REVIEWER 2

Authors thank the reviewers for their useful and very interesting comments and for the time spent to review our work. Reviewers have common concerns which we addressed to the best of our possibilities:

- We clarified the introduction, the method and the results sections by moving some figures to supplementary material and by re-organizing the text.
- The abstract has been rewritten in order to highlight the most significant results.
- A discussion has been added in the paper on the limitations of our methodology to derive aerosol and fog microphysical properties and on the limitation of this work due to the location of the campaign and the weak chemical variability.
- We also added a discussion on the relevance of our results for numerical weather prediction.
- Cyrielle Denjean have been added to this papier as coauthors to acknowledge their contribution to discussion related to these correction.

1) It is acknowledged that the used SMPS does not give information on the aerosol particles with diameter larger than 496nm. Why don't you use data PALAS-WELAS to get an estimate on the particle concentration larger than that? Based on Figure 7 there is quite nice overlap and agreement between the instruments after hygroscopic growth in ambient conditions is accounted for.

We wrote (p9, l1):
"We also suppose that the concentration of aerosol particle larger than 496 nm can be neglected."
Because we did not have any instruments to measure size distribution above 496 nm under dry conditions. Figure 7 shows indeed a very nice overlap between the SMPS and WELAS size distributions. However the overlap do not covers all the PALAS-WELAS size range. The overlap is supposed to cover sizes up to the activation diameter. On figure 7.b) adding measurement from PALAS-WELAS would lead to an overestimation of the larger size diameter aerosols concentration. A "dry" instrument measuring higher diameter would be useful in future studies. Including Welas data would result in more errors.

2) The number of fog droplets is compared to both CCN at 0.1% supersaturation and N_200, which both are on average clearly higher than the observed droplet concentration. Without further analysis it is quite strongly said that the droplet concentration does not depend on aerosol. As the aerosol is measured up to 496nm and PALAS- WELAS instrument could be used for larger particles, it would be quite straightforward to analyze if the shape of aerosol size distribution is affecting the observed droplet concentration. Now it can be only concluded that N_200 and CCN(0.1%) are not good proxies for fog droplet concentration in polluted conditions.

Actually Fig 9.b) provide some answers on that point.
For an equal Nact we don't have the same Dd, meaning that the shape differs. However looking at the ratio Nact/N* on Fig 10.b), correlation is rather good, that suggest that the shape differs more on intensity than on form. That would also suggest that on a given diameter range, results on dependencies between N* and Nact would be the same whatever the threshold diameter. Indeed sensibilities tests made with threshold at 250 nm, 300 nm or 400 nm confirms it.

Sentence must be added in the text to explain that point.
We added in section Impact of aerosol particles on fog droplets concentration :
" Correlation when N ∗ was calculated from the range of critical diameter inferred during the campaign (150 to 400 nm). This suggest that N ∗ using a single critical diameter can be considered as a good proxy of the number of activable particles. This can be understood since the supersaturation occuring in fog has a narrow range of value. CCN at a supersaturation between 0.02 and 0.11 % are mostly composed of accumulation mode particles. »

Conclusions is that the concentration of activable particles is not a good proxy for fog droplet concentration, neither is Nccn(0,1%), while it could be a good one for other type of cloud.

We added in the conclusion:
"Concentration of activated particles using a single critical diameter can be considered as a good proxy of the number of activable particles."

3) The results presented are mainly for the first hour into the fog lifecycle although data would provide a nice possibility to analyze the whole lifecycle. Is there some reason except the comparison to pre-fog aerosol? Is the first hour somehow relevant for the whole cycle? For example in Figure 5, why does PALAS-WELAS see such a strong increase in the concentration whereas FOG-monitor values are quite constant in the morning just before fog dissipation? Is this the situation in all observations?

The present study focuses on activation processes so we only looked at the first hour. Once the fog formed, the aerosols can be captured by the droplets (activation, scanvenging), which are not sampled by the SMPS if they are bigger than 2.5 µm. Thus all aerosols contained in hydrated particles and droplets larger than 2,5 µm won't be measured. Or they constitute a big part of the aerosol we are interested in. This is the reason why we do not perfom activation study once fog formed.

In figure 5, increase of N palas-welas, may be due to increase of Naerosols, and thus to more hydrated particles at RH=100%. However as SS is lowering, no more droplets are formed. We do observed this behaviour in all our observations.

A companion paper is under preparation to describe the evolution of the droplets during the fog life cycle. We show what are the dominant process controlling the fog life cycle, an analyse of the thin fog, thick fog and thin fog becoming thick fog will be presented too.

4) In the end it is discussed that the droplet concentration in radiation fog is subject to a pronounced decrease in the droplet concentration while stratus lowering cases are not. This is quite obvious as in the beginning of radiation fog formation the whole fog layer is cooling (higher supersaturation maintained) and there is lots of aerosol particles present. While fog matures and grows in height, available particles are consumed within the fog and cooling is more efficient at the top of fog. See e.g . Boutle et al. (2018) or Tonttila et al. (2017). I do not see any point in the comparison to cumulus clouds where dynamics is totally different.

The way fog dissipates is still unclear. The LES study of Bergot (2016) showed evidences of the key role of dry downdraughts at the top of the fog layer on dissipation. Later, Waersted 2017 confirms with experimental data the importance of top processes for the fog dissipation. He performs a comprehensive study on the interaction with the upper layer and showed the importance of the dry state of the upper layer and the amplitude of the inversion. Mixing processes leading to dilution could be of importance.

Dilution ratio being as high as in cumulus cloud in radiative fog bring a new element on that point, meaning that turbulent mixing must be of importance.

Indeed, concentration of aerosols is suppose to be lower with increasing height but the supersaturation higher (it is growing with the fog optical depth increase and the dynamical set up). Impact on droplets concentration are thus not obvious and processes making droplets evolve in fog are still poorly know. Evolution of bottom concentration could be due to a vertical mixing from top to bottom of droplets in the fog layer, to an (de)increase of supersaturation at the bottom (certainly due to the dynamical set up) or to deposition, collision-coalescence, ect... Some models shows an increase of droplets concentration (close to the ground) during fog life cycle (see Stolaki et al, 2015) while we show a dilution with magnitude close to what happen in cumulus cloud. As we expect a positive contribution of dynamics on supersaturation, we have to consider this possible downdraught being of importance.

Sentences have been added in the section Impact of CCN concentration on fog microstructure to clarify this comparison:
" Recents work of Bergot, 2016 has shown, with a LES study, evidence of the key role of dry downdraught at the top of the fog layer on dissipation. These downdraughts would even then reach the surface allowing to sun to warm it. Waersted, 2017 using experimental data, confirms the importance of top processes for the fog evolution. Dilution ratio being as high as in cumulus cloud in radiative fog bring a new element on that point, meaning that turbulent mixing must be of importance."

And in the conclusion:
meaning that mixing with clear air may be of importance for fog microphysic evolution.

5) The big question is what is the relevant droplet concentration for numerical weather prediction and climate modelling purposes? Is it really the number of droplets actually activated or some other value accounting also the biggest hydrated aerosols? I would like to see some discussion on that.

We agree with the reviewer and add a discussion on that point on the discussion session:

«In a modelling purpose, distinguishing hydrated aerosols particles from droplets allows to calculate an accurate repartition of the vapour deposition on the droplet size distribution. Indeed, behaviour of hydrated particles and droplets is quite different, the second one grow as long as there are exceeding vapor while the first one stay at an equilibrium diameter. The second one may grow enough to produce drizzle and have strong interaction with radiation. A clear distinction should improve the representation of processes in numerical weather prediction model and so on the visibilities forecast. »

We add in the introduction :

« Moreover as shown by Boutle,2018 an accurate representation of droplets concentration for fog could also impact climate projection. »

6) It is concluded that the activated fraction mainly depends on the aerosol size. I do not agree on this statement. In Figures 9 and 10 I only see that the critical size, for activation and fraction of activated particles anti-correlate, but this does not say anything on the mechanism driving this relationship. Not the size of aerosol particles at least. Instead Figure 10a gives some indication that particle chemistry might have some role in activated fraction. To really make any conclusions about the effect of size or chemistry, the information of aerosol size distribution above 200nm should be used. This is available, so I don't see any reason why not to use it.

Figures 10-b) shows that as the dry diameter decrease, concentration of activated particles increase. But as said in answer to 2), size is actually hidden in N* (intensity differs more than shape for particles larger than 200 nm).
Looking at what happen for equal Nact on Figure 9 b) different diameter can correspond, for example Nact = 100 cm-3. However looking then at Figure 10 b), ones can see a very good correlation between dry diameter and ratio. That mean that, for points of equal Nact with different dry diameter, points with smaller Dd have also a lower N*. Thus points with lower concentration at larger diameter activate at lower diameter. That mean that size diameter distribution does have an impact on concentration of activated aerosol.

Then looking at the link between N* and Nact (Figure 11), one can observe a modulation of the concentration of activated particles with the N*. For high N* we do not observe high Nact (on the contrary to what could be expected). Our explanation is that high concentration of aerosols low down the supersaturation by vapor captation by hydrated aerosol.

We add in the Impact of aerosol particles on fog droplets concentration session :

"Nevertheless, this figure (11) also shows that, for high concentration of activable particles (N ∗ ,lower N act are obtained. As suggested by Bott et al. (1990) this could be due to a lowering of vapour supersaturation with the hydration of the numerous aerosols particles."

and in the conclusion:

"But Nact could be modulated for high concentration of aerosols particles. Hydration of numerous aerosols particles could indeed lower the supersaturation."

7) Visibilities and LWC values are discussed but not shown. Comparison between visibility and droplet concentration could give some idea how relevant is the role of activated droplets when compared to hydrated aerosols in different cases. Even more if visibility at both measurement altitudes is given.

A comparison of the extinction as measured by the visibilimeter and the FM-100 [2-50]µm have been done on the three seasons (2010-2011, 2011-2012 then 2012-2013), we observed a strong correlation over the seasons. For a given episode evolution is similar with some exception that may be due to the wind direction (and the location of the head of aspiration of the FM-100). A specific study should follows on the instrumentation intercomparison.

Moreover, discussion on visibility should be included in the second paper of this study on the fog droplets evolution during the fog event.

8) Figure 4 does not give support for understanding the method for iterating critical size and supersaturation. Please try to improve it.

We agree with the reviewer, Figure 4 has been improved.