

Interactive comment on “Airborne measurements of nucleation mode particles II: boreal forest nucleation events” by C. D. O’Dowd et al.

Anonymous Referee #1

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Review of O’Dowd et al., Airborne measurements of nucleation mode particles II: Boreal forest nucleation events.

General comments

The paper presents new experimental data which were obtained with great effort and at appreciable expense. Based on these data several conclusions are reached and the paper certainly deserves publication in ACP. However, the authors should significantly improve their presentation. They should clearly explain what they see in the data. This aspect has not been handled well, and although there observations and conclusions may be clear for specialists, to most other people (including myself) it is likely less what is seen in the data and what conclusions can be reached from these. Therefore I

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classify this abstract as 'accept with major revisions':

There still are several spelling errors (e.g. p.2822, l.3: analyzed; p. 2823, l.4: Truvnd or Tunved as in ref listing; p. 2828, l. 6: were or where?; p. 2828, l. 27: more or mode?; p. 2829, l. 3 space between over and which; l. 10: expands; p. 2830, l. 25, move ');'; l. 29: indicates).

The authors present many data in 14 figures, but hardly explain what has been observed. For instance, on p. 6 they describe the vertical structure presented in Figure 5 with a surface layer, a second layer and the free troposphere inversion at 1100 m, observed in the descent; Some explanation would be desirable, for me it is not clear why 'FT is at 1000-1100 m'. This is a very shallow FT.

The text on several figures is too small to read, in particular Fig. 15, but also Figs 1, 12, 13, 14

Detailed comments

Please indicate how layer heights are defined when discussed the first time for Figure 5.

In Figure 6 neither the figure nor the caption gives a hint to the difference between the two figures: I suggest that Figure and caption together are self-explained, or refer to the text if that would require too much text. Furthermore, in Figure 6 I see peak values at ca. 300 m, can the authors explain why they see them at 4000-500 m?

p. 2827, l. 2: Contrary to the authors' explanations of Figure 7, I could interpret the observations as homogeneously distributed with some outliers in layers at e.g., 100 m, 300 m, 480 m etc. Please provide more explanation.

p. 2827, l. 10: suggesting what? Apparently some text was deleted here.

p. 2829: l. 2829: is the conclusion that nucleation does not occur over sea justified? Or should it be that nucleation does not occur over ice? Since the sea has been reported

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in this article to be frozen, and this would make a large difference!

p. 2830: first par. The whole period shown is 0.06 days, since there is a ‘sequence of events’; ‘later in the day’; is correct, but the suggestion as I understood it is that there could be a longer time lapse between events; Here I suggest to show what the evolution of the surface and mixing layer were, please show in Fig 14.

p. 2831, l. 10: what is the basis for concluding that the time scale is 30-60 min?

p. 2831, l. 19: maps could confirm when the data were collected over forest or over lakes (also frozen)? I suggest that the authors check this rather than speculate.

Figure 15: I cannot really read this Figure, but it seems to me that a and b are the same figure?

The authors seem to use the term surface layer for the layer adjacent to the surface? I suggest they use common terminology, cf. Stull, Boundary Layer Meteorology, p. 10-11 for definitions.

p. 9 and figure 12: apparently nucleation start between 10 and 10:38, see also horizontal runs in Figure 13 and the profiles show signals first between 700 and 900 m; the signals in the horizontal runs are similar when descending from 300 m to 30 m and increase only after 87.450. Yet in Figure 14 the near surface concentrations are highest in each profile. Since Figure 14 is the basis for the final conclusion that nucleation occurs just above the canopy, the authors should explain better how this extrapolation was made, because the only profile supporting Figure 14 is the one in Figure 12 at 11:23. In all other profiles there is no indication whatsoever for the strong increase close to the surface as suggested by Figure 14. How do Figures 12 and 14 connect?

The authors make a large effort to show that particle concentration is not a conserved parameter. Why? They observe the formation of new particles in the boundary layer, so how could their concentration be conserved? And why would that imply that production

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occurs in the 'surface layer'; before mixing takes place? And why would that be close to the canopy (conclusion, last sentence), whereas we see peaks at 300 m in Figure 6.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 2821, 2008.

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