Answer to Interactive comment by Anonymous Referee #2

We wish to thank the Referee for the interest in our work and the valuable inputs on the manuscript. The comment in the following text are italicized and intended and the answers are addressed below them.

> *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.*

We admit that not all phenomena are interpreted completely. However, the view point of this paper is more a descriptive one. The NPF events in the atmosphere are complicated and multifactorial processes and there are still many poorly understood aspects related to them. The case studies represent days that differ from each other, but are typical in the dataset (the case III is an exception since it is the only night flight), and we thus think they all have their values when describing the NPF phenomena. We have trimmed the text and corrected the typos.

> *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?*

We extended the literacy review and clarified the objectives of the paper.

> *Methods: Was the sample flow dried before measuring?*

It was not dried. The temperature inside the aircraft cabin was warmer than the ambient temperature, which caused evaporation of water from the sample. Also, during the early spring campaign 2014, the ambient relative humidity was below 40 % and there was no need for drying.

> *How often was the OPSS used? During some flight is not very specific. I did not recognize any data in the manuscript.*

The analysis of OPS data will be presented in a separate manuscript. We added the number of the flights when OPS was used into the text.

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.

We thank the referee for this correction and checked the text.

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

In this paper we were mainly interested in the NPF and thus in the smallest particles which cannot be observed with Lidars or other remote sensing instruments. The ground particle data was used for comparison purpose, and we chose to compare with the continuous SMEAR II DMPS measurements, not with the campaign wise ARM DMPS data.

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.

We thank the referee for the comment. We clarify that the medians of the total concentrations do not differ clearly, but the shapes and variability does, and also the both considered size bins.

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).

This is also corrected.

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).

Please see the comment above.

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

Not necessarily, since the canopy losses reduce the particle concentration near ground. Also our analysis does not separate the altitudes but takes into account all that are inside the mixed layer.

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?

The side-by-side intercomparison after the 2013 campaign showed less than 10 % difference between Hyytiälä and Cessna values. Since the median differences of the total concentrations was very close to this, we think the result is not large enough to make strong conclusions. Also, since the CPCs used were not originally designed for airborne measurements, there is uncertainties if and how the flows adapt for changing pressure (particularly TSI 3776). In future, the calibrations and test with varying pressure would give insights to this.

> *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!*

We have trimmed the text. The variability of the smallest particles is the highest, and this observed large concentration variability during the NPF events reflects that.

> *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.*

As said in the first answer, the NPF events in the atmosphere are complicated and multifactorial processes and there are still many poorly understood aspects related to them. The case studies represent days that differ from each other, but are typical in the dataset (the case III is an exception since it is the only night flight), and we thus think they all have their values when describing the NPF phenomena. We merged and extended the introduction of case studies from the chapters 2.5.4 and 3.3, and placed it now only in the beginning of the chapter 3.3. We admit that not all phenomena are interpreted completely. However, the view point of this paper is more a descriptive one.

> *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.*

No, two spots are discussed. The first spot is about the high concentration spot within the ML, and the second one refers to the layer above the PBL. We clarified the text.

> *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.*

We modified the color scale of the concentration to be 0-6000 cm^{-3} instead of 0-10000 cm^{-3} to ease seeing the differences in the free troposphere.

> *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.*

Thank you for correcting the reference. We removed the turbulence part.

> *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.*

Thank you for this comment. We decided to remove this from the manuscript.

> *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.*

We admit that this paper is not offering explanation for some of the observations. Its aim is to be a descriptive paper. We have trimmed the text and added information of wind profile when the sounding data has been available.

> *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.*

Thank you for pointing the misleading figure letters. We clarified the figures and removed figures 6 and 13 c (ACPD version).