Comments on "Particle mass yield from b-caryophyllene ozonolysis" by Chen et al.

The paper is very well written; it's well organized and details are mostly well explained. With experiments at different levels of ozone and b-caryophyllene carried out, the results are relevant. Parameterization of the aerosol yield data is valuable for modelers although in taking such parameterization and applying it to all sesquiterpenes the inherent assumption is that ozonolysis of all sesquiterpenes behaves like b-caryophyllene. I believe the analysis is sound; the only concern I have is about collection efficiency of AMS being 1 (item 3 below). Since the yield calculations are based on AMS measurements, it is fundamental to the findings of the paper that AMS quantification is done properly. Because of this, although I am suggesting publication of the paper after my 'minor' comments and questions are addressed, I would still like to review the response from the authors before publication.

Comments on content:

1. Line 12, p. 30532: polydispersed seed was used to 'minimize' (rather than 'eliminate') contribution of nucleation particles, so was nucleation observed in any of the experiments? If so, how do you think that affects the results?
2. Lines 1-5, p. 30534: as written, it sounds like the only way to get reliable yield data from b-caryophyllene+O3 (in excess) expts is by doing the expt in a CMFR. Isn't it true that if one waits long enough in a batch chamber with excess O3+b-caryophyllene, the final yield is practically what can also be obtained from a CMFR set up? If so, add a sentence to clarify this.
3. Line 16, p. 30534: Based on results from many lab and ambient studies, I'm very surprised that for lab generated SOA at RH=40%, CE=1 seems appropriate based on the SMPS and AMS closure. Have the authors seen this for any other SOA system? Can there be other errors (for example, errors in IE calibrations, losses in SMPS sampling line, errors in RH correction for SMPS-based mass before comparing to AMS mass (because AMS sees mainly dried particles while SMPS would see the water associated with the particles), differences in size range of two instruments, etc.) that would lead to the apparent CE to be 1?! How is the AMS-SMPS comparison for other systems?
4. Line 25, p. 30535: what resolution for AMS mass distributions was used?
5. Line 23, p. 30539: how is the lower-end estimate for $\tau(CMFR)/\tau(exo) = 0.1$ determined? Is that basically just a number $<<1.8$?
6. Line 25-26, p. 30540: The argument that the RO2 channel wins over the O3 channel at higher O3 is counter intuitive since RO2 is secondarily produced while O3 is inherently higher at higher O3. Can you add some kinetic calculations to support your hypothesis? Also, can there be other losses of p238 at higher O3?
7. Line 28-29, p. 30540: It's not clear why "the nearly completion of P302 from P252-1 at 50 ppbv ozone" can explain lack of a significant change in P252-x??! Please explain better the relation between P302, P252-1 and P252-x. Also, as written, the section of the sentence I've quoted here in "" is not clear- do you mean 'nearly complete formation of P302 from P252-1'?!

8. Line 7-8, p. 30541: Is the evidence for P254-1 to be 2nd generation product the fact that its mass fraction goes up with increase in O3? If so, clarify that. If not, what is the evidence then?

9. Lines 8-10, p. 30542: If the height of the green bars in Fig. 5 indicates the ultimate particle yield, it means that at Morg=1 ug/m3, yield is 10% regardless of the ozone level while at Morg=10 ug/m3 yield is 25-30%, and at Morg=100 ug/m3 yield is 50-80% depending on the excess ozone level. I think this is important to highlight since most ambient concentrations of Morg are ~1-10 ug/m3 and at these levels, it seems ozonolysis particle yield is not as high as one would think.

10. Line 23, p. 30542: With quantification of the 15 products identified by UPLC-EST-ToF-MS, can the authors estimate the yield of each product at different ozone levels, rather than speculating that P270-1 has a low mass yield?

11. Line 26-29, p. 30542: How can the authors be sure that it's 2nd generation products at C*=100 ug/m3 that partition to the aerosol phase and not further generation products? Since contribution of C*-class to particles goes up with increase in ozone, it is quite plausible to think that further oxidation of 2nd generation products leads to increase in C*-class in the particles. Please clarify.

12. Line 4-14, p. 30544: I find the discussion in this section repetitive. Oxidation state and O/C levels give for the most part the same level of information. In page 30543, same conclusions about what type of products exist at different Morg and O3 levels were drawn from O/C measurements. I suggest combining these two sections by just mentioning the corresponding oxidation state for the measured O/C values in page 30543 and removing most of the discussion on page 30544.

13. Line 7, p. 30545: For clarity, I would say that organic mass 'inversely' correlates with oxidation state and material density.

14. Lines 14-30, p. 30545: since Griffin et al. chamber data are based on OH reactions and as the authors indicate on p. 30544 OH oxidation may move the products to those at lower volatilities and therefore increase the particle formation yield, I don't think it's appropriate to compare the current results with models that do use yield data of Griffin et al. An additional discussion here regarding this is needed. Also, for comparison, you may want to concentrate more on panel a since that's where most of the ambient Morg range is.

15. Table 2: what is unique about expt 2?! The O/C of this expt is similar to many other expts while the density is significantly higher.

Editorial Comments:

1. Remove 'nevertheless' in line 22 of abstract and line 8 of p. 30546.
2. Figure 4 caption- it's important to clarify that the fractions are based on the mass of the 15 compounds that were identified (not all the organic mass). I suggest adding the

following: "(a) Mass fraction of first- and second-generation products as identified by UPLC-EST-ToF-MS for increasing ozone concentrations"

3. Figure 7- make the modeling lines thicker or the whole plot bigger- it's hard to distinguish some of them at the current size of the figure. Also, having the oxidation state here is confusing. Since O/C values are already plotted in Figure 6, I suggest adding the points in OS-space as the right-axis in Figure 6.