

Interactive comment on “Non-volatile residuals of newly formed atmospheric particles in the boreal forest” by M. Ehn et al.

Anonymous Referee #1

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General comments

The article deals with atmospheric measurements of sub-micrometer aerosol size distribution preceded or not by a 280 °C heating system, for the study of the volatility of newly formed particles in the boreal atmosphere. New particle formation in the boreal forest has been extensively studied for many years. However, although the technique is not new, and similar measurements have been published from data collected in another rural environment, this volatility measurements give additional inputs in our knowledge of nucleation in this area by focusing on ultrafine particles.

This study lies within the scope of ACP.

Generally the paper is well written, easy to understand and well structured. However,

I have a few general remarks that should be addressed before the paper can be published:

Only six nucleation events have been explicitly studied, although three months of measurements are apparently available. It is not really clear how representative these dates are, relative to the whole of the nucleation events occurring at the site (which I believe are much more numerous). Are the studied dates typical of clean Northern air masses advection over the area? I would expect different condensing vapour during the growth of nucleating particle, whether the air masses studied are clean or polluted. The six cases seem to have, from the volatility point of view, similar features. Is it also the case with other basic parameters?

The non volatile core of the particles is observed to grow with time during nucleation events (and with the size of the selected particles, because the size of the selected particles is growing with time). Have any tests been conducted relating the size of a particle and the time needed for its volatile fraction to evaporate in the author's oven? In my mind, it is possible that the non-core apparent growth is simply due to a longer time needed for a larger mass fraction to evaporate. The only "quality insurance" given by the authors is the comparison between their set-up and the "Leipzig thermodenuder". It is difficult for the reader to be convinced of the quality of the set-up without having read the technical description of this other device in another paper. Hence it would be very helpful to have the essential information about this device presented here (i.e., residence time in the oven, evaporation efficiency as a function of size, what characteristics of the desorption section?..).

Specific comments

Abstract

One main implication cited in the abstract: "newly formed particles, ..., are unlikely to consist of sulphuric acid, ammonium sulphate, and water alone", has not been mentioned in the rest of the article. This affirmation should be mentioned or discussed

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somewhere if it is emphasised in the abstract.

2. Measurements

I would suggest to move the 4.1. “Losses of sampled particles in the heating tube” in this section, because the rest of the efficiencies of the set up are discussed here. Figure 1 gives the penetration efficiency of particles in the oven at 280 °C as a function of size. How was this calibration test performed (what was the nature of the particles, for example, maybe the electrometer mentioned on the figure legend should be mentioned in the text)?

4.3. Aerosol number concentration

I understand from this section that the total number concentration of heated and non-heated particles are compared between the SMPS in the “V” mode and the DMPS. I can understand this when looking at Figure 3 where, on the night of Julian day 85, the total concentration of the VSMPS is higher (almost by a factor 2!) than the concentration of the ambient SMPS, which does not allow any calculation of the volatile fraction. In this context, I do not think it is of any relevance to discuss the difference 90% to 100% fraction of non-volatile cores (line 15 of p.10411): the uncertainty on the measurements is obviously higher than 10%. One last question is: if in this section, the DMPS is preferred over the ambient SMPS as a reference, why use the ambient SMPS in the rest of the study?

5. Discussion

Last paragraph of the section (lines 8 to 14 of p. 10414) would fit better to section 4.3.

Technical corrections

Page 10414, “from 3 to 40 nm” instead of “from 3-40 nm”

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 10403, 2006.

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