect the predicted aerosol dynamic. I suggest to discuss this point in more detail in the manuscript.

The too low H2SO4 concentrations were compensated by increasing the kinetic coefficient K in the nucleation parametrization.

How critical is the adjusting of K in context of too low prediction of H2SO4 ?

The explanations why the model fails in the sulfuric acid concentrations fall a little too short. How important is the H2SO4 production from OH? You overestimate OH by 100% in the afternoon, so the missing term might be really huge. Is that realistic? Could it be that simply the SO2 input is too low? I suggest also here more explanation why the model prediction fails.

The organic contribution too growth is parameterized by using the first generations of

stable vapors from MBO and MT generated by the oxidants OH, O3, and NO3. Vapor pressures where then attributed to the vapors, and the effect of MT snd MBO alone and of both MBO and MT together was studied. MBO and MT vapors are needed to predict the observed size distributions and the agreement between prediction and observation is not too bad.

Nevertheless I wonder, why the first generation vapors are used as a measure. It is well known that with exception of ELVOC from ozonolysis the vapor pressures of those products are way too high to explain growth and SOA formation. Moreover during daytime first generation products can be oxidized further by OH. How such an ageing process would influence the results?

The authors derive limits for the vapor pressures to match the observations and suggest in the Conclusion section that the condensing vapors should have vapors pressures as low as 10^6 cm-3. The author should discuss in how far the vapor pressures attributed to VapI, VapII, and VapIII match the lumped compound classes. And what can be concluded from such a comparison.

Otherwise the paper is quite well written. Figures are informative material is presented in a good way. After addressing the major comments above the manuscript can be published in ACP.

Minor comments:

p9039, l13: The tower on the measurement site was not introduced before.

p9040, l3: use “differential mobility analyzer” instead of “differential particle counter”

p9044, l25ff: Does such a to flat diurnal temperature profile influence the vertical transport? If so, what does that mean for the model observations?

p9045, l14: I suggest to use either “mast” or “tower” throughout the manuscript.

p9045, l18: I don’t understand point (2), are suggesting that the two different tempera-

ture measurements were potentially off by several degrees ?

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 9033, 2015.