Aerosol-fog interaction and the transition to well-mixed radiation fog: 2nd Response to reviewer 1

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We thank the reviewer for their follow up to our previous response - being able to discuss things like this is a huge benefit of the ACP format. Responses to the follow up are included below, along with a description of how the manuscript has been altered to address both sets of comments – any page or line numbers refer to the latexdiff file.

1. Demonstrate the usefulness of using 3 types of models (LES - NWP - climate model) : I totally agree with you about complementarity between observations and LES. LES are a great tool to better understand the processes occuring during the fog life cycle

We have tried to clarify the roles of each type of model in the introduction (P2, L15-19), making it clear that our focus is on evaluating and improving the NWP model, but we are utilising the LES as a process model to understand the observations. We have also added more discussion on the usefulness of the LES model (P3, L8-17), particularly to comment on how we are not reliant on a traditional parametrization of aerosol activation.

I also agree that the ultimate goal is to improve NWP simulations, and a statistical study could demonstrate that this goal is achieved. In my opinion, the statistical validation of NWP is very interesting and needs to be included in the revised version. However, for fog (rare event) I am not sure that probability of false detection (b/(b + d)) would be the best indicator of false alarm because d $\gg$ b. I prefer the false alarm rate : b/((a + b)

The statistical validation has now been included in the paper (Fig 12, P14 L10 - P15, L9). You have also spotted a typo / inability on my part to read the documentation - what we present as probability of false detection is indeed the false alarm rate b/(a+b).

I am not convinced by the usefulness of climate simulations. In my opinion, this

part of the article makes the manuscript more confusing without added scientific values.

We agree that the climate simulations are distinct from the rest of the paper. This is why they are included at the end of the conclusions section, essentially as a corollary to the rest of the paper, rather than part of the main text. However, we feel that it is an important issue, which has not been raised anywhere else in the literature, but does have important consequences for the climate modelling community. We have now included a reference to Vautard *et al.* (2009) (P17, L8-10) to further motivate why this is so important. As such, we would like to retain them in the paper as motivation that future work is needed on this topic.

For the LES, it would be useful to study in detail the variability found in LES simulations, and to validate it with observations (if available). Moreover, what is the impact of microphysics in this variability?

We agree that studying the variability in the LES would be useful. The observations taken as part of LANFEX were not targeted at the scales of LES variability - stations were located several kilometres apart, e.g. in adjacent valleys or on hilltop locations, to investigate the mesoscale variability in fog formation and evolution. Therefore studying the variability in our single LES would be somewhat unconstrained and distinct from the main focus of this paper. With that in mind, we are planning an LES intercomparison based around this IOP1, in which the variability and effects of microphysics on this will be a much greater focus. Therefore this is left as future work.

I also have questions about activation processes in LES model. Given the time step used in LES study, I am not sure that a direct coupling between microphysics and LES updrafts (turbulent updrafts) is the best way to modelize the activation process. What is the representative time for activation processes? Is it compatible with the time step used in LES or with the lifetime of turbulent updrafts?

The timestep of the LES is limited to a maximum of 0.5s, and in practice is dynamically shortened to between 0.2 and 0.3s after the turbulence within the fog has formed. Thus the timestep is clearly shorter than the time needed to simulate cloud activation, which occurs on the order of a few seconds depending on the updraft velocity or cooling rate. As far as we know, the direct coupling between the turbulence and microphysics is currently the only way to accurately simulate the activation, and also the evaporation of droplets within fogs or cloud. Typically used parametrizations for droplet formation are targeted at calculating activation at the cloud base and those are not valid in conditions with pre-existing droplets. With direct coupling we are also able to take into account the kinetic limitations related to condensation, i.e. particles are not always able to exceed their critical size and activate into droplets even though the ambient supersaturation might be higher than the particle critical supersaturation for a short period of time. We have included a brief comment on this in the revised manuscript (P3, L24-26).

2. Validate the microphysical parameterization: agree

We have included a summary of the previous discussion in the revised manuscript (P3, L8-17).

3. Validate the numerical model used and particularly the frost-dew deposition: I agree with your reply. Deposition (dew and fog settling) is clearly an important process in the formation phase of a fog layer, and I agree that it is an area in which NWP are deficient. Given the instrumentation deployed during LANFEX, you could perhaps discuss this point and discuss the impact of your modification on water deposition on ground. I think that the total water deposition on the ground could be more useful than the evolution of the specific humidity at screeen level.

We have included the water deposition rate figure in the revised manuscript (Fig 4a), and the previous discussion about dew deposition rates and the results of Guedalia and Bergot (1994) (P6, L29 - P7, L6). This actually re-enforces our argument about the water contents being correct and the droplet numbers being the primary source of error, so thank you for this suggestion.

For the soil-atmosphere exchanges, It would be nice to discuss the limitations of the appraoch used (imposed surface temperature and consequently no interaction between land and atmosphere). During the formation and dissipation phase, it seems that the surface - atmosphere interactions have a huge impact on fog life cycle. Therefore, your approach could be limiting.

Yes, we agree that surface-atmosphere interaction is relevant for the life cycle both in terms of formation and dissipation. The goal of the LES exercise presented is not to study how environmental conditions are affecting the fog, which has been done in detail previously (e.g. Bergot *et al.*, 2015; Maronga and Bosveld, 2017). Here we have used the LES only to understand the coupling between aerosol and droplet activation, interaction with radiation and fog dynamics in the observed conditions. We have added a comment to the revised manuscript (P3, L19-22) to discuss that this may be a limitation.

4. Contribution of this study with respect to bibliography : Agree fog Bott (1991). Your work should be discussed with respect to this reference.

We have now placed the work of Bott (1991) up-front in the introduction as clear motivation for our own work (P2, L5-11, 15-16). We have also now stated that points (i)–(iii) are a summary of the mechanisms affecting the development of well-mixed fog (P16, L11), not necessarily our own contribution.

The comparison of your results with the results of Maronga and Bosveld (2017) should be added in the revised version. I agree with your reply but this point should be clarified in the revised version.

We have included a paragraph on the comparison of our results to Maronga and Bosveld (2017) in the conclusions (P16, L20-29).

References

Bergot, T., Escobar, J., and Masson, V. (2015). Effect of small-scale surface heterogeneities and buildings on radiation fog: Large-eddy simulation study at parischarles de gaulle airport. *Q. J. R. Meteorol. Soc.*, **141**, 285–298.

Bott, A. (1991). On the influence of the physico-chemical properties of aerosols on the life cycle of radiation fogs. *Boundary-Layer Meteorol.*, **56**, 1–31.

Guedalia, D. and Bergot, T. (1994). Numerical Forecasting of Radiation Fog. Part II: A Comparison of Model Simulation with Several Observed Fog Events. *Mon. Weather Rev.*, **122**, 1231–1246.

Maronga, B. and Bosveld, F. (2017). Key parameters for the life cycle of nocturnal radiation fog: a comprehensive large-eddy simulation study. *Q. J. R. Meteorol. Soc.*, **143**, 2463–2480.

Vautard, R., Yiou, P., and van Oldenborgh, G.-J. (2009). Decline of fog, mist and haze in europe over the past 30 years. *Nat. Geosci.*, **2**, 115–119.