

## ***Interactive comment on “Significant source of secondary aerosol: formation from gasoline evaporation emissions in the presence of SO<sub>2</sub> and NH<sub>3</sub>” by Tianzeng Chen et al.***

### **Anonymous Referee #2**

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Chen et al. propose a study presenting the impact of NH<sub>3</sub> and SO<sub>2</sub> on secondary organic aerosol formation produced from the oxidation of unburned gasoline vapors. While this study presents interesting results, some aspects (see comments below) remain unclear and/or should be better discussed. One of my main comment is the relevance of the mix of VOC studied in this work when the authors claimed to study relevant unburned gasoline vapors. Indeed, the emission inventory proposed by Liu et al appears very different than the mix of VOC employed in this work. Overall this can lead to an overestimation of the importance of this potential SOA source.

Lines 49-53: If the highest concentrations of NH<sub>3</sub> were observed in urban areas, is the

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main source from agricultur or vehicle emissions?

Lines 66-70: Split the references and cite proper references. For instance, the work from Kamens and al. should be better acknowledged.

Lines 83-86: The authors should better discuss this section and not overestimate the potential of VTE in producing aerosols. According to Liu et al., 2017, the VOC emitted from the evaporation are mainly dominated by short chain alkane (e.g., Pentane, butane,...) which have a very limited impact on SOA formation. However, the authors mainly discussed/focused on the oxidation of aromatics. In addition, important papers are missing and should be discussed; e.g., Pye and Pouliot 2012, Tkacik et al., 2012.

Lines 121-122: Please change to the widely used acronym (PTR-TOF), (i.e., Yuan et al., 2017 Chem Review).

Lines 130-131: Is the density comparable to other studies?

Lines 191-192: The authors should be careful here. The PTR can measure a certain (small) subset of OVOC produced from a given reaction. Therefore, it is not because the analytical technique was not able to identify any significant differences that mean SO<sub>2</sub>/NH<sub>3</sub> did not significantly impact the gaseous phase. As an example, a large formation of H<sub>2</sub>SO<sub>4</sub> is mentioned in the paper. In addition, the authors should propose a more quantitative comparison (as realized with the AMS data) and not only briefly compare the mass spectra.

Line 206: The background of ammonia appears quite subsequent. Do the authors have an explanation? Is it an expected background in chamber experiments?

Fig.3 the color scale is hard to read please modify it.

Lines 213-219: In this section, there are some inconsistencies with the literature. The number (i.e., up to 50 times more particles) reported in earlier studies are different than in the work proposed by Chen et al. However the concentration of SO<sub>2</sub> and the precursors were similar. For example, while the SO<sub>2</sub> concentration was multiplied by 4

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the formation of particles didn't increase significantly. The authors should explain such discrepancy and discuss/compare their results to the existing literature.

Lines 227-232: This is in line with my previous comments. How do they explain such a moderate increase between SGN 1 and 4?

Lines 245-246: Which compounds are the authors referring to? Products from heterogeneous reactions or Aldehydes? In both cases, the sentence should be revised. (i) products formed in the particle phase cannot partition as they are already in the condensed phase and second the aldehydes are not considered as low vapor pressure compounds (Kroll and Seinfeld, 2008).

Lines 249-253: Before speculating on the potential formation of organosulfur the authors should present/discuss the validation of the AMS results: calibration of the AMS, uncertainties of the measurements, SMPS vs AMS data,... This is important in order to claim a large formation of organosulfur.

Lines 265-270: In other words, the large gap between SO<sub>2</sub> and SO<sub>4</sub> cannot be explained by the organosulfur. The authors should not speculate something and a few lines after conclude that is not the case. In Fig S8, the difference should be tens of  $\mu\text{g m}^{-3}$  while the authors estimated the formation organosulfur of a few tens of  $\text{ng m}^{-3}$ .

Lines 387-388: The O:C ratio didn't change significantly so I do not think the authors can claim to the formation of highly oxidized species.

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