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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Nucleation mode growth rates in Hyytiälä during 2003–2009: variation with particle size, season, data analysis method and ambient conditions" *by* T. Yli-Juuti et al.

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Response to referee 3

We thank the referee for his/her constructive and positive comments. We have now carefully considered them and will modify the revised manuscript accordingly. Here are our detailed answers to the referee's comments:

The authors present the study of growth rates of nucleation mode particles over 7 years of data collected in the boreal forest with three different classifiers. They investigated

the variation of GR between the instruments, over the seasons, years and particles size ranges. A comparison between two different analysis methods is presented. Methods and data are well reported and proper statistical analysis has been applied. This work confirms the results of previous studies updating and extending the analyzed time series focusing on the size dependency of GR and correlation with meteorological data. Although not crucial for the publication, I would consider shortening the paper, removing some redundancy, to improve the readability. I believe this paper is suitable for publication in ACP after minor revisions.

We have carefully revised the manuscript and tried to remove all the redundancies we found without sacrificing the accuracy of the presentation.

Major comments:

Page 21283, line 25: "This suggests that the processes and/or vapours limiting particle formation rates are different from those limiting the growth and survival of the formed particles to climatically relevant sizes". This was the same conclusion drawn by Dal Maso et al. in 2005, but is this really the only possible explanation? Couldn't the temperature's seasonal profile explain the same observation?

Our conclusion about different processes/vapour limiting the particle formation and growth rates is, as probably noted by the reviewer, based on the different seasonal patterns (and thus a non-correlation) of these rates. We agree with the reviewer that in principle such differences would be possible even if the same vapour was nucleating and growing the particles – as long as these two processes show a different seasonal dependence (i.e. dependence on temperature). If the same vapour(s) was forming the particles through nucleation and growing them to larger sizes by condensation, the lack of a peak in formation rate during summer would be possible if the nucleation was limited by the evaporation of the clusters (saturation vapour pressures) and condensa-

tion by the ambient vapour pressures. This would mean that the nucleation would be limited by evaporation where as growth would be limited by condensation. However, basing on the observations of sulfuric acid showing such a clear connection of atmospheric nucleation rates but not explaining the growth, we think it is very unlikely that it would be the same vapours that were responsible for both the initial cluster formation and their growth to climatically relevant sizes.

Page 21296, lines 16-19: "Based on the seasonal pattern of the GR of larger than 3nm particles and the correlations of GR with the ambient parameters the concentrations and O3-oxidation of BVOCs seem to be the most important ambient variables connected to the GR". This is quite a strong statement, I would be more cautious in the formulation of this sentence being in the conclusions, for instance stressing once more that the correlation of GR with ozone-oxidized organics is driven by one single data point (over 7 years), although already clearly presented in the section 3.3.

We agree and the conclusion will be stated more cautiously in the revised manuscript.

Specific comments:

Page 21274, line 26: "In order to reduce the fluctuation in the data, the original 6 min averaged data was converted into 15 min format". You could add which type of average you applied to the BSMA data.

The data was averaged with standard procedure of BSMA. The 15 minute averaged data is calculated by dividing each hour in quarters and taking arithmetic averages of the 6 minute averaged data points falling in these specific quarters.

Page 21279, line 10: "first-order polynomial fit", isn't it simply a "linear fit"? (same for

Page 21281, line 6)

We agree that it could as well say 'line'. We will clarify this in the revised version of the manuscript

Page 21280, line 22: "In this study, only the NPF events for which the growth rate fitting was successful were selected for the further analysis". Here it is not clear for the reader what does "successful" mean.

We selected only those NPF events for which there was no obvious reason for the GR calculation to fail due to e.g. air mass changes. This will be clarified in the revised manuscript.

Page 21287, line 16: Although you do not use it, you could mention whether or not it is feasible to apply the mode-fitting method to the AIS and BSMA data as well.

Mode-fitting method is not applied to AIS and BSMA data due the detection size ranges of these instruments. The log-normal mode can not be clearly distinguished from cluster mode in the small size ranges and on the other hand, the upper detection limits of these instruments are so low that, when the particles are grown to larger sizes, big fraction of the nucleation mode is not detected. Latter is the case especially for BSMA. Also, it should be noted, that if mode fitting method is applied to ion size distribution the result will be affected by the size dependency of charged fraction of particles. If aerosol particle population has equilibrium charge distribution, the fraction of charged particles will increase with the particle size. Therefore the median diameter of nucleation mode determined from ion size distribution (AIS, BSMA) is expected to be larger than the median diameter determined from total particle size distribution (DMPS). The size dependence of charged fraction is not an issue if maximum concentration method is used, as the method relies on the moments of maximum concentration, not on the absolute concentrations. We will add a note about this in the revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 21267, 2011.