

Interactive comment on “Growth of sulphuric acid nanoparticles under wet and dry conditions” by L. Škrabalová et al.

Anonymous Referee #1

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This manuscript, which seems to be very closely related to ACPD-13-2313-2013 by Neitola et al. (also under review) deals with the growth of sulphuric acid nanoparticles at three different temperatures, two RH:s, four residence times and varying initial acid concentrations.

The main findings listed in the conclusions (as I see them) are: 1. The results are reproducible. 2. High acid concentrations result in higher growth rates than low acid concentrations. 3. Ammonia promotes particle growth. 4. Sulphuric acid alone cannot explain the growth rates. 5. The wall losses cannot be described by the correlation suggested by Hanson and Eisele (2000) and are substantially lower. Equilibrium conditions between the gas and wall are suggested. 6. Wall losses are important in nucleation experiments, since the real amount of sulphuric acid is important to know.

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So what is new information here??? This question is especially important since the 'companion paper' by Neitola et al. seems to describe the same system and also look at the important question in which form the sulphur is in the particles. I have a hard time to find in the paper the scientific novelty, even if the topic itself is hot. Perhaps the key interesting finding of the paper is presented in the abstract, that the effect of RH on growth was found to be inconsistent: at low acid conc. high RH seems to slow growth but at high acid conc. a higher RH increases growth. This observation is, however, not explained.

In my view the key problem of the paper lies in the poor quantification of the wall losses, which is partly due to the unknown impurities in the system - and affects all further analysis. The presence of impurities (e.g. ammonia) in the system, even at low concentrations, will probably have significant effects on the aerosol dynamics of the system. As a significant fraction of the sulphuric acid may be in larger clusters (as studied by the same group in Neitola et al.), it can have big effects on both the growth and the deposition behaviour of the aerosol. In addition, the presence of impurities on the wall may also affect deposition rates. Partly because of these reasons it might not make so much sense to analyse whether the deposition expression by Hanson and Eisele, that has been developed for a different system, works or not? In addition, the statement "We estimated the real wall losses of H₂SO₄ by matching the predicted and observed growth rates through tuning the initial H₂SO₄ concentration in the model", on page 24103, will probably result in wrong wall losses because of these same reasons?

With these uncertainties in wall losses and not knowing the impurity concentrations (or more details of the cluster distributions), much of the growth rate data is unfortunately not very interesting and is probably very system specific. As the group seems to have access to all the newest toys (CIMS, APITOF, MARGA...), as shown by the much better characterized experiment by Neitola et al., why were they not applied here?

Acceptance of this, in my view, would require better quantified wall loss rates and cluster distributions and some idea of the impurities (and their role in the dynamics). If

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this is not possible, I encourage the authors to merge the key findings of the paper with Neitola et al, if possible.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 24087, 2013.

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