

Interactive comment on “New particle formation during α - and β -pinene oxidation by O_3 , OH and NO_3 , and the influence of water vapour: particle size distribution studies” by B. Bonn and G. K.Moortgat

Anonymous Referee #2

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OVERVIEW

Overall, this is a well-written paper on a timely topic (organic aerosol nucleation from the oxidation of monoterpenes). The approach is appropriate, and the paper presents new data. There are a number of specific questions regarding the details of the experiments and analysis listed below.

SPECIFIC COMMENTS

On page 471, what is meant by further oxidized? Is it not just oxidized (typically monoterpenes are pure hydrocarbons)?

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Also on page 471, where it is said that semi-volatile products will partition onto pre-existing aerosol, it should probably read newly formed (i.e., via nucleation). Pre-existing aerosol in homogeneous nucleation experiments would be a bad thing!

On page 472, where it says that the chamber was usually filled with air, does that it was sometimes filled with other materials?

On page 473, what relative humidities do those water vapor concentrations represent?

Also on page 473, what is the minimum diameter measured by a long column DMA such as the one used here? Would it be possible to use a nano-DMA to get at smaller particles? How much do the authors believe their inability to measure the smallest particles affects their data?

When the authors used such high concentrations of cyclohexane as a radical scavenger, did they confirm that it did not lead to SOA formation itself? (page 474)

In the OH-initiated runs, was ozone also formed in the gas-phase? It seems as though the chamber had all of the required species to form ozone (NO, NO₂, peroxy radicals, light). (page 475)

On page 476, what is the source of the N₂O₅?

On page 478, the authors should note that there are also many ring-fragmentation products of the ozone-initiated oxidation of beta-pinene (though nopinone might be the dominant overall product).

On page 479, in the analysis, are wall losses taken into account?

Also on page 479, the loss of the biogenic within 2 minutes seems extraordinarily fast. What was the OH-level (modeled or measured would be fine)? Is this a number relevant to the atmosphere?

On page 484, it would be helpful to do some calculations to try and model HNO₃ formation so that an estimate of this artifact could be found.

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Also on page 484, it needs to be made clearer what the difference between this paper and the previous paper investigating organic aerosol nucleation is. Are the experiments described here completely new?

On page 485, the authors describe the limiting effect of water on nucleation through competitive reaction with carbonyls (a higher concentration of water competes with carbonyls for reaction with the Criegee intermediate, with the carbonyl reaction being more likely to lead to nucleation). In this vein, how do the carbonyl concentrations in the ambient compare to that of water? And in the chamber? Essentially, the question is whether or not the concentration of carbonyl in the ambient is high enough to matter relative to water.

On page 486, the authors indicate that if appropriate reaction orders were not followed, more SOA was formed in low concentration experiments as opposed to high concentration. This indicates to me that appropriate cleansing methods were not employed and calls into doubt some of the data since there is no way of knowing if there are even additional artifacts than those described in the paper.

On page 487, is a scavenger still used in the flow ozonolysis experiments?

On page 489, only the photochemical experiments of Griffin had NO_x (not all experiments as is stated). The authors should also note that their NO_x levels are even higher and that the work of Griffin was performed at low humidity. There are other comparisons that can be made as well (Odum, Hoffmann, Pandis, Zhang, among others).

Figure 3a does not provide much information since it is so crowded. Many of the lines can be removed since they are essentially all the same anyway. Same with figures 4 through 7.

TECHNICAL CORRECTIONS

Rayleigh is misspelled (page 470).

The authors should cite works describing Mie and Rayleigh scattering (page 470).

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It might be helpful to include Figure 1 earlier (suggestion: page 471 where the structures of alpha- and beta-pinene are first discussed).

On page 473, it would be clearer to use one 10 s upscan (as opposed to 1 10 s).

Dynamical should be dynamic on page 477.

On Figure 2a, is cyclohexane in ppm or ppb? The text says ppm, the figure ppb.

Interactive comment on Atmos. Chem. Phys. Discuss., 2, 469, 2002.

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