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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Concentrations and fluxes of aerosol particles during the LAPBIAT measurement campaign in Värriö field station" *by* T. M. Ruuskanen et al.

Anonymous Referee #2

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General Comments

The paper by Ruuskanen et al. is a thorough summary of the various measurements made at Varrio during LAPBIAT. The measurements seem very sound and are of interest to a wider readership. The English is mainly excellent, with a few minor suggestions for improvement made below.

However, while the individual measurements are good, their reporting in this paper remains rather descriptive. In addition, there is no clear thread that ties the different measurements (snow deposition, aerosol physics, aerosol fluxes) together. As a consequence the conclusions and hence the overall message of the paper are not very

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strong. The discussion and conclusions sections, for example, contain common place statements such as: “Deposition measurements open new possibilities for interpreting the results”, “Snow sampling is a fruitful additional measure to an aerosol measurement campaign carried out in an Arctic.” and “Transport modelling based on atmospheric dynamics is also a useful tool for aerosol research.” The authors need to distil some more quantitative information from the paper to warrant publication in ACP. It appears to me that the individual datasets reported in this paper could have contributed to a suite of much stronger papers, if integrated with measurements elsewhere in the Arctic.

The only stronger conclusion of this paper is that the measurements further support the (fairly well established) theory that particle formation depends critically on pre-existing aerosol concentration. I agree that the measurements do indeed support this view. However, I have some conceptual problems with notion that more polluted air masses ‘switched off the nucleation event’. This would imply that the nucleation observed is a phenomenon occurring only at the point of measurements. Surely, the particles observed are formed upwind of the measurement site. The wind direction changed to advect more polluted air to the measurement site. In this more polluted air mass the nucleation probably never occurred (except possibly, upwind of the emission sources; but the data do not provide information on that). By contrast, in the clean air the nucleation event most likely continues, but it can no longer be observed at the measurement site. Thus, I suggest the authors change the language with which the change is described.

Scientific Comments

- p714, l12. Could the authors please add references to the PHAUCPC and LICPC which are non-standard instruments?

- P715. l15-l21. A laminar flow through a 4.4 m long tube should induce significant flux loss, which should be estimated. Clearly, this has no effect on the direction of the flux, but on the magnitude.

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- P718. l10. The under-counting of the APS 3320 for large particles is well documented. However, if the APS was calibrated against the MOUDI gravimetric measurements, the disagreement between calibrated MOUDI measurements and EAS (Fig. 4) would imply that either the MOUDI is losing material or that the EAS is over-counting?

- P718, l22 and P725, l11. Which process causes the high NO3- deposition during the 2nd sampling period, which is not mirrored by high NH4+? Is it likely due to (i) deposition and washout of NO3- particles that are not NH4NO3, (ii) deposition and washout of HNO3 or (iii) NOx snow chemistry? It seems the ions were not balanced during this period. Is there any ancillary information in the overall dataset? Could the combination of deposition and concentration measurements be used to derive effective deposition rates or scavenging ratios?

- P719, l4. The product of particle number concentration and gravitational settling velocity will provide a lower estimate of the deposition flux. At 5 m there is still a strong contribution of non-gravitational processes to the overall deposition velocity, especially over aerodynamically rough surfaces such as forest.

- P719, l19 and Fig. 6. Text and figure caption suggest that the graph should show mass fluxes. In fact it appears to show number fluxes!

- P720, l6. As described in the introduction above, the word ‘interrupted’ reflects, in my opinion, not the right concept. At the very least it is not the event that was ‘interrupted’ but the observation of this event.

- P721, l6. No indication appears to be given as to how these growth rates were derived / calculated. Is the dependence of growth rate on particle size consistent with other measurements? What is controlling it?

- P721, l14. Have the fluxes been filtered for unsuitable micrometeorological conditions, such as (i) low turbulence, (ii) non-stationarities and obstructed wind sectors (if relevant)? What are deposition velocities are implied by the deposition periods and

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how do they compare with the maximum theoretical limit of 1/Ra?

- P721, l28. References are needed to back up the statement that the direction of the flux during nucleation events is consistent with previous observations.

- P722, l5. According to Fig. 1, wind speed (and thus turbulence as further supported by the small values of |L| in Fig. 2) is very low on the morning of 30 April, and this is an alternative reason why fluxes during this period are small. It could be argued that fluxes cannot be measured in the morning and the time trace of Fig. 12 is therefore unconvincing. It does, however, show that fluxes are downwards during nucleation events.

- P722, l10. The hypothesis that circulation patterns associated with cloud streets is responsible for the fluctuations in the measured aerosol flux is highly speculative. If this were the case, the concentration should show similar fluctuation. Does it? The variability in the flux could equally be due to advection and storage errors during the changing conditions of the nucleation events, which are by nature non-perfect conditions for flux measurements.

- P723, l23. Which ions (presumably not measured) do the authors propose balances the large ammonium concentration in the accumulation mode?

- P724, l24-26. How was this coarse deposition flux derived from Table 3 and why is the agreement with the deposition estimate based on the EAS data so much better than suggested in Section 3.4?

- P725, l1. If values of 32 and 0 mg m-2 are judged to be in 'rather good agreement' with each other, one might wonder what the authors would have judged to have been poor agreement Ĕ But I do agree that a lot of assumptions went into the comparison.

- P728, l4. "facility's goal is to"

Technical Corrections

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- Title: better: “at Varrio field station”

- p711, l2: better: “in the free troposphere”

- p711, l15: “as a possible mechanism”

- p711, l17: “However, in some special cases Ĕ”

- p712, l16: better: “ to identify, the contribution from local diffuse Ĕ”

- p714, l7: “measurements are shown in Table 1.”

- p714, l9: “with an Air Ion Spectrometer”

- p715, l1: “continuous spectra.”

- P715, l5: “treated as a real sample”

- P715, l18. “Hyytiala” does not appear to have been introduced in the text and not all readers may be familiar with the SMEAR II station and the earlier flux work performed there. Maybe better introduce this as: “Ĕ to that used in previous flux studies in Southern Finland (e.g. Buzorius et al., 2001), except Ĕ”

- P716, l19: better: “temperatures above zero”

- P717, l3: “arriving at the SMEAR I station”

- P721, l28: “during the pollution episode”

- P723, l3: “The first week of the Ĕ”

- P723, l7: Please improve English of “while coarse mode around 2 um.”

- P723, l8 (and two occurrences l12): “of the Aitken mode”

- P723, l13: “presents the detailed contribution of each of the inorganic”

- There are further omissions of the article throughout the text, that should be corrected.

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- P726, l14. “similar to the recent”

- Table 2. Some additional spacing between rows would make the table easier to read.

- Fig. 6. The caption and y-axis label are inconsistent (see above).

- Fig. 15. Presumably this composition is based on mass loading (rather than mole or charge). Please specify in the legend.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 709, 2007.