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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Is a more physical representation of aerosol activation needed for simulations of fog?" *by* Craig Poku et al.

Anonymous Referee #2

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Recommendation: Major Revision

General comments: The manuscript proposes an improvement of aerosol activation parametrization for LES of fog. This is an interesting topic as most of LES of fog now use 2-moment microphysical schemes and also produce an overestimation of cloud concentration and mass, leading to a too rapid transition into optically thick fog. Whilst the topic is important, I feel major modifications to the manuscript are required, with substantial inputs essential for publication. To be frank I hesitated with a rejection.

My major concerns are that:

- The improvement brought by the SMOD scheme is not convincing, and it is a problem as it is the main objective of the paper. The authors argued a more realistic approach

but it is a theotical point of view. In fact there is no improvement for IOP1, and all the simulations show significant differences with the observations.

- The initialization of aerosol is chosen to limit the discrepancy with the observations but is not realistic, and not representative of the clean air typically found at Cardington. Indeed, direct observations of aerosol concentrations were not available for this case, but we can rely on typical measurements over Cardington. The authors would argue that the initialization of a single accumulation mode with 100 cm-3 has already been used in Poku et al. (2019) but it was already unrealistic. The reference paper for IOP1 for the time being is Boutle et al. (2018), who proposed an aerosol distribution initialised with 1000 cm-3 concentration of Aitken-mode aerosols, 100 cm-3 accumulation-mode aerosols and 2 cm-3 coarse-mode aerosols. Authors also argued that Aitken mode aerosol can be ignored, but they have to prove it by running a simulation with the 3 modes and the initial concentrations proposed by Boutle et al, and by comparing it with the present simulation. I am not at all convinced by this equivalence, and by the justification to neglect the Aitken mode. This comparison is required for the acceptance of the paper.

- In the same way, the fog life cycle is only presented during 10 hours, but it would be more interesting to present the whole life cycle until 12 UTC as in Boutle et al. (2018): we can indeed suppose that it is not shown as the simulations would depart too far from the observations.

- Too few observations are used to evaluate the simulations: other temporal evolutions, for temperature, vertical velocity variances, sensible heat flux are available for this IOP, as well as vertical profiles of cloud mixing ratio, droplet concentration, droplet diameter. New comparisons need to be added in order to prove that SMOD is more realistic. They are also required for the acceptance of the paper.

- The study is not presented in a larger context where major papers have introduced some advances in the activation parametrization. Hence Thouron et al. (2012) for stratocumulus and Lebo et al. (2012) for deep convective clouds studied the relevance of saturation adjustment for LES. Then Schwenkel and Maronga (2019) studied the activation parametrization for LES of fog, but the authors only mention this last study in the conclusion. The necessity to consider the radiation cooling in the supersaturation evolution equation for fog is not new. So my general question is: what does this paper bring compared to the previous studies ? If new results are indeed shown, then they should be presented in the context of these other studies. It would be also necessary to compare the SMOD equations with the equations of Schwenkel and Maronga (2019) for instance.

More minor concerns are:

- For the visibility calculation, why not to use a direct calculation according to the Koschmieder (1925) equation, linking the visibility to an extinction coefficient function of the DSD, through the Mie theory, instead of a diagnostic from Gultepe et al. (2006), which could be questionable ?

- MONC needs to be presented.

- l 224-225 : the remark about the grid spacing is hardly understandable: why is it critical to run MONC at 1m or 2m resolution? The same sentence has been written in Poku et al. (2019) and it was already misunderstood.

- The radiation scheme SOCRATES is called every 5 min. Is it not too large for a LES of radiation fog, with a necessary accurate estimation of radiative cooling ?

- l 254 : it is said that radiative cooling is the biggest source of saturation. But it would be nice to compare it to the total temperature tendency, or to show that the consideration of the turbulent contribution to the non-adiabatic temperature tendency does not change the results.

- l 320 : there is a reference to the impact of surface heterogeneities, but it is not clear why this discussion is introduced here. Bergot et al. (2015) considered buildings,

while Mazoyer et al. (2018) and Ducongé et al. (2020), which considered trees and orography (over Lanfex) heterogeneities respectively, could be added.

References : - Ducongé, L., C.Lac, B.Vié, T.Bergot, and J.D. Price, Fog in heterogeneous environments : The relative importance of local and nonâĂŘlocal processes on radiativeâĂŘadvective fog formation, Quart. J. Roy. Meteor. Soc., 146, 2522-2546, 2020. - Lebo, Z. J., Morrison, H., & Seinfeld, J. H. (2012). Are simulated aerosol-induced effects on deep convective clouds strongly dependent on saturation adjustment?. Atmospheric Chemistry and Physics, 12(20), 9941-9964. - Thouron, O., J.-L. Brenguier, and F.ÂăBurnet, Supersaturation calculation in large eddy simulation models for prediction of the droplet number concentration, Geosci. Model Dev., 5, 761-772, 2012.

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