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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Particle mass yield from $\vec{\beta} - caryophylleneozonolysis''byQ.\ Chenet\ al.$

Anonymous Referee #2

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This work investigates aerosol formation by beta-caryophylene ozonolysis, under conditions of excess ozone. By varying the level of excess ozone, distinction can be made between fast-forming low-volatility and slow-forming higher-volatility second-generation products contributing to the total mass yield. This is confirmed by analysis of the O:C ratio and density of the aerosol. A VBS model is derived that could be used in regional or global models.

The paper is well written and organized, scientifically sound and relevant. My main concern is that the large wall-losses could have an affect on the results. I recommend publication in ACP after the following comments are addressed:

Remarks: p. 30538. Given the high wall-losses that are encountered here, should it be recommended then that aged bags are used and/or reneutralization of seed particles is performed, for future experiments?

p. 30538 and Figure S5. The authors have made a thorough effort estimating the amount of SOA lost to the walls and have included this amount in the corrected wall loss yields, which as a consequence increase by a large factor. They show that wall losses are similar (and large) for the series of experiments conducted in the same time period in the same teflon bag. Still, the difference in amount of SOA wall loss between two experiments might also have an impact on the gas phase chemistry or on thermodynamic behaviour of the condensable products, which in turn might impact the SOA concentration. Partitioning to the liquid phase might be increased/decreased in an experiment in which wall losses are high compared to one in which they are low. Could the authors speculate on the possible consequences of such a difference in wall loss? The authors have already partly addressed this problem by indicating in red the two experiments for which wall losses were lower. There does not seem to be a trend obvious from these two experiments. Still, for the experiment 27, in which wall losses were five times lower, the wall loss corrected SOA yield is significantly lower (28.7%+-1.3%) than in the corresponding experiment 18 with higher wall losses (41.4+-3.6%). Also, compared to exp. 18, both the SOA density and the O:C ratio in exp. 27 are lower. This suggests an impact of wall loss on the chemical composition of the SOA. Therefore it seems that, only given the presented data, it is not yet an established fact that the presence of a large wall loss will not have a significant impact on the final corrected SOA mass which is obtained. This should be recognized in the main article.

The discussion on wall-loss focuses on the SOA. What about the wall-loss of gas-phase products? Is there evidence that this can be neglected? The work of Matsunaga and Ziemann (2010) shows that for Teflon walls, gas-wall partitioning can be very important and species-dependent. A discussion on the possible impact of wall-loss of gas-phase products, with reference to Matsunaga and Ziemann (2010), should be inserted.

p. 30541, line 5-6. "Our data are consistent with the plausible suggestion that P254-1 has been incorrectly assigned as..." Which data? Simply the fact that P254-1 increases at higher O3 levels, or is there other data that suggest that P254-1 is second-

generation?

p. 30542, line 17. "This conclusion is supported by...". This is only true if P302 has a dominant contribution and P270-1 a negligible contribution. Otherwise it does not prove that. Do you have an estimate of the contribution of the different compounds?

end of p.30545: "Nevertheless, the comparison between our data set and the parametrization of Carlton et al. (2010) suggests a possible underestimate by that parametrization of 100% to 300% for organic particle mass concentrations less than 3 μg/m^3." Availability of data in this lower SOA mass range is indeed a great improvement over an extrapolation from data at higher masses, as this might be responsible for a large uncertainty. However, when comparing SOA yields for beta-caryophyllene between the present study and the study of Griffin et al. (1999), it should be recognized in the article that the different photochemical conditions in these experiments might lead to different SOA masses. Although in the photo-oxidation experiment of Griffin et al. (1999) beta-caryophylene will probably react with ozone too, the primary oxidation products might also undergo reactions with OH instead of with ozone. The presence of NOx might also have a significant impact on SOA yields, although it is not clear whether for sesquiterpenes this might be a reducing influence, as for monoterpenes, or an increasing one (Ng et al., 2007). In the real atmosphere the secondary products might also partly react with OH instead of with ozone. Photolysis might also play a significant role. Therefore it is not yet certain whether the current parametrization for dark ozonolysis ageing experiments including an OH scavenger will exactly represent all aspects of beta-caryophyllene aging in the real atmosphere. The reader should probably be warned about this.

Fig. 7, comparison to literature. There is a comparison with Lee (2006) and Griffin (1999), but not with Jaoui (2003), Grosjean (1993), or Hoffmann (1997), present in Table 1. Include the points or motivate why this is not appropriate.

Technical remarks:

p. 30539, line 15. Put spaces between "and" and inequalities. line 16-17: strange sentence construct. Probably "show" must be "showing". line 23: second "tau_exo(50)" should be "tau_exo(200)"

Table 1. "Huffmann" should be "Hoffmann"

References: Matsunaga, A. & Ziemann, P. J. Gas-Wall Partitioning of Organic Compounds in a Teflon Film Chamber and Potential Effects on Reaction Product and Aerosol Yield Measurements Aerosol Science and Technology, 2010, 44, 881-892

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 30527, 2011.