

Interactive comment on “Insights into secondary organic aerosol formed via aqueous-phase reactions of phenolic compounds based on high resolution mass spectrometry” by Y. Sun et al.

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This paper describes the results of a laboratory study of aqueous-phase photooxidation of phenolic compounds. Formation of lower volatility products, such as oligomers and oxalic acid, was observed in illuminated solutions. Reaction products exhibited similar characteristics as field aerosol samples. This paper is the first laboratory investigation of SOA formation from aromatic compounds through cloud processing. Though the paper is thorough and the results are presented clearly, we have a few comments for the authors to consider before publication.

1. Field studies suggest that the concentration of phenol in cloud/fog/rain samples is

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usually less than 1 micromolar (Harrison et al., 2005). Formation of oligomers is less favored at lower precursor concentration (Tan et al., 2009). The authors should justify the high precursor concentrations they used in experiments, or they should address other possible conditions in the atmosphere (i.e. aerosol water).

2. Was the solution saturated with air? Oxygen is an important radical scavenger. In the proposed mechanism (Figure 5), the authors should consider the competition between radical-radical reactions and radical - O₂ reactions.

3. The authors indicate that the phenol + simulated sunlight did not produce SOA in the caption of Table 1. In contrast, SOA material was observed in the phenol + OH radical experiment. However, the authors conclude that “The mass spectral patterns and ion compositions of SOA formed from the same precursor are similar for both of the experimental conditions (i.e., light only as well as light plus $\dot{\text{A}}\text{uOH}$)” at pg. 2922 line 29. As written, the importance of OH radical in the SOA formation from phenol is not clear. In general, are the yields higher in the presence of OH? Is formation faster in the presence of OH?

4. This work uses the photolysis of 100 micromolar H₂O₂ by simulated sunlight to produce OH radical. Is it possible for the authors to estimate the OH radical concentration in reactions? 100 micromolar H₂O₂ is indeed atmospheric relevant. However, this H₂O₂ concentration could lead to lower OH radical concentration than atmospheric conditions. Gas-to-droplet partitioning, nitrate photolysis, and Fenton chemistry are important OH radical sources in cloud droplets, while the direct photolysis of H₂O₂ could be a minor source. If the OH radical concentration is lower than cloud physics model predicted (Jacob, 1986), it could explain modest differences between experiments with and without OH radical. As OH radical can promote the formation of oxalate, it is also possible oxalate could be a more important product under atmospheric relevant conditions.

5. Pg. 2919 line 2, the authors took the sample when half of the precursor reacted.

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It would be useful if the authors can provide the approximate duration for each experiment. I guess the duration should vary for different precursors under different conditions. For example, the degradation of precursors could be faster when OH presents. This duration could also provide some insights into reaction kinetics.

6. Pg. 2927 line 15, this conclusion might be too broad and strong. Biomass burning could produce VOCs besides phenolic compounds. This work did not investigate the aqueous-phase chemistry of other biomass burning VOCs. The analysis only suggests that cloud processing of phenolic compounds could explain the observed SOA components very well, as indicated in the next sentence.

7. How water soluble are these precursor compounds? What are their Henry's law constants and how do they compare with other SOA precursors?

8. Drying – first paragraph of results – was this also done with the dark control? What was found?

References:

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Jacob, D.J. 1986. Chemistry of OH in Remote Clouds and Its Role in the Production of Formic Acid and Peroxymonosulfate. *J. Geophys. Res.* 91: 9807-9826

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