

## ***Interactive comment on “Synergetic formation of secondary inorganic and organic aerosol: Influence of SO<sub>2</sub> and/or NH<sub>3</sub> in the heterogeneous process” by Biwu Chu et al.***

**Anonymous Referee #2**

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The manuscript reports data related to the effect of SO<sub>2</sub> and NH<sub>3</sub> on aerosol formation from the oxidation of toluene in the presence on NO<sub>x</sub>. The experimental study was conducted in the presence or absence of inorganic seed aerosol: AL<sub>2</sub>O<sub>3</sub>. NH<sub>3</sub> and SO<sub>2</sub> are two species emitted into the atmosphere and can have a large effect on atmospheric chemistry. The data analysis show aerosol formation and growth increased in the presence of SO<sub>2</sub> regardless of the presence of NH<sub>3</sub>. This study and its topic is of great interest and appropriate to ACPD journal. This study is worth to be published since it present an important set of data that can be useful for the atmospheric communities. However, I feel that the text and the scientific discussion (interpretation of experimental data) (see my comments below) need to be addressed before publication.

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The experimental part needs to be addressed and clearly state how the experiments were run. Toluene as well other gas phase species need to be reported vs time in this study. The role of OH radicals should be discussed? The wall loss of gas phase and particles should be addressed also? The errors and uncertainties need to be addressed since assumptions were made in this study. Yield should be reported in this study for the different systems studied.

As I mentioned, the manuscript reports a set of great data important to scientist interested in atmospheric organic and inorganic aerosol formation and the effect of NH<sub>3</sub> and SO<sub>2</sub>!

Comments:

In the introduction (1st paragraph), the authors report literature data for NH<sub>3</sub> in China and almost no data was provided for SO<sub>2</sub>. I suggest SO<sub>2</sub> should be provided also and a comparison should be reported between SO<sub>2</sub> and NH<sub>3</sub>. The text in the manuscript should be edited for consistency. I found it very hard to follow the authors' ideas in the manuscript, although lot of information is provided. For example, sentences reported between lines 89 and 115 are very difficult to follow for me!!! This is true for most the manuscript!

Line 125: this study focusses on "toluene" and "VOC" should be deleted.

The chamber was a 2 m<sup>3</sup> and the losses expected to be higher. The authors should give more information in this study about the wall losses of gas phase and particles.

How NH<sub>3</sub> was estimated (line 168)? Needs errors and uncertainties?

Line 171. title not clear to me? Be specific I would suggest: Effect of NH<sub>3</sub> and SO<sub>2</sub> on particle formation and growth

Table 1 change "hydrocarbon" to "toluene"

Data provided in Table 1 are initial concentration? The authors need to specify how the

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chamber was run (as flow reactor or batch reactor). Based on fig 1, it seems to me it was conducted as a batch reactor. In this case the wall losses are important and need to be incorporated in the discussion and how toluene and SMPS data were analyzed to get to Figs 1, 2.. and tables 1, 2.

It is also very important to provide data for Toluene reacted, SO<sub>2</sub> reacted, NH<sub>3</sub> reacted and NO reacted, NO<sub>x</sub> reacted (vs. time) in the gas phase in a separate figure.

The experiments were conducted under 50% RH. The authors didn't reported in the text until Table 1 was mentioned. This is a very important parameters that should be reported and discussed at list briefly. How it was measured and controlled in the chamber!!! Same thing for the temperature! I'm expecting the RH will change aver the run time and will be not constant?

How NH<sub>3</sub> was introduced into the chamber? Please elaborate!

It's important to have the amount of toluene reacted in each case in order to see how much was oxidized. then measure the yield etc... The OH radicals present in the system can be reacted with different gas phase species (e.g. toluene, SO<sub>2</sub>,...) and then depending on the rate constant, it can affect the conclusion reported in this study.

Line 225. .How the authors distinguish between secondary organic aerosol and secondary inorganic aerosol in these experiments?

Line 228-230 How the authors come to this conclusion? It's speculation and not based on data reported here. How nitrate are measured in this study? Are you refering to inorganic or organic nitrates? It will be great if a distinction was made between SOA and secondary inorganic aerosol in this study?

It will be interesting to estimate the concentration of OH radicals vs time in these experiments? Line 239. "The larger diameter resulted in more significant wall deposition, reduced the surface area of the suspended particles, and shifted the partition equilibrium to the gas phase." Are the authors measured the wall losses at different size

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distribution or this only speculation?

Line 245 – 249. It seems to me that these were not based on data reported in these study. How N<sub>2</sub>O<sub>5</sub> plays a role here? N<sub>2</sub>O<sub>5</sub> was measured in this study? How it was formed in the chamber. Is ozone was measured in this study? If yes it should be reported vs time.

Lines 256. Is ammonia salt was measured? How the authors come to the measured ammonia salt.

NH<sub>3</sub> was estimated in the chamber according to Table 2. Why NH<sub>3</sub> was not measured experimentally vs time? This is very important (same for SO<sub>2</sub>). I suggest to use "experiment without NH<sub>3</sub> added to the chamber" instead of "NH<sub>3</sub> poor"? It's confusing and make it difficult for comparison with other data?

Line 314. Should be Table 2.

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