

Interactive
Comment

Interactive comment on “On the roles of sulphuric acid and low-volatility organic vapours in the initial steps of atmospheric new particle formation” by P. Paasonen et al.

Anonymous Referee #2

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This paper describes the model studies on predicting the formation rate of 2 nm particles by using activation and kinetic sulphuric acid nucleation mechanisms along with six other mechanisms involving a low volatility organic vapour which cannot yet be identified. The growth rates of particles from 2 to 4 nm in diameter were calculated from the measurements of gaseous H₂SO₄ and ultrafine particle size distributions performed in four different sites in Europe. The results showed that the most promising candidate compared with the measurements was the kinetic homomolecular nucleation of H₂SO₄ together with the heteromolecular homogeneous nucleation of H₂SO₄ and organic vapour.

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Interactive Discussion

Discussion Paper

The study is scientifically sound, and nicely applies the measurements to model calculations, even though explicit values of low volatile organic vapour concentrations are missing. I can recommend publication in ACP. However, I have some points that should be addressed before publication.

My main comment concerns equation (2) where the growth rate of 2 to 4 nm particles due to H₂SO₄ is calculated. The authors have assumed that the observed concentration of H₂SO₄ ([H₂SO₄]_{det}) is totally available for making the 2 nm particles grow to 4 nm sizes. Why the condensation sink of H₂SO₄ due to pre-existing particles has been eliminated? How much would the results change if the pre-existing particles are taken into account?

Other comments:

2.2.2 What is the time resolution of CIMS? Were all measurements performed at ground level?

2.3. lines 15-17: "... and the concentrations of H₂SO₄ and other vapours condensing on sub 4 nm particles" should read ...on 2-4 nm particles.

2.3.1 lines 9-13: please, clarify these two sentences.

2.3.2 line 14: assumption of density of 1200 kg/m³ needs a reference

2.3.4. line 26: please, add a reference for parabolic differentiation algorithm whereas the lines 9-10 (2.4.2) give too elementary information and thus are not necessary

3.1. lines 3-4: I assume that the 16% variation in C(GR=1,H₂SO₄), due to temperature and RH between the different sites, has been taken into account. However, the uncertainty due to the two CIMS instruments was as high as up to 50%. How did you handle this in regard to C(GR=1,H₂SO₄)?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 11795, 2010.