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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Organic nitrate and secondary organic aerosol yield from NO_3 oxidation of β-pinene evaluated using a gas-phase kinetics/aerosol partitioning model" *by* J. L. Fry et al.

Anonymous Referee #1

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General comments:

The manuscript presented (Organic nitrate and secondary organic aerosol yield from NO3 oxidation of beta-pinene evaluated using a gas-phase kinetics/aerosol partitioning model) by Juliane Fry and co-workers reflects an interesting study of measured and modeled aerosol yields for the reaction of NO3 + beta-pinene. As pointed out by the authors very little have been done in the past to research the effect of night time reaction between the nitrate radical and the most common volatile organic compounds and the ability of their reaction products to participate in the formation process of secondary

organic aerosols. The manuscript is written in a very detailed and comprehensive way and presents interesting aspects which are important for further studies. It might be that after publication, this manuscript may encourage part of the community to improve and test their aerosol yields in regional and global models in comparison with observations. I believe that after consideration of some minor aspects this manuscript will fit complete in the frame of the journal ACP and contributes very interesting aspects. Following up I recommend publishing this manuscript after small improvements.

Detailed comments concerning the experimental setup and interpretations of these outcomes are already published by referee number 2 and not the field of my expertise. The modeling part reflects a simple gas-phase kinetics model and the use of a sectional aerosol dynamical model. The authors mention on page 18049 that ozone-initiated oxidation of beta-pinene is less than 1% of NO3 which would give with the reported ozone concentration and the used reaction rates a nitrate radical concentration around 30 ppt in the chamber at time zero. This value is at least for all rural environments an upper limit and reflects more the situation in or downwind of polluted areas. To make this easier for the readers the authors should include in figure 1 the single concentrations of NO3 and N2O5 which they should receive from their gas-kinetic model. In this context it would be also desirable to include the color size distribution plots for both experiments beside the discussion about these data in the text.

In the end of the manuscript the authors use their results to point out the effect of NO3-monoterpene reactions on the global scale. Two issues should be taken into account when making the final estimates for the global SOA source: the high difference of a factor 5 between the outcomes from this study compared to the experiments done by Hallquiest and co-authors. It is obviously not possible anymore to clarify the reason for this high discrepancy and for this reason the upper estimates of the aerosols yields should be reduced or at least mentioned at the end. Second beta-pinene reflects only a small fraction of the total sum of monoterpenes (25 % mentioned by the authors which will be for many areas an overestimation) and very little is known about the possible

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NO3-aerosol yields for the other monoterpenes especially for those with an internal double bond e.g. alpha-pinene.

Technical corrections:

Page 18048: Second paragraph, the authors mention that at time 0.5 h in the dry experiment, the oxidant mixture is charged further by a second addition of NO2 and O3. However, this is for O3 not at all visible in figure 1 e and f. Is this correct or was the edition so small that you can not observe it in the measurements. Please include the added value of O3.

Same page 3. paragraph the authors say that the concentration of O3 remained elevated (above 40 ppb for ozone) throughout the experiment. In figure 1 e and f the ozone concentrations decreased to 20 ppb.

Page 18049: first paragraph last sentence: This suggests that water vapor does not affect the gas-phase mechanism or partitioning into the aerosol phase. Comparing the figures for dry and humid conditions the changes are small but visible, so I would improve the sentence: This suggests that water vapor affect the gas-phase mechanism or partitioning into the aerosol phase only to a small extend.

Figure 5 a + b: The marks for beta-pinene and mass aerosol formed are mixed up.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 18039, 2008.

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