The paper by Altstädter et al. aims at showing evidence for the occurrence of NPF aloft under cloudy conditions and vertical transport of the newly formed particles close to the ground. Observations were performed from the Melpitz ground based station as well as from the unmanned aerial system ALADINA. The combination of these two datasets is of great interest, however the choice of the two case studies included in this work is not fully justified (6 cases available in total), and is even more questionable that instrument failures occurred during both of them. With the exception of some sentences which are unclear, the paper is well written. However, measurements/observations are often under-used, or used to support conclusions which sometimes do not seem to be correct, or inconsistent between the different sections. Also, references are lacking, which makes is difficult to evaluate the significance of this work compared to earlier studies; additional references would also help better understanding some of the tools/methods which are only very briefly introduced. All in all, I would not recommend the publication of the manuscript in its current form. However, because I think the dataset is highly valuable, and even if major revisions are needed, I really encourage the authors to consider submitting a new version of their manuscript. Here are some comments/suggestions to help in this process.

Specific comments

Comment 1, P1, L12: The use of « closure » is misleading as LES -model was used in the absence of measurements, and not to confirm observations.

Comment 2, P2, L9-12: I assume that the authors refer to the Class III events identified by Gröss et al. (2015) to support the occurrence of NPF under cloudy conditions. I think this reference should be used with caution in this context, as NPF event classification is based on a new approach in the abovementioned study. Thus, I would recommend to at least explicitly mention the use of this alternative method, especially because according to the authors those Class III events are "very weak events with very small-scale particle bursts that do not evolve into a fully developed and spatially distributed nucleation event. In any case, this class of observations includes what most researchers would call "non-events"". Also, in its current form the sentence on L9-12 sounds weird to me; I would either remove the last part, starting from "and these", or replace "is possible" by "might be possible".

Comment 3, P2, L19-21: The concept of "breaking waves" should be briefly recalled/clarified. Also, I assume there is a word missing in the last part of the sentence.

Comment 4, P3, L12: The expression "the appearance of nucleation and particle growth" is confusing, and should be replaced by something like "the appearance of nucleation mode particles and their subsequent growth", as the nucleation process itself is not observed at ground level.

Comment 5, P3, L16: Regarding the expression "In contrast to typical NPF events at ground by high incoming solar irradiance". It is in my view too simplistic and a bit confusing, as it suggests that only radiation is driving the occurrence of NPF events observed close to the ground. Some other factors such as the presence of primary precursors (e.g. SO_2) as well as the strength of their sink are also determining the process.

Comment 6, P4, L1-6: Was the effect of pressure on the CPCs detection efficiency accounted for in the data analysis or did you assume it could be neglected over the range of altitudes discussed in the present study?

Comment 7, P4, L27: I would suggest to move the reference to Manninen et al. (2010) to the next paragraph, where other NPF related results are reported, or find a way to "group" all the NPF related observations in section 2.2 in order to clarify the message.

Comment 8, P5, L10-12: More explanations should be reported about LIDAR and Ceilometer measurements/data analysis since they provide information on the occurrence of clouds, which are to some extent in the scope of the present work. At least, earlier studies involving these instruments should

be mentioned. In particular, the meaning of « range corrected signal » (captions of Fig. 3 and 5) should be explained.

Comment 9, P6, about the "Results and discussion section": It is surprising to have results about the frequency of occurrence of the phenomenon of interest in this study (i.e. occurrence of NPF in so-called non-favourable conditions) reported in the abstract (P1, L6-8) and in the conclusion (P10, L32 – P11 L2) which are not further discussed in the abovementioned section, or not even recalled. I would strongly recommend to introduce and discuss those at the beginning of section 3, before the case studies. In particular, the frequency of this process occurring close to the inversion layer should be for instance compared to that of "clear" banana shape NPF events detected at the ground level. This is needed to assess the importance of the observed phenomenon. Also, the choice of the two cases investigated in the present work is not clear, and should be supported by additional explanations, especially because data were missing from ALADINA during the selected days (CPC data during first case, meteorological parameters during second case). In specific, are those two cases representative of all 6 days when similar phenomenon was observed, or do they all display contrasting conditions?

Comment 10, P6, about "Section 3.1, Case I April 2014": In my view, the data shown in this section do not fully support the conclusions provided on P8, L10-19. In particular, I believe that some of the observations meant to evidence the transport of small particles downwards do not support the occurrence of such phenomenon. More detailed comments regarding this section are provided below.

Starting with ALDINA vertical profiles:

- Detection of small particles close to the inversion layer and/or in the vicinity of clouds has already been observed. Therefore, the authors should refer to earlier studies in order to highlight how their results are similar or differ from observations reported in the literature;
- As mentioned previously, it is complex to really "follow", and thus validate, the transport of particles downwards because of missing CPC data during 2 of the vertical profiles performed with ALADINA. Looking at the profile from 11:47 on Fig.4, high N_5 values are observed in a layer between 100 and 250 m a.g.l., which seems to be disconnected from the layer above 600 m a.g.l. where high N_5 values are also detected. The authors suggest that the small particles (N_{5-10}) observed close to the ground at 13:50 were transported from the upper layer (>600 m): how can they exclude the formation of small particles in the lower layer (100-250 m), and further transport of these specific particles close to the ground?
- Also, particles in the range 5-10 nm are observed at ground level with the CPCs onboard ALADINA at 13:50, while particle number size distributions measured at the same time with the SMPS do not show any clear change in this size range compared to the distributions measured earlier during the day. How do the authors explain this? It might be useful to show the values of N_{5-10} calculated from SMPS measurements, in a similar way as done in Fig. 9 and 10. The analysis of fluxes such as those discussed in the second case study might also give further insight into the processes observed from ALADINA;
- Last, have the authors evaluated the threshold of N_{5-10} above which the concentrations can be considered significant? Looking at the last profile (13:50) $N_{10} > N_5$ are observed above 800 m, and the magnitude of the resulting negative concentrations appears to be similar to the magnitude of some of the positive concentrations which are discussed in this work.

Now focussing more on ground based measurements; they do not seem to support at all the conclusions on P8, L10-12 ("the lifted layer of freshly formed particles was transported downwards, which can be further seen by the temporal appearance of the small particles of a few nm in diameter in the aerosol data at ground level"):

- Two of the small particles concentration peaks in the SMPS were observed before the occurrence of the cloud. This suggest that those were not formed under cloudy conditions (as

suggested by the title), and since they were seen well before the first vertical profile was obtained from ALADINA, they cannot really support/validate the observations from these profiles;

- Assuming the last two peaks observed during daytime might result from transport of particles downwards, why are they so sporadic? Which process (P8, L12 "other processes") could explain such a behaviour? The authors should discuss more the disappearance of these particles in the spectrum;
- It is also surprising that the authors did not discuss at all the fact that these peaks coincided almost perfectly with the peaks of NOx. This observation suggests a local source of anthropogenic particles; this is further supported by the occurrence of two of the peaks during the night, when NPF is usually not favoured.

Comment 11, P8, L15-19: The authors suggest that small particles observed close to the ground are originally formed close to the inversion layer, where, based on Fig. 4, the concentrations of large particles are significantly decreased compared to lower altitudes (and this idea is further mentioned in the conclusions, on P11, L34). This behaviour is thus not similar to what was reported by Rose et al. (2015), who found that in contrast NPF was occurring under "high" CS values, at least when compared to non-event days. Same comment applies to P11, L5-8.

Comment 12, P8, regarding instrumental setup and data analysis: Why are the instruments and methods applied different between the two case studies (eg: ceilometer/LIDAR, NOx measurement/no NOx measurement, no fluxes investigation/fluxes investigation)? I understand that LES-model was used in the second case in the absence of measurements from ALADINA. However, it would have been interesting to have similar simulations for the first case in order to evaluate the ability of the model to reproduce measurements, and give further insight into the reliability of the model outputs used in the analysis of the second case. Also, more information is needed regarding measurement of CO_2 fluxes and LWP, as well as additional description/explanation for Fig. 7a. Last, the authors should provide more information about the use of this fluxes analysis in the frame of studies dedicated to NPF: to which extent is this approach novel, was it used previously, was it modified compared to earlier work?

Comment 13, P9 and following: the authors refer to their observations as nucleation; this is not correct since only >7 nm particles are observed. The use of "formation events" (eg P9 L6, L9) is also questionable, and an expression such as "the appearance of small particles" would in my view better describes the observations.

Comment 14, P9, L5-7: Based on the surface plot of the particle number size distribution (Fig. 6a), the increase of the particle concentration seems to be seen up to 30 nm (instead of 20 nm), and it is thus not correct to say that these events coincided with a decrease of the particle concentration in the range 20-50 nm.

Comment 15, P9, L22-23: The sentence "The water vapour mixing ratio q increased during the day in the vertical distribution between 0 and 1500 m" is confusing and should be rephrased.

Comment 16, P9, Figure 8: "The vertical profile of turbulent kinetic energy… showed a strong connection with the structure of the ABL" (L24-25). What does this sentence mean? Also, regarding the analysis provided on P10, L6-8: it is quite complex to discuss/assess the effect of the parameters shown on Fig. 8 a and b since the vertical distribution of $N_{7\text{-}12}$ is shown to vary faster than the time resolution of the model outputs.

Comment 17, P10, L12: The expression "validate" might be too strong, as some of the profiles do not start from ground level, and thus do not allow for a direct comparison. This analysis is anyway very interesting, and, as mentioned previously, I would recommend to do it also for the first case study.

Comment 18, P11, L3-20: It seems that some of the conclusions regarding the first case study reported in this section are different from those provided in section 3.1. In particular, the sentence on P11 L13-14 "however ground observations did not catch the newly formed boundary layer aerosol" does not suggest anymore the transport of particles downwards, and contrasts with the sentence from P8 L10-12 "the lifted layer of freshly formed particles was transported downwards, which can be further seen by the temporal appearance of the small particles of a few nm in diameter in the aerosol data at ground level". Surprisingly, a possible connection with anthropogenic sources of particles is mentioned but NOx measurements shown in Fig 2b are not used to support this hypothesis. Also, the reference to Bianchi et al. (2016) should be reconsidered; the end of the sentence should be rephrased and the message clarified, as Bianchi et al. (2016) suggest that organic compounds from anthropogenic origin (and not CO) are involved in the NPF events they observe. CO is only used as a tracer for the anthropogenic origin/signature of the sampled air masses.

Comment 19, P11, L21-33: While the authors consider the possibility for the particles formed aloft to have disappeared on L24, they assert on L25-27 that these particles are those observed close to the ground after vertical mixing. These sentences seem to be a bit contradictory, and the authors should be more careful in providing their conclusions, and use some formulations such as "might", "could", "suggest" … In particular, the analysis of the particles and CO_2 fluxes used to support the conclusions seems to be uncomplete. Indeed, while CO_2 fluxes show similar behaviour during the events observed at 10:10 and 16:10, particles fluxes show contrasting behaviour, being slightly positive during the first event and negative during the second one. This cannot be simply summarized as "During sporadic formation events, significant deposition occurred, taken from the negative values of particle fluxes. In contrast, in between formation events, emission was observed, indicated by positive particle fluxes" (P9, L13-15). Also, as already suggested for the first case study, the authors should discuss more the disappearance of the particles.

Comment 20, P11, L23: "The maximum … and increased rapidly while descending". This sentence is confusing, please try to rephrase.

Technical corrections

P3, L14: "high" should be replaced by "large".

P5, L10: "and" instead of "und".

P6, and following sections: For clarity I would suggest to give all particle concentrations as N, and not dN/dlog(dp).

P6, L8: "of" instead of "on".

P7, L11-12: I would suggest to change the expression "in the particle diameter". The authors can simply refer to N5, N10, N5-10, N390, N500 and N700 as those were defined before.

P7, L31: Please try to reformulate the expression "particles belonging to the diameter".

P8, L32: I would suggest to change the expression "in the particle diameter".

P9, L1: "in the particle diameter range between 3 and 10 nm" instead of "of 3 and 10 nm".

P10, L6: "790 m" instead of "790 nm".

P10, L7: "N7-12 decreased to the total aerosol number concentration…". This is confusing, please try to reformulate.

P10, L16: "integration of a size scan".

P11, L7: "high concentrations of sulphuric acid".

P17, Fig. 1: I would suggest to show the location of the station on the map, even if it said that the station is located in the domain centre.

P18, Fig. 2: the ticks on the x axis do not ease the reading of the times (10 intervals for 6 hours). Same applies to Fig. 6.

P20, Fig 4: Logarithmic scales should be used for particle concentrations as it is very difficult to seen the variations of N5-10 and N700.