

## ***Interactive comment on “Aqueous-phase photochemical oxidation and direct photolysis of vanillin – a model compound of methoxy-phenols from biomass burning” by Y. J. Li et al.***

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Received and published: 18 December 2013

Li et al. report very interesting results concerning the aqueous phase oxidation of vanillin, an important biomass burning emission. Oxidation by UV and by OH are both tested and the generation of secondary organic aerosol (SOA) mass and its composition examined. The experiments are well designed and the data analysis is robust and insightful. The findings are highly relevant to ACP readers and fill a current gap in knowledge concerning aqueous SOA formation from cloud processing of prescribed and wild fire emissions. I commend the authors on this impressive piece of work and have only a few suggestions and comments to improve a final version of the

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manuscript.

1. The authors use UPLC/ESI-TOFMS to report the composition of some product species. More information is needed regarding how the authors went from the elemental formulae provided by the TOFMS to the structures they present. Mention is made of fragmentation loss, but the analytical description provided in the supplement does not address the fragmentation approach used. I assume in-source fragmentation was employed, but this should be explicitly stated and relevant details provided.
2. On a related note, is there a reason the authors did not scan below  $m/z$  50 in the ESI-TOFMS analyses to look for the presence of smaller product molecules?
3. Please mention the volume of solution used for the reactor experiments.
4. The authors conclude in section 3.6 that vanillin loss through aqueous oxidation by UV light can be as important as vanillin oxidation in the gas phase. This claim needs to be better justified. In particular, I question whether the UV exposure (both wavelengths and intensity) utilized in the lab experiments is comparable to atmospheric conditions. If it is not comparable, the authors should explain how they scaled their aqueous results for comparison to atmospheric gas phase oxidation rates. They did a good job of this for vanillin's aqueous oxidation by OH but did not address it for the UV exposures.
5. As mentioned by another commenter, the authors should include information about the pH of reaction solutions in their experiments. Ideally this should be monitored throughout aqueous aging. At a minimum, it should be measured at the beginning and end of each experiment, if not controlled. Please provide pH information and discuss the possible role of varying pH in more realistic atmospheric clouds on the aqueous chemical processing observed here.
6. Please specify the “dry conditions” RH used in the growth factor experiments and discuss whether the tested particle types are likely to be completely free of water at that RH.

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7. “bond” should be changed to “bound” in line 14 of the supplement
8. “maker” should be changed to “marker” in line 4 of p. 27644
9. I prefer the use of the term “saturation vapor pressure” to “saturated vapor pressure,” but that may simply reflect a difference in British vs. American English
10. Change “form” to “formed” on line 17 of p. 27646

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 27641, 2013.