Review 2 of ACP-2020-904 : « Is a more physical representation of aerosol activation needed for simulations of fog?" by Craig Poku et al.

Recommendation: Minor Revision

General comments: Based on reviewer comments, the paper has been significantly improved by providing a more detailed comparison with IOP1 observations and expanded the discussion to account for highlighted discrepancies in the work.
However a few last concerns already raised need to be better introduced before acceptation and I insist on it.

My concerns are that:

- Concerning the initialization of aerosol I still do not agree that a single accumulation mode with 100 cm^{-3} is representative of typical measurements for a clean rural site similar to Cardington. In Boutle et al. (2018), it is clearly said that aerosol distribution representative of the clean air typically found at Cardington is 1000 cm^{-3} concentration of Aitken-mode aerosols, 100 cm^{-3} accumulation-mode aerosols and 2 cm^{-3} coarse-mode aerosols. I understand that you have not the possibility for the moment to use a multi-mode aerosol spectrum which is perfectly admissible. But I am not at all convinced that considering 100 cm^{-3} accumulation-mode aerosols is equivalent as you rely on tests not shown. Therefore you have to say that: i) an aerosol distribution of 1000 cm^{-3} concentration of Aitken-mode aerosols, 100 cm^{-3} accumulation-mode aerosols and 2 cm^{-3} coarse-mode aerosols as proposed and used in Boutle et al. (2018) would be better representative of the clean air typically found at Cardington but cannot be used in this paper; ii) the assumption of a single accumulation mode with 100 cm^{-3} probably limits the overestimation of droplet concentration that would lead to a too rapid transition to a thick fog layer.

- You have not answered to my previous question:
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 ?

- Line 470: The reference Thouron et al. (2012) for stratocumulus needs to be cited for the prognostic supersaturation in the same way as Lebo et al. (2012) for deep convective clouds.

Reference :

- Thouron, O., J.-L. Brenguier, and F. Burnet, Supersaturation calculation in large eddy simulation models for prediction of the droplet number concentration, *Geosci. Model Dev., 5,* 761-772, 2012.