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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Contribution of mixing in the ABL to new particle formation based on some observations" *by* J. Lauros et al.

J. Lauros et al.

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We would like to thank the referee for useful comments which have benefit our work.

General comments

We have compared mean conditions on event and nonevent days at 6–12 LT, as it is the most probable time of day for new particle formation. The time of sunrise varies similarly between event days as it varies between nonevent days. We believe that the time of sunrise cannot solely explain the observed difference in turbulent conditions. We have added a statement considering times of sunrise, in the end of Sect. 4.1: "The onset and strength of turbulence are connected to shortwave radiation which could partly explain the difference between event and nonevent day conditions. Therefore it would be interesting to compare the time of sunrise and onset of turbulence with the

onset of particle formation." Actually, we have done the comparison already (Nilsson et al., in preparation).

We agree that if the event days are driven by high emissions other factors become less important. However, we aimed to show that meteorological conditions differed between event and nonevent days. We believe that sources are out of the scope of this study and we are not directly interested in them. Therefore we have not done any assumptions (steady-state) related to them. Instead of giving an estimate of sources, we have given a relative source strength which is needed for a positive change of saturation ratio of organic vapor, section 4.3. In section 4.1. we discuss on turbulence conditions, which are not affected by sources.

The sodar wind components are calculated over 30 minute periods. It would be more interesting to have mean values over 10–15 minute periods when ascending and descending branches of large eddies could be observed. However, the relative strength of noise would (probably) increase too much if a shorter period than 30 minutes is used.

We have already commented on results by Mikkonen et al (2006) and therefore, we have not expanded the discussion considering if our results from logistic regression apply to other measurement sites. We have added a statement considering mixing (conclusions, first paragraph): We believe that the results can be generalized to other measurements sites where clean air is mixed at the top of the ABL.

Specific comments

p. 7538, l. 17: The actual organic vapor is unknown but the enthalpies of vaporization for atmospheric organic compounds are typically below 170 kJ mol^{-1}. The used value corresponds more likely mono- and dicarboxylic acids than PAHs.

We have rephrased: In this study $\Delta H_{os} = 170$ kJ mol^{-1} which is a reasonable value for mono- and dicarboxylic acids (see e.g. Strader et al., 1999).

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p. 7540, l. 22: We rephrased: Temperature and humidity were measured at six levels between (4.2, 8.4, 16.8, 33.6, 50.4 and 67.2 m) using ventilated shielded Pt-100 sensors and gas analyzers (infrared absorption), respectively. Wind measurements were carried out using an ultrasonic anemometer.

p. 7541, l. 7 In this paper, we need to estimate surface heat flux for the slab-model, which calculates analyzed variables. During early morning hours it is often even impossible to define a surface layer. When the ABL grows and the surface layer can be observed, the fluxes are not constant in the whole layer. The canopy height is 15 m and the measurement level is around 10 m above this. The level is probably located in the interfacial sublayer, below the constant flux layer. However, we consider the 23.3-m value the best and adequate estimate of heat flux at the surface because the magnitude, sign and behavior of flux are most probably representative.

p. 7541, l. 13 was $\rightarrow$ were. Thank you for the correction.

p. 7541, l. 15 We rephrased and added an explanation for event days in the begin of section 3: ...82 event days. The classification to event and nonevent days was based on visual analysis of particle data and following criteria for event days were used: a new mode had to start growing from nucleation mode size range and the mode had to grow and exist hours (Dal Maso et al., 2005).

p. 7543, l. 1: We added an explanation: The conversion is based on measured dry and wet particle size distributions at SMEAR II station and it is a function of particle distribution at dry conditions and RH.

p. 7543, l. 18 Thank you for the comment. Actually the RH dependence on CS has been taken into account via hygroscopic parametrization laakso04, and it is as referee mentioned non-linear process. However, water vapor itself will participate actively in condensation after $RH > 100$ %, before it is only co-condensing (see e.g. papers describing condensation growth like Vesala et al., 1997).

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p. 7546, l. 3 result → results. *p. 7546, l. 8* implemented → conducted. *p. 7546, l. 8* observe → assess. Thank you for the improvements.

p. 7546, l. 20–23 Actually, the value of CS is not constant in Fig. 5c. However, the altitude affects CS insignificantly (CS increases between surface and 500 m by 4 %) in comparison to meteorological effect. The red dashed curve in Fig. 5c corresponds the values given in Fig. 5a (now similarly a red dashed curve). We added a statement: ...(red dashed curve, the total effect in Fig. 5a)...

p. 7549, l. 2. This is a statement about the relative source strength of precursor gas(es) $Q/(C_0 S_{os}$ that is needed to get an aerosol formation event, not a statement about the aerosol formation strength, and hence it is in the unit of relative source strength. It is not source strength of aerosols, but of the precursor(s). The reason to why we give it as relative is that it should then be possible to compare this number with the source strength derived from different studies where organic emissions are measured/estimated, indifferent to what the condensation sink happens to be.

p. 7549, l. 21–22: This is an interesting question. Unfortunately, we cannot give any better estimation than a reader could do. Therefore we prefer not to add any new statement.

p. 7550, l. 23–24 We cannot quantify the effect but it has been discussed in the end of section 4.2. We rephrased: ... correlated (see Table 2) and therefore e.g. dependency on $d\theta/dt$ may reflect an effect which originates from RH.

p. 7551, l. 1 We added a statement: .. over three times larger data set of Hyytiälä measurements in comparison to our data set...

p. 7551, l. 14–15 We expanded the discussion: The event observations depend on real mechanisms in the ABL, e.g. entrainment and dilution. Even if our model does not include the effect of dilution, we know that this is correlated with simulated entrainment velocity and can draw conclusions based on this.... despite the fact that it could

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be expected that the probability increases if entrainment velocity increases due to a decrease of preexisting particle concentration.

p. 7551, l. 27: top of the atmosphere → top of the boundary layer

p. 7557, Tab. 1 The table include only correlations and the latter sentence should be removed. In addition, one line has disappeared.

$$w_e \quad d\theta/dt \quad \exp(CS_{z_i}) \quad \exp(P_{\mathrm{met},z_i}) \quad T_{z_i} \quad RH_{z_i}$$

Thank you for the remark.

p. 7557, Tab. 2: We added a statement: Every column of numbers corresponds to one logistic regression model. The mean values and standard deviations before a normalization have been given in brackets after the variables.

p. 7562, Fig. 4b: Done. (Actually, something has happened during the edition process.)

p. 7563, Fig. 5a: Colors/symbols have been changed, now the mean values are represented by crosses. The figure caption was changed respectively.

p. 7567, Fig. 9: A "d" preceding "theta" (line 4) was added. Thank you for the remark.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 7535, 2007.