

Interactive
Comment

***Interactive comment on “Glyoxal processing
outside clouds: towards a kinetic modeling
framework of secondary organic aerosol formation
in aqueous particles” by B. Ervens and
R. Volkamer***

Anonymous Referee #1

Received and published: 9 June 2010

General Comments: The authors have done an extensive literature search. They have characterized and synthesize theory, laboratory results and modeling approaches well and bring them together in important and meaningful ways. Their assertion that observed O:C ratios in ambient SOA can be explained by models considering these processes in a crucial point. This is an important paper and I recommend it for publication in Atmospheric Chemistry and Physics. The authors should consider the following comments.

Specific Comments: 1.) Abstract: line 10 Processing in aerosol particles yields 2-3

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orders of magnitude more SOA than dilute cloud droplets. Do the authors mean more SOA mass for a particular time period, or for the lifetime of an atmospheric particle versus the lifetime (or contact time) of a cloud droplet? Is this true in urban and remote environments? This should be clear in the abstract.

2.) Units and explanations for the various terms in all of the equations would be helpful, in particular because of the combination of gas- and aqueous-phase terms which makes balancing units complicated (e.g., Eqn 10 in section 3.1).

3.) As the authors mention, SOA in atmospheric models typically consider only absorptive partitioning. In general, these models neglect adsorption of semi-volatile material. Adsorption is proportional to available particle surface area. Can the authors describe their results that demonstrate a dependence on particle surface area in the context of adsorption? Would adsorption be insufficient to explain all results?

4.) An important and useful aspect of equation 10 is that it removes the need to define certain particle parameters (e.g., radius of the H₂O part only, surface area) which are difficult to accurately calculate and typically not characterized well in atmospheric models. This makes Equation 10 appealing to atmospheric modelers and the authors may want to highlight this point.

5.) Equation 10: The authors' work demonstrates an increase in predicted SOA mass when there is increased liquid water available (high LWC). Equation 10 as written shows $d[\text{SOA}]/dt$ inversely proportional to LWC. This is not intuitive.

6.) In Section 4.1, the authors apply the same uptake parameters to dilute and highly concentrated solutions. Can the authors provide an estimate of whether in highly concentrated "solutions" the uptake would be higher or lower, that is, does this approximation provide a higher or lower bound?

Minor Technical Comments: Figure 6: the maximum value in the y-axis for panels "c" and "e" is half that of the other panels and for "f" and "g", each y-axis is orders of

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magnitude different. Can authors make the y-axes the same, or put a note in the figure caption?

Figures 3 and 7 would be easier to read and interpret in color.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 12371, 2010.

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