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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Hygroscopic Properties of Aminium Sulphate Aerosols" *by* Grazia Rovelli et al.

Anonymous Referee #1

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Rovelli et al. describes the hygroscopic properties of single aerosol particles composed of aminium sulfate using a cylindrical electrodynamic balance. They demonstration this is a good way to measure the aminium sulfate aerosol hygroscopicity due to fast measurement time that does not allow for evaporation of analyte. In this paper it is shown that there is a correlation between the nwater/nsolution dependent on the number of alkyl groups present on the amine. The results are compared to previous studies that have used other methods (bulk and tandem DMA) are discussed to describe where the differences from other methods could be derived. The experiments and attention to detail on the preparation of the solutions to avoid ambiguity are well thought through to eliminate sources of error. The paper is well written and discusses/compares results with previous literature in detail about discrepancies that are well explained through the discussion.

Specific Comments:

Pg. 2 Line 32: It would be nice to give numbers for seasonal variation here since other numbers are given. Additionally, on P3 line 3 mention the location of the measurements for completeness.

Pg. 3 line 5: It would be good to discuss the new particle formation in more detail with references.

Pg. 2 line13: Include the solubility of the amines compared to the salt products.

Pg. 4 line 23: Could you give the RHs used in this experiment here? The only place I saw them was in Figure 1 for the AS experiment.

Pg. 6 line 24: What is the difference in any calculations due to less of the amine (e.g. formation of aminium bisulphate) possibly being present?

Pg. 8 line 5: Mention error again here in regard to the TEAS concentration measurement since you are making a direct comparison and caution that there might be error.

Pg. 11 line 5: It would be nice to include the trend from Saurwein 2015 somewhere in reference to the GFr and nwater/nsolute since it is not discernable from the comparison graphs in Fig. 5. Are they the same trend as your measurements?

Pg. 13 line 15: Section 3.3 (and corresponding fig. 8) does not seem to add to the paper since the method has already been validated in a previous publication and could be moved to the supplemental information.

Figure 1: Note in 'experimental data graph' that you alternate between the sample and a standard (or clarify when it is performed). Some additional description of a, b, c, in the figure caption would be helpful in discerning the use of the figure even though it is described well in text.

Figure 3: Why is there no data for MMAS and TMAS between ∼0.7-0.8 aw? What is the RH range used for these measurements?

Fig. 3,5,6, 8, 10: In description make it explicit that the ammonium sulfate is a model that you already validated earlier in the paper (Fig. 2). On first read through I missed this and was wondering why it was compared to a model and not the experiment.

Fig. S2: Why is the Saurwein et al. 2015 data not included here?

Technical Corrections:

Pg. 2 line 14: 'Because of this. . .' The 'this' is ambiguous and seemingly refers to solubility, reword sentence for clarity.

P6 line 15: Change the mass fraction to wt% for consistency within paper

Figures: Keep the lettering (a,b, etc.) capitalization consistent, Fig. 3 and S1 are capitalized while others are lowercase.

Fig. 5/6: note that the Clegg et al. 2013 data is from Qui and Zhang 2012, this was not clear.

Figures 5, 6, S2, S3: Include lettering in the graphs (a,b etc.) for consistency and ease of reference in text.

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