

Interactive comment on “Secondary aerosol formation from atmospheric reactions of aliphatic amines” by S. M. Murphy et al.

Anonymous Referee #2

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General comments

This paper presents data on a previously unexplored route for formation of secondary organic aerosol (SOA) in the atmosphere - that route being oxidation of amines. Because there is a discrepancy between observed levels of organic aerosol and our understanding of the sources, this paper addresses relevant scientific questions and is well within the scope of ACP. The conclusions are suitable given the generated data, and the title and abstract appropriately reflect the contents of the paper. The manuscript is adequately referenced and is well written.

Specific comments

In the experimental section (section 2, page 293), it is stated that the concentration of

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the amine is estimated through the volume of amine injected. This assumes that the volume of the chamber is always 28 m³. How accurate is this? Is this uncertainty taken into account when calculating SOA yields? Or is this uncertainty small compared to all others?

In equation (1), d (delta) is defined as being the internal void fraction. It is then stated the effective density is that of the material (I assume this is ρ_m ? This needs to be defined.) if this parameter has a value of zero. However, based on equation (1), if $d = 0$, the effective density should be infinite. Should the term in the equation be $(1-d)$?

I would suggest moving the discussion of reaction pathways associated with Figure 5a-c to section 3 where the atmospheric reaction pathways of amines are discussed. This would clearly necessitate the renumbering of figures.

In section 4.2, should the statement ‘typically assumed to be the dominant atmospheric nitrate salt’ (referring to ammonium nitrate) be qualified? This is only true in areas with sufficient ammonia to neutralize sulfate. In areas where this is not the case, the dominant form of nitrate appears to be that associated with soil or sea salt. This could also be qualified by referring to the dominant FINE nitrate salt.

In section 4.4, would it be possible to derive a time-dependent collection efficiency so that the sulfate data from the PILS and the AMS match?

In some cases, the authors see that the original salt continues to disappear after the formation of oxidation-induced SOA. In other cases, the original salt does not disappear. Wouldn't these results argue that the dissociation constant, not a coating, controls the dissociation? This is an important result that influences our understanding of heterogeneous processes and should be highlighted. Could this be explored a little more by investigating how thick an organic layer might be present to see if this is a relevant parameter?

Could relevant effective densities be included in the summary results presented in Ta-

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ble 4?

In Figure 5a, it could be included how the amide is converted to the relevant acid.

Technical corrections

SOA has been defined early in the manuscript - yet then is not used consistently. This should be corrected throughout.

End of section 5.4, top of page 315. Should this refer to Figure 9D rather than 10A?

Section 5.8, page 318. This should refer to Figure 12, not 13.

In Table 1, I believe that the formulae for MA and MEA are not correct as shown. MA should be CH_3NH_2 . MEA should be $(\text{CH}_2\text{OH})\text{CH}_2\text{NH}_2$.

In Figure 5a, should the formula of the aqueous amine on the right hand side be $\text{R}_3\text{R}_2\text{NH}$?

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 289, 2007.

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