

We appreciate the careful consideration of our manuscript by the reviewers. We have carefully responded to all of the [point-by-point](#) comments and issues raised by the reviewers and have revised the manuscript accordingly. These revisions are described in detail below

### **Anonymous Referee #1**

This paper contains novel data on reactive uptake of ammonia ( $\text{NH}_3$ ) by secondary organic aerosol (SOA) particles. Specifically, the authors describe chamber-based measurements that provide the reactive uptake coefficients of gaseous  $\text{NH}_3$  by two very different types of SOA. An important result of this paper is the direct observation of a relatively efficient uptake resulting in the formation of a significant concentration of nitrogen organic compounds (NOC). The paper is well written, and the experiments seem to have been carried out very carefully. A few relevant recent studies were missed by the authors; the list of some of the missing references is provided at the end. Other than that I am in favor of publishing it in ACP.

**Response:** Thank you so much for your positive comments and suggestions.

### GENERAL COMMENTS

1. In my opinion, the importance of BrC resulting from the  $\text{NH}_3$  + SOA reactions is overstated in the introduction. I would strongly recommend emphasizing that formation of BrC in reactions of  $\text{NH}_3$  with glyoxal, methylglyoxal, and SOA compounds have been demonstrated in the lab in experiments with model systems but never actually confirmed in the field. Furthermore, the existing evidence suggests that BrC produced by such  $\text{NH}_3$  reactions is rather unstable with respect to degradation by oxidants (Sareen et al., 2010) and sunlight (Lee et al., 2014; Zhao et al., 2015). Finally, the BrC formation by this mechanism is relatively slow and the MAC values of this type of BrC are quite small, which implies a low level of impact on the climate relative to that of primary

BrC from biomass burning. I would therefore recommend shifting the stress of the introduction from the formation of BrC to the formation of NOC, which may or may not be light absorbing but likely have very interesting chemical properties.

**Response:** Thank you for your suggestions. In the introduction, we have weakened the statements about the possible role of NH<sub>3</sub> uptake reactions by relevant organic aerosols. For example, the word “significant” has been replaced with “possible” in line 62, page 4. The sentence in lines 87-94, page 5 has been revised as “Although it has not been confirmed with ambient data, the formation of light absorbing compounds has been inferred in laboratory studies during reactions between glyoxal, methylglyoxal and primary amines glycine, methylamine and ammonium (Zarzana et al., 2012; Yu et al., 2011; Powelson et al., 2014; Lee et al., 2013a; Trainic et al., 2011). Visible light absorption has also been observed from the reactions between O<sub>3</sub>/OH initiated biogenic and anthropogenic SOA and NH<sub>3</sub> (Updyke et al., 2012; Nguyen et al., 2013; Lee et al., 2013b; Bones et al., 2010).” A new sentence as “Recent studies have found that BrC produced via such reactions is unstable with respect to degradation by oxidants (Sareen et al., 2013) and sunlight (Lee et al., 2014; Zhao et al., 2015). Regardless, NOC are likely have very interesting chemical properties and atmospheric implications.” has been added in lines 98-101, pages 5-6.

2. The choice of references cited in support of the importance of BrC in the introduction section appears to be a little unbalanced in my opinion. Citing review articles such as the one by Andreae and Gelencser (2006) is very appropriate; however, more focused papers by Powelson (2014), Updyke et al. (2012), Alexander et al. (2008), etc., are perhaps not the best choices for the introduction of BrC as a topic. The formation of BrC from NH<sub>3</sub> + organics reactions has recently been reviewed in detail by Laskin et al. (2015); the authors could significantly cut down their citation list by taking advantage of this review. A related review by Moise et al. (2015) could also work very well for that purpose.

**Response:** Thank you for your good suggestion. The references have been updated as you suggested. For example, “Alexander et al., 2008” has been replaced with “Laskin

et al., 2015” in line 44, page 3; “Andreae and Gelencser, 2006; Powelson et al., 2014; Alexander et al., 2008” have been replaced with “Andreae and Gelencser, 2006; Alexander et al., 2008; Moise et al., 2015” in lines 45-46, page 3.

3. The authors appear to be unaware of the study by Flores et al. (2014), who examined reactive uptake of NH<sub>3</sub> by different types of SOA, including alpha-pinene SOA, using a ToF-AMS instrument amongst other things. The present ToF-AMS results should probably be compared to the results of Flores et al. experiments, if such a comparison makes sense to the authors.

**Response:** Thank again for this suggestion. As you pointed out, Flores et al. (2014) have examined the composition of NH<sub>3</sub>-aged SOA from ozonolysis of  $\alpha$ -pinene and limonene with DESI-HR-MS. In that study a significant increase in the fraction of organic constituents with one or two N atoms after ageing with NH<sub>3</sub> was observed. For NH<sub>3</sub>-aged limonene SOA, they also observed products containing 1-4 units of ketolimononaldehyde and 0-2 nitrogen atoms in the m/z range of 168 - 360 with ToF-AMS. Due to the differences in the precursors and the mass range used, we cannot directly compare their results with ours. However, in their work, 1-2 N atoms have been observed in both reaction systems. This is consistent with our results. Therefore, the following sentence has been added in our revised manuscript “Our results are consistent with the previous work that observed a significant increase in the fraction of organic constituents with one or two N atoms for NH<sub>3</sub>-aged  $\alpha$ -pinene SOA (Flores et al., 2014).” in lines 248-250, page 12.

4. One potential weakness of the AMS method is the complications from the pyrolysis/ionization processes occurring in the ionization region. Can the authors prove that the NOC species they are observing are not produced in that process? I would feel much more comfortable with the conclusions of the paper if the authors presented reference AMS spectra ammonium salts of several carboxylic acids (e.g., of pinonic acid and of benzoic acid) recorded under their specific AMS operating conditions and showed that they gave much different patterns in the mass spectra.

**Response:** While we cannot entirely rule out the possibility that some N-containing fragments might be formed in the ionization region of the AMS it is likely to be very small. Since pinonic acid contains one isolated carbonyl, Schiff-base or Mannich reactions may occur during synthesis of the ammonium salt from pinonic acid in the laboratory. Hence, here, we directly measured the mass spectrum of pure ammonium benzoate (Fig. R1). While some N-containing fragments were observed in Fig. R1B (when the mass loading of OA was around  $200 \mu\text{g m}^{-3}$ ), the fragment pattern was very different from that observed in chamber experiments (Fig. R2 and Fig. S3), in which the signals of  $\text{C}_x\text{H}_y\text{N}_n$  and  $\text{C}_x\text{H}_y\text{ON}_n$  were prominent. These fragments are not likely to be observed at the low aerosol loading levels of our chamber experiments. Hence it is highly likely that the uptake of  $\text{NH}_3$  mainly contributed to the signals of NOC during photochemical reaction of m-xylene. We do recognize that some uncertainties might arise from the possible pyrolysis/ionization processes in the AMS. In the revised manuscript (Page 9, lines 170-173), we have added some discussion about this possibility as “It should also be pointed out that some NOC species may be formed through the pyrolysis/ionization processes occurring in the ionization region. This would result in a positive uncertainty for NOC measurements in this study, although it is expected to be small”.

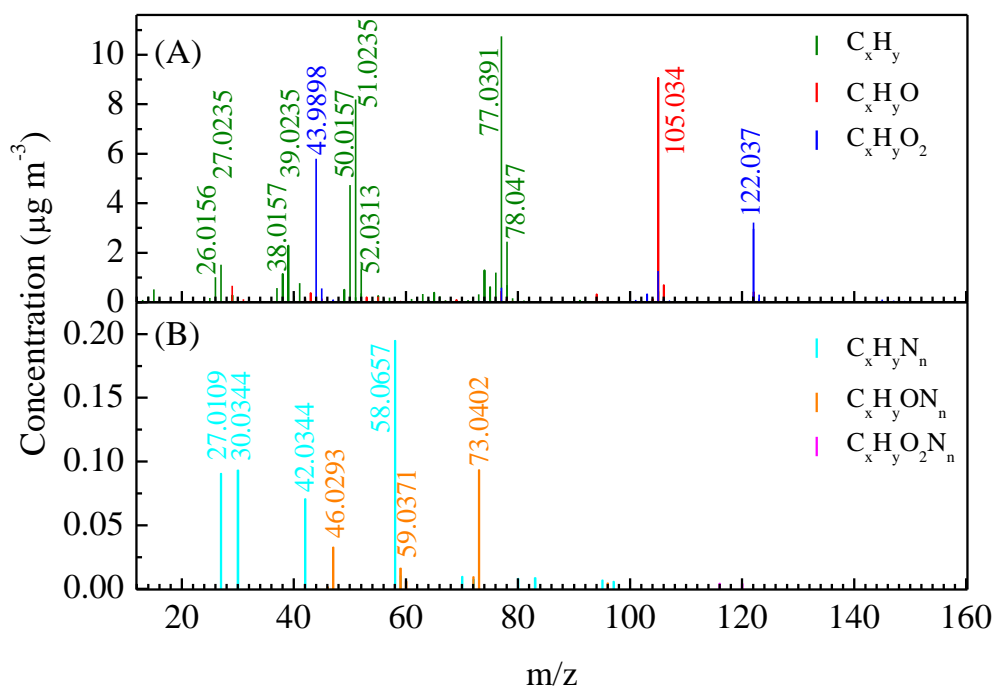


Fig. R1. Mass spectra of (a) non-N-containing species fragments and (b) N containing species fragments of ammonium benzoate.

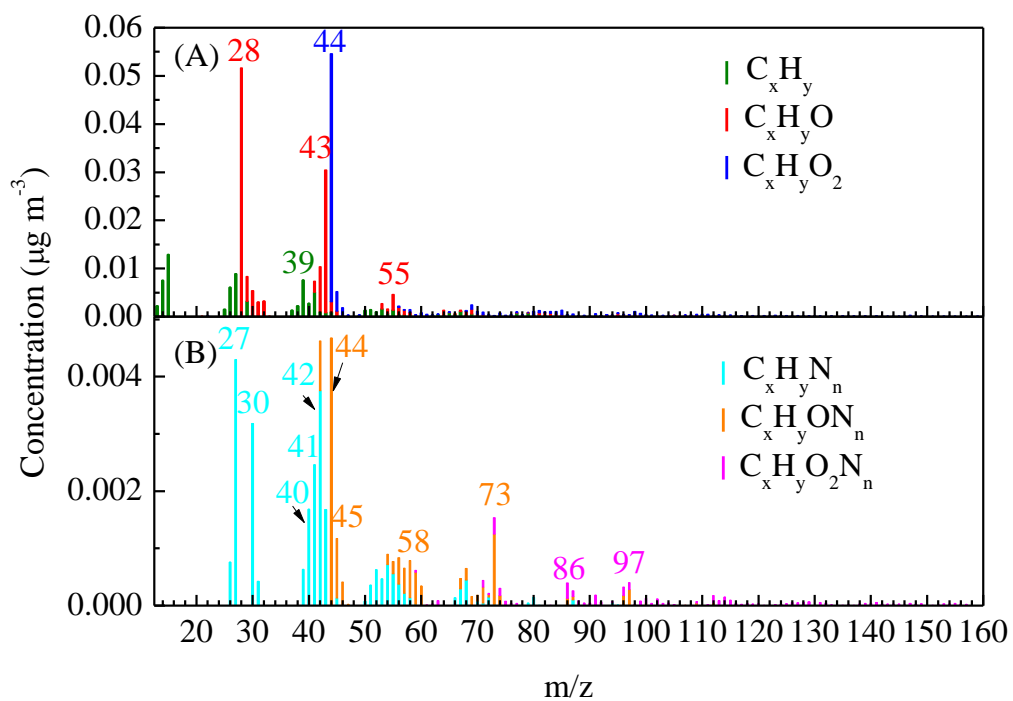


Fig. R2. Mass spectra of (a) non-N-containing species fragments and (b) N-containing species fragments in the SOA formed by OH oxidation of m-xylene in the presence of

66 ppbv NH<sub>3</sub> (Exp. M16).

#### SPECIFIC COMMENTS

Equation 2 and equations in the SI section: Chemical uptake of each NH<sub>3</sub> molecule is probably accompanied with a release of one or more H<sub>2</sub>O molecules as byproducts (this prediction is based on the multiple studies cited in the text, and on Scheme 1 used by the authors). Organic Carbonyl + NH<sub>3</sub> → NOC + x H<sub>2</sub>O. Unless the initial carbonyl and NOC have very different volatilities and molar volumes, there might not be any increase in the overall particle mass and size, or possibly even particle shrinking if  $x > 1$ . Would this consideration affect equation 2 and conclusions drawn from it? I have not analyzed this question too carefully but equation (2) does include particle size and density in it, and equations (S5) does imply that the authors assume an increase in the particle diameter during the reaction.

**Response:** We agree with you that chemical uptake of one NH<sub>3</sub> is accompanied with a release of one or more H<sub>2</sub>O molecules in Schiff-base reaction. If the H<sub>2</sub>O molecules escape from the particle surface and the NOC has a similar molar volume as the initial carbonyls, the overall particle mass and size will reduce. However, if the formed H<sub>2</sub>O remained on the particle surface, the particle size will increase after uptake of NH<sub>3</sub>. It should be noted that uptake of an additional aldehyde in Mannich reaction will lead to an increase of particle size. In addition, the measured uptake coefficients are comparable to the similar reaction systems as discussed in page 24. This also increases the confidence with respect to the method used in our work.

Page 17459, line 8: The proposed assignment of an IR band to a C=C-CN species cannot be claimed with the level of certainty currently used in the manuscript without isotopic data. This conclusion is highly speculative and it should be clearly labelled as such. Related to this, please see the comment about Figure 2 below. Page 17460, the first paragraph on this page, and subsequent discussion: Laskin et al. (2014) actually have a paper on alpha-pinene SOA reacting with NH<sub>3</sub>, which might be much more relevant for the discussion than some of the less relevant papers on limonene SOA

+NH<sub>3</sub> reactions currently cited by the authors.

**Response:** Yes, the assignment of the IR band at 2195 cm<sup>-1</sup> is highly speculative. In the revised manuscript (lines 276-277, pages 13-14), the sentence "..., which was not present in the  $\alpha$ -pinene derived SOA (Fig. 2) and was assigned to  $\nu_{s,C=C-C\equiv N}$  (Lin-Vien et al., 1991) (a nitrile)." has been revised as "..., which was not present in the  $\alpha$ -pinene derived SOA (Fig. 2) and was potentially assigned to  $\nu_{s,C=C-C\equiv N}$  (Lin-Vien et al., 1991) (a nitrile)." The discussion, in lines 302-306, page 15, have been revised as "In addition, SOA, which was formed through the ozonolysis of  $\alpha$ -pinene and d-limonene, subsequently impacted on a polymeric plate and then exposed to gaseous NH<sub>3</sub>, resulted in a significant enhancement in relative abundance of several NOC molecules, such as, C<sub>9</sub>H<sub>11</sub>NO<sub>2</sub>, C<sub>9</sub>H<sub>13</sub>NO<sub>2</sub>, C<sub>19</sub>H<sub>29</sub>NO<sub>4</sub>, C<sub>19</sub>CH<sub>29</sub>NO<sub>5</sub>, C<sub>19</sub>H<sub>33</sub>NO<sub>5</sub> (Laskin et al., 2014)."

Page 17464, line 12: is -> are

**Response:** It has been corrected in the revised manuscript (line 434, page 21).

Page 17464, line 13: insert comma before "respectively"

**Response:** It has been corrected in the revised manuscript (line 434, page 21).

Page 17464, lines 14-17: there is a simple present - simple past tense inconsistency in this sentence.

**Response:** The "is" in line 436, page 21 has been revised as "was" in the revised manuscript.

Page 17464, lines 19 and 23: upon -> on

**Response:** It has been corrected in the revised manuscript (line 440, page 21).

Page 17464, line 24: In my opinion, there is no inconsistency here because Updyke et al. (2012) did not report any NOC amounts; they only reported absorption coefficients. More NOC does not mean more absorbing aerosol.

**Response:** Thanks. The sentence "This is in contrast to recent reports that OH initiated

anthropogenic SOA forms relatively less NOC than ozone oxidation of monoterpenes (Updyke et al., 2012). However this is likely dependent upon differences in a variety of conditions between experiments” in line 443, page 21 has been removed.

Page 17466, line 19: insert comma before “respectively”

**Response:** It has been corrected in the revised manuscript (line 503, page 24).

Page 17467, line 1: at present -> at the present

**Response:** It has been corrected in the revised manuscript (line 512, page 24).

Page 17468, line 8: photo-chemistry -> photochemistry

**Response:** It has been corrected in the revised manuscript (line 546, page 26).

Page 17468, line 17: significant organic material (SOA) -> significant amount of organic material (e.g., in the form of SOA)

**Response:** It has been corrected in the revised manuscript (line 556, page 26).

Page 17470, line 18: a value 1.4 -> a value of 1.4

**Response:** It has been corrected in the revised manuscript (line 617, page 29).

Page 17470, line 22: troposphere -> the troposphere

**Response:** It has been corrected in the revised manuscript (line 621, page 29).

Page 17471, line 2: Updyke et al. (2012) is an unsuitable reference for BC absorption coefficients; a review on BC would be much better; the already quoted review by Andreae and Gelencser (2006) should work.

**Response:** Thanks. It has been updated as you suggested (line 628, page 29).

Page 17471, line 24: the result -> the results

**Response:** It has been corrected in the revised manuscript (line 651, page 31).



Reference section: I suspect that “P. Natl. Acad. Sci. USA” is not the correct abbreviation for this journal. I would guess “P.” should be “Proc.”

**Response:** Thanks. It has been corrected in the revised manuscript.

Page 17473: fix subscripts/superscripts in the title of Bones et al. reference.

**Response:** It has been corrected in the revised manuscript.

Page 17481: fix alpha and beta in the title of Zhang and Zhang reference.

**Response:** It has been corrected in the revised manuscript.

Page 17483: Where TN -> TN.

**Response:** “Where TN” has been replaced with “TN”.

Figure 2: I have a couple of issues with this figure. The first issue is related to the ambiguity of FTIR band assignments mentioned above. The decomposition of the observed profile into sub-bands does not look very convincing to me, especially for the “1563”, “1315”, and “1050” bands shown in panel b. The authors should explain how their peak centers and band widths were chosen in the fit. The second issue is technical: what is the significance of different colors (if any) used in this figure? This has not been explained in the caption or text.

**Response:** Thanks. Here, we decompose the observed profile into sub-bands to qualitatively demonstrate the presence of these species which have been assigned in Table S1. The IR curve was fitted based on the deconvolution curve as well as the frequencies of these products reported in the literatures. The peak centers which have been labeled in the figure were fixed and the band widths was set to vary during curve fitting. We only keep the same color for the same or similar species in panels a and b.

Figure 3: I would make capitalization of panel labels uniform; the red arrow in panel (a) is not placed well; the choice of red line on blue background in panels (c) and (d) is

likely to make the red line invisible in print. In terms of the experimental design, I think it was not ideal to turn the lights off and add extra ammonia at the same time, as shown in panel (b). It would have been better to wait between these two events.

**Response:** Thanks for your suggestions. This figure has been edited as you suggested and shown as follow (Fig. R3). Since the uptake reactions of  $\text{NH}_3$  by OA should be related to Schiff-base or Mannich reactions as discussed in this work, UV light should not be necessary for these reaction. The objective of this portion of the experiment is to confirm whether the relatively aged SOA can take up  $\text{NH}_3$ . Therefore, additional  $\text{NH}_3$  was introduced to the chamber immediately after the UV lights were turn off. Turning the lights off avoids unnecessary complicating SOA formation (of less aged organic material).

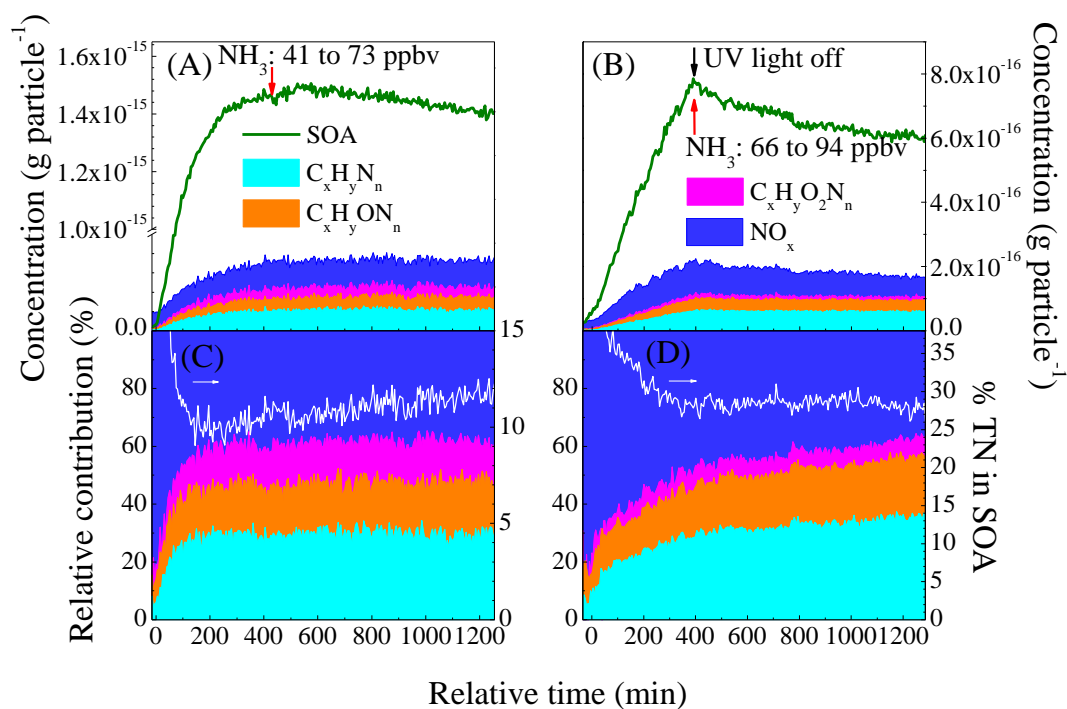


Figure R3. Concentration changes for N-containing fragments and SOA for (A) ozonolysis of  $\alpha$ -pinene (Exp. P6) and (B) OH oxidation of m-xylene (Exp. M16), respectively; the relative fraction of each species to total NOC mass ( $\text{TNOC} = \text{C}_x\text{H}_y\text{N} + \text{C}_x\text{H}_y\text{ON} + \text{C}_x\text{H}_y\text{O}_2\text{N} + \text{NO}_x$ ) and TNOC to SOA fraction for (C) ozonolysis of  $\alpha$ -pinene and (D) OH oxidation of m-xylene, respectively.

Figure 6: I believe the second sentence should reference (c) and (d), not (b) and (c).

**Response:** Yes, it is and has been corrected in the revised manuscript.

Figure 7: m-mylyene -> m-xylene

**Response:** It has been corrected in the revised manuscript.

## **Anonymous Referee #2**

In this manuscript the authors report on an experimental investigation of reactive uptake of ammonia onto secondary organic aerosols (SOA), with the intends to evaluate the kinetics and mechanism leading to organonitrogen formation. Environmental chamber measurements were presented for the ozonolysis of  $\alpha$ -pinene and the OH oxidation of m-xylene in the presence of ammonia. Organonitrogen formation from their chamber experiments was analyzed using a High Resolution Time-of- Flight Aerosol Mass Spectrometer and a Quantum Cascade Laser instrument. The uptake coefficients of  $\text{NH}_3$  to SOA leading to organonitrogen compounds were estimated, and the reaction of ammonia with carbonyl was speculated to be by liquid-phase diffusion limited heterogeneous reaction on SOA. Overall, this work represents a useful study to evaluate the role of  $\text{NH}_3$  on SOA formation. The paper was reasonably written. I recommend that this work can be publishable in ACP, provided that the following issues are adequately addressed.

**Response:** Thank you for your positive comments.

Their discussion on the mechanism of  $\text{NH}_3$  on SOA leading to organonitrogen formation was both qualitative and speculative. Two types of mechanism were suggested under their experimental conditions, i.e., the reactions of ammonia/ammonium with carbonyl functional groups in SOA leading to the formation of covalently bonded carbon to nitrogen and acid-base reactions between ammonia/ammonium and organic/inorganic acids in particles leading to organic

ammonium salts. The authors claimed that a large diversity of nitrogen containing organic (NOC) fragments observed were consistent with the reaction of ammonia with carbonyl. However, it was uncertainty on what types of carbonyls might be relevant to their experimental conditions. It was indeed interesting that they explored two types of VOC oxidations, ozonolysis of  $\alpha$ -pinene and OH-initiated oxidation of m-xylene. For the former type, it would be expected that organic acids are the dominant SOA component (for example, Ma et al., *Geophys. Res. Lett.* 40, 6293–6297, 2013). On the other hand, small  $\alpha$ -dicarbonyls would be most dominant products from the OH-initiated oxidation of m-xylene (Zhao et al., *J. Photoch. Photobio. A* 176, 199-207, 2005). In fact, their measured larger NOC mass fraction for m-xylene derived SOA than that for the total  $\alpha$ -pinene derived SOA would be indicative of the efficient reaction mechanism for  $\alpha$ -dicarbonyls with  $\text{NH}_3$  to form imine and/or imidazole formation. Nevertheless, the authors would need to provide some quantitative assessment of the plausible mechanism leading to organonitrogen formation, in light of available literature results on the yields of organic acids and small  $\alpha$ -dicarbonyls from VOC oxidation.

**Response:** Thank you for your comment and suggestion. In this work, we speculated two types of mechanisms based on previous studies (Lee et al., 2013; Bones et al., 2010; Galloway et al., 2009; Zarzana et al., 2012), namely the reactions of ammonia/ammonium with carbonyl functional groups in SOA leading to the formation of covalently bonded carbon to nitrogen, and acid-base reactions between ammonia/ammonium and organic/inorganic acids in particles.

As shown in Figure 3 and 6, a larger amount of nitrogen containing organic (NOC) fragments and higher uptake coefficients were observed during uptake onto SOA from OH-initiated oxidation of m-xylene than ozonolysis of  $\alpha$ -pinene. We agree with you that efficient reaction for  $\alpha$ -dicarbonyls with  $\text{NH}_3$  to form imine and/or imidazole formation is likely here as carbonyls are likely the most dominant products from the OH-initiated oxidation of m-xylene (Zhao et al., 2005) compared to ozonolysis of  $\alpha$ -pinene (Ma et al., 2013). We cannot of course, be fully quantitative in these types of

experiments for a variety of reasons. Such reasons include the inability to detect specific product molecules with the AMS, and the potential for other functional groups (ie: acids) to be formed from both precursors with oxidative aging. Regardless, the possible dominance of dicarbonyls in the aromatic system is a plausible reason for the enhanced NOC observed. While this cannot be confirmed we have nonetheless modified the manuscript to reflect this possibility. In the manuscript (page 15, lines 312-314 and lines 316-318), two sentences were added as “In particular, previous work has found that organic acids are the dominant SOA component (Ma et al., 2013).” and “It has been found that  $\alpha$ -dicarbonyls are the dominant products from the OH-initiated oxidation of m-xylene (Zhao et al., 2005)”, respectively. And in page 21, lines 443-448, the sentences “This is in contrast to recent reports that OH initiated anthropogenic SOA forms relatively less NOC than ozone oxidation of monoterpenes (Updyke et al., 2012). However this is likely dependent upon differences in a variety of conditions between experiments.” has been replaced with “As discussed in Section 3.2,  $\alpha$ -dicarbonyls are likely the most dominant products from the OH-initiated oxidation of m-xylene (Zhao et al., 2005), while organic acids are more likely the dominant SOA component from ozonolysis of  $\alpha$ -pinene (Ma et al., 2013). This suggests an efficient reaction for  $\alpha$ -dicarbonyls with  $\text{NH}_3$  to form imine and/or imidazole and is consistent with the higher NOC content in the total SOA mass from OH oxidation of m-xylene.”

Two previous studies (Paciga et al., Environ. Sci. Technol. 2014, 48, 13769-13775; Na et al., Environ. Sci. Technol. 2007, 41, 6096-6102) have assessed the effects of ammonia on SOA formation, but were not discussed in the present work. In particular, the later work examined the effects of ammonia in an environmental study of  $\alpha$ -pinene ozonolysis in dry and humid conditions. Furthermore, a recent review article has provided a good coverage on heterogeneous reactions relevant to acid-base reactions and reactions relevant to carbonyls with basic species (Zhang et al., Chem. Rev. 115, 3803-3855, 2015). Those literature results should be carefully considered, when discussing their present results.

**Response:** Thank you for your suggestion. These previous studies has been considered and discussed in our revised manuscript (page 16, line 327-331), as “Previous studies have observed the neutralization reaction between  $\text{NH}_3$  and organic acids in both flow reactor (Paciga et al., 2014) and environmental chambers (Na et al., 2007). In particular, high concentrations of  $\text{NH}_3$  greatly promoted SOA formation from ozonolysis of  $\alpha$ -pinene (Na et al., 2007). This was ascribed to the formation of organic ammonium salts.”

What were the systematic and random errors in their estimated uptake coefficients? The methods for such uncertainty assessment should be discussed.

**Response:** As shown in equation (2), the uncertainty of the uptake coefficient results from the uncertainty of NOC mass concentration measured by the AMS, the concentration of  $\text{NH}_3$  measured by the QCL and the diameter measured by the SMPS. In chamber studies, it is very difficult to perform experiments with exactly the same conditions each time. In this study, the error bars are derived from the uncertainties of the uptake model parameters based on the measured time series of mass concentration of NOC fragments. The uncertainties of  $\text{NH}_3$  concentration and particle diameter have been considered in this model. In the titles of Figures 5 and 6, the uncertainties have been pointed out. A sentence about this has been added in page 11, lines 223-227, as “The uncertainty in the uptake coefficient will result from the uncertainty in NOC mass concentration measured by the AMS, the concentration of  $\text{NH}_3$  measured by the QCL and the diameter measured by the SMPS. In this study, the uncertainty is derived from the uptake model parameters based on the measured time series of mass concentration of NOC fragments.”

Could they differentiate whether such an uptake was surface or bulk reaction?

**Response:** As discussed in the manuscript (Page 28), the measured uptake coefficients decrease with the increasing of  $\text{NH}_3$  concentration, which is consistent with surface reaction. However the observed acidity dependence argues against surface reaction. We therefore cannot rule out bulk reaction in our experiments.

In the abstract, add “respectively” after “m-xylene derived SOA,”.

**Response:** Thanks. We have corrected the similar mistakes throughout the paper.

**Reference:**

Bones, D. L., Henricksen, D. K., Mang, S. A., Gonsior, M., Bateman, A. P., Nguyen, T. B., Cooper, W. J., and Nizkorodov, S. A.: Appearance of strong absorbers and fluorophores in limonene-O(3) secondary organic aerosol due to NH<sub>4</sub>(+)-mediated chemical aging over long time scales, *J. Geophys. Res.- Atmos.*, 115, doi: 10.1029/2009JD012864, 2010.

Galloway, M. M., Chhabra, P. S., Chan, A. W. H., Surratt, J. D., Flagan, R. C., Seinfeld, J. H., and Keutsch, F. N.: Glyoxal uptake on ammonium sulphate seed aerosol: reaction products and reversibility of uptake under dark and irradiated conditions, *Atmos. Chem. Phys.*, 9, 3331-3345, doi: 10.5194/acp-9-3331-2009, 2009.

Lee, H. J., Laskin, A., Laskin, J., and Nizkorodov, S. A.: Excitation Emission Spectra and Fluorescence Quantum Yields for Fresh and Aged Biogenic Secondary Organic Aerosols, *Environ. Sci. Technol.*, 47, 5763-5770, doi: 10.1021/es400644c, 2013.

Ma, Y., Brooks, S. D., Vidaurre, G., Khalizov, A. F., Wang, L., and Zhang, R.: Rapid modification of cloud-nucleating ability of aerosols by biogenic emissions, *Geophys. Res. Lett.*, 40, 6293–6297, doi:10.1002/2013GL057895, 2013.

Na, K., Song, C., Switzer, C., and Cocker, D. R.: Effect of Ammonia on Secondary Organic Aerosol Formation from  $\alpha$ -Pinene Ozonolysis in Dry and Humid Conditions, *Environ. Sci. Technol.*, 41, 6096-6102, 10.1021/es061956y, 2007.

Paciga, A. L., Riipinen, I., and Pandis, S. N.: Effect of Ammonia on the Volatility of Organic Diacids, *Environ. Sci. Technol.*, 48, 13769-13775, 10.1021/es5037805, 2014.

Zarzana, K. J., De Haan, D. O., Freedman, M. A., Hasenkopf, C. A., and Tolbert, M. A.: Optical Properties of the Products of  $\alpha$ -Dicarbonyl and Amine Reactions in Simulated Cloud Droplets, *Environ. Sci. Technol.*, 46, 4845-4851, doi:

10.1021/es2040152, 2012.

Zhao, J., Zhang, R., Misawa, K., and Shibuya, K.: Experimental product study of the OH-initiated oxidation of m-xylene, *Journal of Photochemistry and Photobiology A: Chemistry*, 176, 199-207, <http://dx.doi.org/10.1016/j.jphotochem.2005.07.013>, 2005.