

Interactive comment on “Influence of aerosol acidity on the chemical composition of Secondary Organic Aerosol from β -caryophyllene” by M. N. Chan et al.

Anonymous Referee #2

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This manuscript describes experiments aimed at understanding the mechanistic details of the formation of secondary organic aerosol (SOA) from beta-caryophyllene in the presence of acidic seed particles. The authors employ an environmental chamber reactor and use ultra performance liquid chromatography/electrospray ionization time of flight mass spectrometry (UPLC/ESI-TOF) to detect and identify many of the individual chemical constituents of the SOA. Several new compounds are identified, including the first organosulfates and nitrates observed in SOA derived from a sesquiterpene. Based on these results, three compounds found in ambient SOA are identified; it is suggested that these compounds can serve as tracers of beta-caryophyllene reactivity. Therefore, the work is quite appropriate for ACP, and is a significant contribution to the

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SOA literature.

The work has been carefully planned and executed, and the analysis is logical. While the mechanistic arguments are by necessity quite speculative, I found the explanations to be clear and appropriately tentative. However, there are some issues related to the clarity of the prose of the manuscript as well as some missing details that the authors should consider before preparing a final version of the manuscript.

A general question: I understand that the main goal in the work was to try to study beta-caryophyllene-derived SOA that would be as similar as possible to the ambient SOA measurements. However, especially since the three ambient SOA components are identified as two second generation and one third generation ozonolysis product, I wonder why separate ozonolysis experiments weren't carried out (i.e., no photochemically generated OH and an OH scavenger added to shut off any OH-initiated chemistry from secondary sources). Since many of the other identified products are speculated to arise from OH chemistry, it would have helped with the development of proposed mechanisms if the separate ozonolysis experiments had been carried out. Is there some reason that the authors didn't carry these out?

Specific issues:

p 29251, line 9, typo: “increase of” should be “increased”

p 29253, line 24, comment: It would be helpful if the authors reported the effective H+ concentration for the seed particles.

p 29254, line 6, comment: It would be helpful if the authors also reported an effective H+ concentration for an average SOA particle.

p 29257, line 26, comment: A reference should be given for the proposed RO₂ + HO₂ → carboxylic acid reaction.

p 29259, line 16, comment: Do the authors have any idea what the HO₂ levels are in their experiment? It seems surprising to me that an RO₂ + HO₂ reaction would

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dominant in the presence of 60 ppb NO.

p 29260, line 17, typo: "oxoium" should be "oxonium" p 29263, line 16, comment: Do the authors mean that the gas/partitioning coefficients must be artificially increased to accurately model experimental data?

p 29264, line 21, comment: It is stated that the Li et al identified all three ambient SOA components as second generation ozonolysis products. As my general comment above indicates, from Schemes 2 and 3, I interpret the formation of beta-dihydroxynocaryophyllon aldehyde as a third generation ozonolysis product. Am I incorrect, or is the present interpretation at odds with Li et al.? I also don't see how Scheme 4 has anything to do with rationalizing the three ambient SOA compounds, or why any OH chemistry is required to rationalize them.

General comment about tables and schemes: When printed on a regular piece of paper the chemical structures are were too small to read. Even when viewed as an electronic document and enlarged to fill the screen of my 25" monitor, it wasn't easy to make out the chemical structures in the schemes.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 29249, 2010.

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