Atmos. Chem. Phys. Discuss., 7, S5054–S5056, 2007
www.atmos-chem-phys-discuss.net/7/S5054/2007/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ion-mediated nucleation as an important global source oftropospheric aerosols" *by* F. Yu et al.

Anonymous Referee #3

Received and published: 19 September 2007

General:

This manuscript investigates the importance of ion-mediated nucleation on the global production of new atmospheric aerosol particles. This kind of research can be considered very important at the moment, especially when considering the large uncertainties associated with the sources of aerosol particles in the atmosphere and their climatic effects. Before the paper can be accepted for publications in ACP, the authors should address carefully the issues given below.

Specific comments:

1. The relative importance of “neutral” and ion-induced nucleation has not been established yet. In fact, either of these two routes may be more important

depending on the location and time. With this in mind, I think that the authors should avoid arguing about the potential importance of ion-induced nucleation at one single location (Hyytiala, page 13600, lines 15-22), especially since the work of Yu and Turco (2007) has not been published yet.

2. Since the sulfuric acid concentration driving the nucleation rate is expect to be directly proportional to the OH radical concentration, the authors should say something about the ability of the GEOS-Chem model to predict the global OH concentration field (pages 13601-13602).

3. In introducing the calculation procedure for ion-mediated nucleation (page 13604), it should be explicitly mentioned that only binary water-sulfuric acid IMN is being considered. The statement like “JIMN can be accurately decided...” (page 13604, line 9) seems a little bit odd to me, given that theoretically calculated nucleation rates are practically always uncertain.

4. The modeled sulfuric acid concentrations should be compared briefly with measured concentrations (page 13605, lines 20-25). I understand that this comparison cannot be comprehensive due to the scarcity of measurement data. However, a few sentences demonstrating that modeled and measured concentrations are consistent with each other would strengthen the paper considerably.

5. The authors state that nucleation in Arctic region is hindered due to Arctic haze. (page 13607, lines 1-3) This may be true during the winter/spring period but not during the summer when the Arctic region is very clean due to effective precipitation. As matter of fact, active particle new-particle production has been reported in several Arctic locations during the summer time. The authors should be more careful here in their statements.

6. One cannot directly compare the ion-production rate and observed particle formation rate, since the vast majority of the ions are probably lost by pre-existing larger particles and oppositely-charged ions/particles before growing into observable size range of >3

ACPD
7, S5054–S5056, 2007

EGU

nm (page 13607, lines 20-26).

7. The authors should explicitly mention that in practically all heavily-polluted areas, observed nucleation rates are by far larger than what can be explained by ion production (page 13608, lines 1-3).

8. The statement (4) on page 13608 (lines 10-12) is incorrect. How could mixing itself create larger particle number concentrations than what has been formed by nucleation? When air is mixing, it inevitably dilutes, as a result of which concentrations are decreased.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 13597, 2007.