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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Enhancement of atmospheric H_2SO_4/H_2O nucleation: organic oxidation products versus amines" *by* T. Berndt et al.

Anonymous Referee #2

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In this manuscript, the authors examine the nucleation and growth of new particles in a flow tube apparatus. In one set of experiments, oxidized organics and sulfuric acid were mixed and the resulting particle concentrations measured. The authors conclude from these experiments that oxidized organic species do not play a substantial role in the formation and growth of new particles but instead, via formation of the stabilized Criegee intermediate and its subsequent reaction with SO2, contribute to an enhancement in the sulfuric acid concentration. In a second set of experiments, amines were added to the setup in the absence of other organic species. Amines were shown to enhance substantially the nucleation and growth rates. This manuscript is within the

scope of Atmospheric Chemistry and Physics and may eventually be publishable after the authors satisfactorily address the comments below.

Major Comments:

1. In this manuscript the authors conclude that oxidized organics are not important to the formation and growth of new particles (e.g. page 16302, lines 11-13). However, it is not always clear exactly what organics the authors are referring to. Are they referring only to first generation oxidation products? A fair amount of evidence now suggests that oxidized organic matter is crucial to the formation and growth of new particles, although most of these species may be the result of several generations of oxidation. If the authors are referring in this manuscript only to first generation oxidation products, then why are they invoking the presence of highly oxidized products as possible candidates to explain the observations from the PHA-UCPC (page 16311, lines 5-14)? It seems that these are two opposed possibilities. If the only first generation oxidation products are formed, then one should not expect highly oxidized products to contribute to nanoparticle growth. In short, the explanation of what organics are/are not contributing to growth and their respective oxidation levels is not very clear and needs to be made more precise.

2. The amine background levels (107 – 108 molecule/cm3, page 16302, line 5) seem to be comparable to typical ambient levels (page 16304, lines 10-13). Was any effort made to reduce the amine contamination? Additionally, it is not clear in the manuscript if or how amine concentrations were measured (e.g. page 16316, lines 10-14). If no reliable way of measuring the concentration was available, how were the amine concentrations approximated?

3. The authors conclude that in the first set of experiments (where no amine was added) about half of the growth was due to sulfuric acid condensation (e.g. page 16320, line 5) and imply that the rest of the growth (by volume?) was due to amines, despite no amines being intentionally added to the setup. It seems a bit unreasonable

that ammonia and amines make up the other 50% of the growth, since they would be much smaller in volume (and mass) than sulfuric acid. The authors should explain in more detail why it is reasonable to assume amines and ammonia are responsible for 50% of the growth.

Minor Comments:

4. Figure 3: It was not clear to this reviewer what the different lines refer to in this figure. Particularly, what does the dotted line represent?

5. Page 16304, line 5, and page 16318, line 12: Wikipedia should not be used as a source. The authors should find an appropriate reference, perhaps from the CRC Handbook.

6. Page 16304, lines 2-4, and page 16318, lines 17-20: The authors should search and reference appropriate literature studies of effects of gas phase basicity and molecular structure of amines in sulfuric acid clusters.

7. Page 16305, line 17: “form” should be “from”.

8. Page 16313, line 13: “dominate” should be “dominant”.

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