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Interactive comment on “Connections between atmospheric sulphuric acid and new particle formation during QUEST III–IV campaigns in Heidelberg and Hyytiälä” by I. Riipinen et al.

Anonymous Referee #2

Received and published: 2 November 2006

[The overall quality of the discussion paper]

The paper is well written . The authors applies the novel theory that is recently proposed by Kulmala et al (2006, ACP, 6, 787-793) to the observed new particle formation events in two different environment, and extend the analysis recently done by Sihto et al (2006, ACP, 6, 4079-4091). It seems important to have a larger collection of reaction coefficients and exponents, which are A, K, nJ1, and nJ3, from new particle formation events observed by other research groups at various locations. The collection of such dataset help the regional/global climate modeling community to model particle formation rate up to 3 nm by simple yet reasonably accurate analytical expressions.

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[Individual scientific questions/issues]

Page 10844, equation (5)&(6)

The authors assume that the particle size distribution function at the upper size boundary, n_6 , is represented by $N_{3-6}/3\text{nm}$. If we also assume that n_3 is also represented by $N_{3-6}/3\text{nm}$, we can solve for GR3. I wonder whether GR3 solved by this approach at the peak of J3 reasonably agree with GR1-3 obtained by the time-shift analysis.

By the way, we tried to apply this approach to predict the J3-6 and GR3-6 from our recent data from Mexico City. Our CoagS at 4nm in Mexico City is around $1.0\text{e-}3\text{ s}^{-1}$. It turned out that GR(3-6) predicted from J(3-6) was very noisy and GR(3-6) also often unreasonably large ($>100\text{ nm/hr!!}$). GR in Mexico City is too large ($\sim 20\text{ nm/hr}$); therefore, it is hard to observe any time shift between H2SO4 and N(3-6). I thought that the authors may find our observations interesting.

Page 10845, equation (7)

It seems better to choose a representative CS between time t and t' if CS changes during this time period.

Page 10846, line 12

I have read your description of author's approach several times. However, I am not convinced about how the author's method gives more statistical weight to the temporal evolution of data. It would be helpful for readers if the authors can elaborate this statement.

Page 10855 line 6

It would be nice if the authors can provide potential reasons for the coefficients A and K are near order of magnitude higher in Heidelberg. If the activation mechanism is the dominant process, is it possible that the concentration of thermodynamic stable clusters may be near one order of magnitude higher in Heidelberg? If the kinetic mechanism

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is the dominant process, is there any third body that can lead to the apparent increase the binary collision coefficient between two monomers?

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 10837, 2006.

ACPD

6, S4304–S4306, 2006

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