

Interactive comment on “Unexpected high yields of carbonyl and peroxide products of aqueous isoprene ozonolysis and implications” by H. L. Wang et al.

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There is a ever growing interest in atmospheric aqueous phase chemistry in recent years, and this paper gives interesting insights in aqueous ozone chemistry. I think this paper would be more robust if the authors put more caution in extrapolating experimental findings.

This paper conclude that "unsaturated VOCs ... may potentially contribute significantly to aqueous SOA in the atmosphere" (pg 6435, line 9). However, as the authors acknowledged, such reactions could happen (and I think they would only happen) "on the atmospheric surfaces of wet particles and plants". This limitation might make current

C1611

conclusions questionable. Can the authors determine if such reactions are significant atmospheric sinks for unsaturated VOCs compared to gas phase reactions? Are these reactions as important as aqueous oxidation of other studied species (e.g. glyoxal, phenols)? I agree these reactions could happen in the atmosphere, but the current conclusion is too bold before some critical questions are answered.

This paper also suggest that ozonolysis of poorly water soluble VOCs could "give an implication for partly explaining the "missing" OH radical source over the tropical rain forest" (pg 6435, line 10). The observed OH radical discrepancy was in the gas phase. If this chemistry happens on plant surfaces, the mechanism for produced OH radicals to enter free troposphere is quite uncertain; if this chemistry happens on wet aerosols, the significance of such chemistry would be highly dependent on uptake efficiency and reaction time scale compared to other sinks. As the paper did not discuss these questions in depth, it is unsafe to make such strong statements.

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