

Interactive comment on “Vertical and horizontal variation of aerosol number size distribution in the boreal environment” by Riikka Väänänen et al.

Anonymous Referee #2

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The manuscript submitted by Väänänen et al. presents aircraft and ground-based measurements sampled during 2 campaigns in 2013 and 2014. Both periods differ from each other due to differences in the meteorological situations resulting in differences in aerosol particle concentration.

The data set is very complex consisting of a high number of flight hours. Therefore, the analysis was focused on a statistical analysis of these data reflecting the differences between the stations. In addition, also case studies were presented and explained but not well enough interpreted. The whole paper is very descriptive and is lacking of deeper interpretation and conclusions. Thus, some more effort should be put in here.

There is also a number of typos in the text which should be corrected carefully.

Comments in detail:

Introduction:

There were also a number of studies in the late 1990s and early 2000 years I would like to see at least some of them mentioned here. And what is new in the study presented here?

Methods:

Was the sample flow dried before measuring? How often was the OPSS used? During some flight is not very specific. I did not recognize any data in the manuscript.

Page 5, line 12: PBL is the lowermost well-mixed atmospheric layer? No, PBL is not necessarily well-mixed, also a nocturnal inversion or the residual layer is part of the PBL. Stull (1988) explains the PBL in detail.

Page 7: The ARM site was operated in Hyytiälä in 2014, why no comparison with these data? Raman lidar? Other remote sensing instrumentation?

Results:

Page 9, Line 17 ff.: I do not completely agree with the description and interpretation of the median profiles. What means the differ 'largely'? This very vague, and I think the difference between the medians is not really large. Thus I think, in Fig. 3(a) the difference between blue and red below 1100 is not very obvious and I doubt that it is significant. Please check all these comparisons carefully.

Page 9, line 25: Here you probably mean the height of the well-mixed layer not that of PBL (is not the same as stated above).

Page 10, line 4: The concentration of 10-25 nm is in 2014 significantly enhanced compared to 2013, but not similarly as the total concentration, the increase in 3(c) is much more significant than in Fig 3(a).

Page 10, line 27: Does this mean the small particles were formed in higher altitudes?

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Page 11, line 15 ff: This mean no hint on NPF in higher altitudes but instrument discrepancy? Did you see this difference also in comparison experiments before or after the campaign?

Subsection 3.2.1: this description of number and statistics is a bit boring. An I miss completely an interpretation here. Do you think this is also due to instrument discrepancy? If yes, it is too high by far!

3.3.: Why did you use the following case studies? The whole section is very comprehensive and my question is if everything is needed. I would like to see some more general words in this introduction of the subsection, why you choose these cases and so on. Also, I miss some more interpretation. At the moment I think the number of case studies is too high and the description of the data is too long and a bit boring. It is simply not clear why all of these cases are needed.

Page 16, line 21 ff, and line 30 ff: Are talking here about the same spot? This is not clear to me. I see only one (or two if you add the descent) spot in around 2500 m height, which should be above the mixing layer. Maybe particles were small than 10 nm or the length of the event was shorter than 4 km corresponding to one full size distribution.

Figure 12: why is it not possible to see the elevated concentration in Figure 12(f). In 12(b), I can see it, but in (d) it is already impossible to conclude which one in the peak in the higher altitude.

Page 17, line 17: I think you mean Fig. 12(a) instead of Fig 13(a) here. The explanation for the NPF does not convince completely. Local maximum in wind shear means what, I miss the connection with NPF through enhanced turbulence. There are also turbulence parameters which can be calculated to prove this hypothesis. On which time scale does the NPF occur? If you talk about days with regard to backtrajectories it is hard to believe that turbulence might be responsible.

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Page 18, line 15: what happen with the condensable molecules while passing a cloud? Are they not affected at all? To me the hypothesis is not convincing that at first the air mass is enriched with required gases and later the accumulation mode is removed while passing a cloud. The cloud also influences the gases in the air parcel e.g. by entrainment.

Summarizing section 3: Interesting data, many plots, difficult to follow all arguments. The section is lacking of interpretation. I would like to see more information about the vertical structure: profiles of meteorological parameters, wind speed/direction. . . Obviously the NPF seems to be a locally restricted phenomenon in higher altitudes, why? Some of the results are really difficult to explain.

Figures:

There are too many figures and tables.

Labelling of figures is a bit confusing, e.g., in Figure 5 (b) is below (a) and in figure 8 (c) is below (a).

I cannot distinguish between inside BL and above BL from the symbols.

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