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Comment

## ***Interactive comment on “Modelling Ag-particle activation and growth in a TSI WCPC model 3785” by F. Stratmann et al.***

### **Anonymous Referee #1**

Received and published: 11 December 2009

Review of manuscript amt-2009-61 “Modelling Ag-particle activation and growth in a TSI WCPC model 3785” 11 December 2009

General Comments: This is a very good and well-written paper. The authors have quite nicely combined a computational model with measurements to describe activation, growth, and detection of particles using a TSI 3785 WCPC. Their results are useful to the aerosol measurement community, and in particular those scientists who wish to use this instrument (or one of a similar design) to measure particles smaller than 10 nm.

Specific Comments: p. 2221, line 7: The only description of the model used for homogeneous nucleation is the Girshik et al. (1990) reference given here. There is also a reference to Fine Particle Model for FLUENT, but the user manuals and details of

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how homogeneous nucleation is handled in the model are not easily available at the Particle Dynamics web site. My concern is this: the Girshik formulation is well-known to produce rates that are about three orders of magnitude high (Du et al., Phys. Rev E, 79, 021604, 2009). The temperature dependence is not bad, but the rates are much too high. Are the authors aware of this? And does the Fine Particle Model for FLUENT scale the homogeneous nucleation rates to account for this known error? The acknowledgement of the error and its correction probably does not need to be in the body text of the paper, but maybe it should be included as a footnote?

p. 2229, lines 3-10: It appears that Fig. 6a is inconsistent with Fig. 5b. In Fig. 5b essentially all 15 nm particles are activated out to concentrations  $10^6 \text{ cm}^{-3}$ . In Fig. 6a, however, the light blue points are for a 15 nm concentration of  $10^6 \text{ cm}^{-3}$ , and these points show no significant growth (i.e., particles well less than 1 micron – the assumed detection limit). If this is not an inconsistency, it needs to be explained!

p. 2229, lines 12-13: Summary point a) appears only to be true for small particles. If Fig. 6a is correct, the authors actually should see counting efficiency drop off for very high concentrations of 15 nm particles. In any case, the trend in Fig. 6a appears clear: as the size of the particles increases, vapour depletion might be expected to play a larger role. I see the results as very specific to the two sizes modeled – if that is not the case, the authors need to state that, and show why it is true for particles of all sizes.

p. 2231, lines 11-16: The last sentence is a little vague. Maybe the authors could highlight the conclusion by giving 3-4 concrete examples. Possible situations to consider include 1) an urban area with large numbers of combustion particles; 2) a rural area where you experience a nucleation/particle growth event; 3) a clean rural/remote continental site; or 4) middle/upper troposphere; etc..

Technical Corrections: p. 2224, line 12: the parameters  $L_v$ ,  $k$ , and  $f_{\text{heat}}$  are not defined in the text following equation (10).

p. 2226, lines 5-6: This sentence is written with a double negative, which is not what

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the authors intended. Either delete the word “not”, or change the “neither . . . nor” to an “either . . . or”.

p. 2226, line 25 and Figure 3: please use theta ( $\theta$ ) instead of alpha( $\alpha$ ) in figure 3. The alpha is confusing, since it is used in equations 1 and 2, and theta is the variable used for contact angle on p. 2223.

p. 2227, lines 9-12: Please reword this sentence – its grammar is quite awkward.

p. 2227, line 28: Change “Fig. 3” to Fig. 4”.

p. 2228, line 3: Change “Fig. 1” to “Fig. 2”.

P. 2228, line 17: Change “5b” to “5a”.

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Interactive comment on Atmos. Meas. Tech. Discuss., 2, 2217, 2009.

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