**Atmospheric
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Discussions**

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Interactive comment on "Charged and total particle formation and growth rates during EUCAARI 2007 campaign in Hyytiälä" *by* H. E. Manninen et al.

Anonymous Referee #1

Received and published: 10 April 2009

General recommendation

The authors made yet another attempt to elucidate new particle formation events using extensive instrumental set-up pushing particle detection boundary to lower sizes. It is truly amazing that after so much research efforts the authors and other groups have put into new particle formation mechanisms, the clear understanding of NPF is not quite in sight. The paper is a valuable contribution using comprehensive approach to the problem and as a whole I recommend publication in ACP. There are, however, minor issues to be resolved, mainly related to providing more details, which might be quite clear for a true specialist, but not for a more general reader.

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Specific comments

1. 2nd paragraph of the introduction should be swapped with the 1st one. Would be great to reference few studies on the growing list of locations where NPF was observed. Regarding Amazonian forest it is not clear whether measurements were not performed there yet or they were, but NPF was not observed.

2. Instrumental section should clarify better, why CPCB is able to measure particles down to 2nm when compared to DMPS system. There is also confusion with the statement that CPC cut-off diameter was 3nm. I believe it is due to the effect of using water CPC in CPCB. Otherwise, CPC used with butanol should be very similar in CPCB and DMPS systems. This issue is very central in later analysis. For instance, is it possible that systematic biases between CPCB and DMPS could artificially produce particles in 2-3nm range? Was there any specific control of the instruments to avoid such a possibility? The range of DMPS system is more often assumed to be up to 800nm as few uppermost channels are not reliable due to noise problems (poor statistics).

3. Author should provide more information on classification of event days into subclasses rather than stating them as Ia, Ib and II. What does it exactly mean "applicability e.g. to a growth rate analysis"?

4. Please give reason why NAIS data was not used in growth rate calculations, when all other instruments were.

5. Why the effect of coagulation was neglected in the growth rate analysis and what were those conditions allowing to do so?

6. Why GR3 in the size range of 1.3-3nm was calculated from BSMA data and not AIS as they covered the same size range?

7. Sensitivity analysis showed that estimation of coagulation sink had the highest impact on growth rate uncertainty. The assumption of coagulation error of 10% therefore needs strong justification.

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8. In the case of ion-induced formation rates, the median value of 10% should be accompanied by the uncertainty range of 1.7. That would put some of the other studies in better agreement with this study which should be discussed as well.

Technical corrections

p.5122. 10-15 lines. Style should be improved by removing repetition of "on the other hand".

p.5131 line 19. replace "indicating" with "which suggests".

p.5132 line 11. replace "different approaches" with "different measurement approaches" as it relates to different instruments I believe.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 5119, 2009.

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