Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-749-RC1, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Degradation Kinetics and Secondary Organic Aerosol Formation from Eugenol by Hydroxyl Radicals" *by* Changgeng Liu et al.

Anonymous Referee #1

Received and published: 18 November 2018

General comments:

In this manuscript, the authors report the first measurements of the rate constant for the reaction of eugenol, an atmospherically abundant methoxyphenol, with the hydroxyl radical in the gas phase. The results are placed in the context of other previously investigated methoxyphenols, including a helpful discussion of substituent effects. The authors also present a detailed characterization of the SOA yield and its response to SO2 and NO2, including a surprising enhancement in SOA yield due to the presence of NO2, which may apply to other methoxyphenols. The experimental work is thorough and precise, with appropriate controls, and the manuscript is well organized. I

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recommend the manuscript for publication following minor revisions.

Specific comments:

144: Though photolysis does not contribute to the decay of eugenol, could it contribute to the evolution of the oxidation products, either in the gas or particle phases? Though it is not a focus of this study (one can imagine forming SOA in one OFR, scrubbing any remaining ozone, and then irradiating the products in a second OFR), perhaps the possibility of photolysis of the oxidation products should be acknowledged at the end of this paragraph.

164: I enjoyed the comparison of the experimental and AOP WIN-predicted rate constants. I wonder if the comparison should be placed in the context of other methods of prediction. For example, the DFT-predicted rate constant for the reaction of guaiacol with the hydroxyl radical (DOI: 10.1002/poc.3713) is about 1.6 times greater than the experimental value (DOI: 10.1021/jp1071023). In this context, the present agreement, with a predicted value about 0.8 times the experimental value, seems quite good.

252: The lack of kinetic limitation to condensation is very interesting. Could this observation be related broadly to the viscosity of the SOA derived from eugenol and guaiacol? How does the present relative humidity of about 44% compare to that in the previous OFR and smog chamber experiments discussed in the comparisons?

365: Perhaps the detailed discussion of the effects of NO2 on the SOA yield would benefit from a mechanistic schematic.

427: I agree that it is very likely that the SOA derived from eugenol is light-absorbing. Other products, in addition to those containing nitrogen, could conceivably contribute to the proposed absorptivity. For example, products of oligomerization like biphenyls have been observed in the aqueous oxidation of phenolic species (DOI:10.5194/acp-14-13801-2014), and this pathway is likely relevant in the highly-concentrated aerosol phase.

Technical corrections:

77: In the phrase "a type of methoxyphenols", methoxyphenol should be singular. Perhaps this phrase is redundant and could be omitted.

115-116: Two instances of "approximate" should be "approximately".

126: "Vaccum" should be "vacuum".

199: In the caption to Figure S4, perhaps explain that the arrows indicate the maximum values (i.e, those listed in Table 2).

217-218: This phrase should be reworded to better reflect that the fragmentation occurs in the particles and that the products subsequently volatilize out of the particles.

238: This phrase is slightly confusing, since some measure of composition is determined using the AMS.

368: Should be "cyclic".

383: Should be "radicals".

414-415: This phrase should be reworded to give, for example, "more attention should be paid to SOA formation..."

442: Should be "formation".

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