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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.

J. L. Fry et al.

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We thank the reviewer for constructive comments on our manuscript. Below we describe our efforts to address each of the reviewer comments. Reviewer Comment: 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.

Response: 1) We plot the sum of NO3 and N2O5 because the fast equilibration between these two means that their sum effectively represents the total NO3 available for reaction. To show the partitioning between these two, NO3 will be added to Figure 1 panels a and b as a separate trace in the revised manuscript, as suggested by the reviewer.

Reviewer Comment: 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.

Response: We will also add the SMPS mode aerosol diameter (nm) to Figure 1 panels a and b in the revised manuscript. This would give people the additional information to extract growth rates etc. from the data, without crowding the figure with the full color size distribution plots.

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

Response: We thank the reviewer for comments about uncertainties in the regional/global extrapolations of SOA yields. In the revised manuscript, we will add text emphasizing these uncertainties and clarifying the assumptions we have made.

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Interactive Comment

Finally, all technical comments will be addressed in the revised manuscript, and we thank the reviewer for careful reading.

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

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Interactive Comment