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Interactive comment on "Enhanced secondary organic aerosol formation from the photo-oxidation of mixed anthropogenic volatile organic compounds" *by* Junling Li et al.

Anonymous Referee #2

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General Comments The reviewed study investigates combining precursors in SOA experiments and the effect of the combination in its ability to produce SOA. Interestingly the authors observe that combining an alkane and aromatic precursor in their experiments results in an SOA yield that is greater than the weighted average of each precursor's individual SOA yield. This could be from a change in partitioning or from new chemical pathways that are activated only in the combination of dodecane and TMD. The authors state the cause is from the occurring chemistry, and not just partitioning. This is justified by looking at molecular species with ESI-TOF-MS observed in in experiments as well as the UV-Vis absorbance of the resulting SOA extract. The authors don't make the strongest case for what specifically changes in the occurring chemistry

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Discussion paper

to generate more SOA, but they do point to measurable differences, such as unique ESI-TOF-MS peaks in the mixed experiments. Overall, the results are interesting and worthy of publication. Specific Comments 1. In Figure 1 a and b you show that the corrected mass is substantially greater than the experimentally measured mass, by at least an order of magnitude it appears. This is concerning as there is undoubtedly some error in the correction that is used. Could the authors please estimate the error associated with the mass correction they use and how that propagates into the "mass corrected" values in Figure 1. 2. In Figure 5 it is difficult to see the difference between peaks because of the x-axis scale. It would be more informative to make the x-axis from 200 – 300 nm, and simply note in the caption that absorbance at wavelengths >300 nm is negligible. 3. It seems erroneous to say the observed peaks around 205 nm are strictly due to carboxyl. Given the presence of NO in your experiments, you will have nitrate functionality in your products. Nitrate absorbs strongly around 210 nm, but this can be shifted from neighboring functional groups, etc. 4. Based on figure 5, you cannot say (line 243) that the mixed AVOCS forms more carboxyl compounds. Because the mixed experiments contains more aerosol mass, you would expect the absorbance to be greater even with the same yield of carboxyls (or nitrate, see comment 3) relative to the non-mixed experiments. 5. The authors note a handful of peaks from ESI that only appear in mixed experiments, but in Figure 4 it is not especially clear that there are many peaks in panel C that are not in panel A and B. Does the ESI have similar sensitivity for all observed species? Or can sensitivity vary greatly between species? Please address this, it would help the reader to interpret figure 4. 6. Also, as it is difficult to see the magnitude of the peaks unique to the mixed experiments in panel C - is it possible that the unique peaks are present in unmixed experiments but just under detectable limits, and upon producing greater aerosol in the mixed experiments, the unique peaks were elevated to above detectable limits? Perhaps a useful way to address this would be give the intensities of the unique peak relative to the larges peak of the spectrum, in addition to addressing comment 5. Technical corrections. 6. Line 84: Add "The" before "OH precursor" 7. Line 84 and 86: Correct subscripts in molecular formulas. 8. Line

219: m/z is commonly italicized, m/z, this should be corrected throughout document.
9. Line 243: “formes” should be forms

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