**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "New particle formation from the oxidation of direct emissions of pine seedlings" *by* L. Q. Hao et al.

Anonymous Referee #2

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The paper describes experiments of nucleation of organic particles from biogenic VOC emitted by living pines. The authors studied the influence of O3 and OH on nucleation rates. The gaseous precursors were characterized by GC-MS and PTR-MS. Particulate matter was characterized by nano-SMPS and aerosol mass spectrometry (AMS). The chemical processes leading to nucleating species were modeled by a hybrid of a explicit and a conceptual model. The manuscript presents new results with respect to OH and O3 in nucleation processes in line with ongoing discussions about the role of organics in atmospheric nucleation. After some revisions the paper should be published in ACP.

Major comments:

p. 8227, line 9f:

Why do you need to simulate beetle attack? To which extent does this effect the patterns and the strength of VOC emission compared to outside pines. Do you have indication of emission of larger oxygenated VOC, sesquiterpenes or other large VOC? How large is the fraction of non-identified compounds?

p. 8228, line 14f: The experimental procedure became not quite clear to me. What do you mean by

" Ozone and TME were kept separated until inside the chamber because of 15 the short lifetime of OH radical. " ?

It would be helpful if add the starting times for second O3 addition in Table 2.

Section 2.5:

The authors should extent this section. It does not became clear to me how the model calculations were performed. How does the model look like, which reactions were included. The reactions should be listed in a table together with the used rate coefficients. How did you solve the reaction system ?

More detailed information should be given how the gamma's in Equ. 6 and 8 were determined.

In the legend of Fig. 5 the derivation of nucleation and condensation rates from the size distribution measurements is mentioned. The procedures should be described somewhere in the experimental section 2.

p. 8235, line 8f and Fig. 2:

The curves could be better understood if you would indicate the time of the second ozone addition.

Section 3.2.2 and Fig 3.

Fig 3 is of insufficient quality. The y-axis of the panels C,D should be doubled in length. The different y-axis' should be better separated. Please add the model OH concentrations to the panels D.

p. 8237, line10 ff :

I don't understand this argument. Growth rate in E1a and E1c are as high as E3 with low O3 rates and high OH rates !? Please clarify and expand your explanations.

p. 8238, line 8:

Mentel et al. claim no nucleation with O3 at their VOC levels. Which role plays the initial VOC concentration in your study compared to Bonn and Moortgat 2002, Burkholder et al. 2007, and Mentel et al. 2009?

p. 8239, line 13: As far as I understood m/z44 is related to carboxyl compounds (acids). These are formed in the ozonolysis reactions. What could be the reason / mechanism for m/z 44 from OH reactions ?

p. 8239, line 26ff:

Can you give a quantitative statement, how well the pattern of your average mass spectra correlate with those observed in the field. bservation of the same masses seems to be a little bit weak statement, especially considering the strong tendency to

fragmentation of the AMS.

p. 8240, line 17:

AMS can measure in the size range of Aitken mode ? How did you determine the density ?

Minor:

p. 8238, line 8: There is a "in" missing.

I would suggest to give the OH concentrations in Table 1 in terms of molecules/ cm-3 not as mixing ratios

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 8223, 2009.