

## ***Interactive comment on “A review of Secondary Organic Aerosol (SOA) formation from isoprene” by A. G. Carlton et al.***

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Response to Reviewer 1

1) Chapter 5.2 has been revised and improved to address Reviewer 1's concerns. Specific examples of the revisions are outlined below.

We agree with the Reviewer that differences relative to total organic particulate mass are an important consideration. When employing yield values corresponding to the upper and low bounds, we now calculate the percent of total organic carbon (OC) represented by the difference in isoprene SOA mass concentrations (>10%). Additionally Figure 4 now has part “a” (the original absolute difference) and part “b” (percent of total OC). We choose to still present the data in absolute concentration as a reference to

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provide context for the % differences that can be very large when predicted concentrations are low.

We agree that a NO<sub>x</sub>-dependent formulation for isoprene SOA is very useful for the community and present the calculation, according the method presented by Presto et al. (2008). Because this is a review paper, we think the application of a new formulation is beyond the scope of this manuscript but we present the equations in the hope the community applies it in future isoprene SOA research.

2) We have reviewed our final version of the manuscript and use “POM” for primary organic particulate matter and “PM” for particulate matter.

3) We have altered the text in the manuscript after line 14 on p. 8277 to address the Reviewer's concern that “OH exposure” is more appropriate than “OH level”:

"Such differences are reflected by variations in OH exposure (OH concentration multiplied by reaction time), which serves as an excellent metric for the extent of reaction. However it is extremely difficult to estimate OH exposures within chambers, since OH concentrations may change dramatically over the course of an experiment, and are typically not monitored once the initial precursor has decayed away. Time-dependent measurements of OH concentrations in SOA formation experiments would allow for the estimation of OH exposures, thereby enabling the comparison of different laboratory experiments."

4) The Reviewer points out that organic aerosol level does not lead to scatter in the yield data because the SOA yield depends on aerosol loading with a known physical basis. We have changed the wording from “scatter in yield values. . .” to “differences between measured mass. . .”.

5) In the manuscript we had explicitly defined high-NO<sub>x</sub>/low-NO<sub>x</sub> on p. 8276, lines 3-5: “experiments in which RO<sub>2</sub>+NO dominates (high NO<sub>x</sub>). . .whereas those in which RO<sub>2</sub>+HO<sub>2</sub> (low NO<sub>x</sub>). . .”

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The Reviewer correctly points out though that the limit of the range is the no-NO<sub>x</sub> experiments. We have replaced “dominates” with “is the only pathway by which RO<sub>2</sub> reacts” and we replaced “low-NO<sub>x</sub> with “NO<sub>x</sub>-free”.

6) All identified technical corrections on pages 8263, 8281, 8273, 8280, 8281, 8285, 8288, 8291 and 8293 were corrected according to the Reviewer’s suggestions.

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 8261, 2009.