

Interactive comment on “Aqueous chemistry and its role in secondary organic aerosol (SOA) formation” by Y. B. Lim et al.

Anonymous Referee #3

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The subject manuscript, Aqueous chemistry and its role in secondary organic aerosol formation, provides an overview of the role of aqueous-phase chemistry in forming secondary organic aerosols (SOA), focusing on radical versus non-radical reactions and cloud-water versus aerosol-water conditions. While the manuscript potentially offers advancements in the field, it is somewhat difficult to discern the novel aspects of this work. The title and much of the paper suggest a review article, however the abstract, and “Experimental section” and “Results and discussion” headers suggest otherwise. It is recommended that the authors address this issue (research vs. review article), discussed in further detail below, before the manuscript is accepted for publication in ACP.

Content and structural comments: While the abstract of the subject manuscript in-

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dicates the literature will be used to describe radical (OH) and non-radical aqueous chemistry at cloud-water and aerosol-water relevant concentrations, the extensive presentation of background material in sections 1 and 2 is disorienting. The presentation of so much background material in the introduction obscures the most relevant details and distracts from the objectives and outcomes of the current work. It is suggested that most of the background material be combined with that in section 2 (readers will be ready for the literature review in section 2, primed by sentence on lines 7 and 8, p. 14167). It is also suggested that in combining parts of section 1 with section 2, care be taken to remove the unnecessary repetition and extraneous detail currently in the manuscript.

A lot of the introduction is devoted to measured O/C ratios for SOA, which is again brought up in the atmospheric implications section; however, it isn't clear how the subject manuscript contributes anything to that discussion (unless it is in the context of a review article, in which case it doesn't seem necessary to include in the atmospheric implications section).

While the title and sections 1 and 2 of the paper suggest a review article, the abstract does indicate new experiments and a modeling application will be discussed. In the experimental section, the authors say that the paper “draws heavily” on experiments presented in Tan et al. (2009) and it does not appear that any new experiments were performed for this work. The results of Tan et al. (2009) may be more appropriate in the introduction, as they seem to be what prompted the subsequent data analysis and kinetic modeling presented in the results and discussion. In general, a better description of how the present work builds on previous work of the group (e.g., as presented in Tan et al. (2009)) would be very useful to have in the introduction. Regarding the experimental section, it is recommended that the authors: 1) rename the section (data analysis and kinetic modeling?), unless new experiments were actually performed for this work; and 2) provide the details of the work done specifically for presentation in this manuscript. Some of those details, e.g., regarding the kinetic modeling, seem to

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be buried in the results and discussion section.

Other comments: p. 14162, sentence 14: suggestion to delete “at least”; sentences 15 and 17: concentrations of? water? glyoxal?

p. 14166, sentence 15: suggestion to replace “these” with “those”

p. 14169, sentence 9: replace period before OH with a comma

p. 14170, sentence 6: suggestion to replace “plenty of” with “sample availability is not limited for chemical analyses” or something to that effect

p. 14172: suggestion to either combine sections 2.2.4 and 2.2.6, or put them in succession, since an organosulfate, as noted, is an ester derived from an inorganic salt

p. 14173, first paragraph: Is it possible that there is an “acidity threshold” at which acid-catalyzed uptake of glyoxal can be observed, but that may not result in a direct correlation between pH and uptake coefficients below that threshold pH value?

p. 14177, paragraph starting on line 3: The irreversibility of products formed from OH radical reactions is included in the section with the header “Reversibility of oligomers formed by acid catalyzed pathways”. It is suggested that the paragraph be moved to a section regarding aqueous-phase chemistry involving OH radicals.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 14161, 2010.

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