



Interactive
Comment

Interactive comment on “Enhancement of atmospheric H₂SO₄/H₂O nucleation: organic oxidation products versus amines” by T. Berndt et al.

Anonymous Referee #1

Received and published: 13 August 2013

General comments:

This manuscript describes results from laboratory flow tube experiments conducted to probe the effects of organic oxidation products and amine compounds on new particle formation from H₂SO₄/H₂O nucleation. All experiments likely had a background amine concentration of 10^{e7} – 10^{e8} molecules per cubic centimeter. The results indicate that, under these conditions, organic oxidation products affect neither the total number of particles nor the early growth of newly formed particles directly. They may indirectly affect nucleation rate by increasing concentrations of H₂SO₄. The amine compounds did result in significant increases in particle concentrations from H₂SO₄/H₂O nucleation.

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The manuscript is well written and the reported results are within the scope of ACP. I recommend publication of the manuscript in ACP after my comments below have been addressed.

Specific comments:

1. The manuscript would benefit from additional discussion on how the results relate to previous work and which results or techniques are novel. For example, the nucleation enhancement of amine compounds is not a new result, and the additional H₂SO₄ formation, likely from stabilized Criegee intermediates, has recently been published. The experiments and results reported in this manuscript are interesting, but the novelty is not very clear in its current version.

2. The authors conclude that organic oxidation products (from alpha-pinene and limonene) alone do not enhance nucleation or contribute to early particle growth. This seems to be inconsistent with numerous environmental chamber studies in which secondary organic aerosols (i.e. particles) are formed from ozonolysis of alpha-pinene without the addition of H₂SO₄. The authors should address these potentially conflicting results. Could the difference be due to different generations of oxidation, i.e. do SOA formation experiments go to later generations? As evidence for lack of growth the authors mention that the particles are always detected in the same size bin. It would be appropriate to report more information here, for example, which size bins were measured. It may also be appropriate to report size distributions.

3. Regarding the organic oxidation products, which as stated by the authors are primarily first-generation, the authors should address the relevance of their work to atmospheric conditions where oxidation proceeds to later stages of oxidation.

4. Wikipedia should not be used as a reference. The two references to Wikipedia currently in the manuscript should be replaced by references to scientific publications.

5. The abstract is rather long and the conclusions are very similar to the abstract. I

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suggest shortening the abstract if possible and rewriting the conclusions, potentially by emphasizing the novelty of the work (see comment 1).

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 16301, 2013.

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