Second review of the paper acp-2018-1139 « Large-eddy simulation of radiation fog with comprehensive two-moment bulk microphysics: Impact of different aerosol activation and condensation parameterizations» from Johannes Schwenkel and Björn Maronga

General comments : Significant improvements have been brought in this second version and authors have made significant efforts to address criticisms. For instance a prognostic approach of supersaturation has been added and has made a substantial contribution, allowing also to correct a bug. But there are still some weaknesses, inaccuracies and confusion, making the paper not suitable for publication in ACP. Therefore I recommend a second revision before publication.

My major concerns are :
- The sensitivity of the supersaturation parametrization is presented in 2 parts without a clear link between them, and the key conclusions are not clear. Indeed, a first part (4.2) refers to 1-moment microphysical scheme (as nc is fixed) and concludes to the negligible sensitivity of the supersaturation parametrization. But this test is not interested as firstly most of LESs use a 2-moment scheme, and secondly a prognostic saturation is only of interest if droplet concentration is prognostic. It would have no sense if a prognostic saturation scheme was associated with a 1-moment scheme. The second part (4.4) refers to 2-moment scheme and concludes to the importance of supersaturation parametrization as LWP is significantly changed. This 2nd test is the most interested. Additionally, these 2 parts are separated by a sensitivity test of activation parametrization (4.3). Therefore the conclusions are confusing and the paper does not appear beautifully built. From my point of view, the best would be to remove the test of supersaturation parametrization with 1-moment scheme. But if the authors want to keep it as I suppose, it is necessary to merge those parts (with 2 subparts : 1-moment and then 2-moment scheme) and to enhance the conclusion with the 2-moment scheme. The main conclusion will be in agreement with Thouron et al. (2012) with a new aspect concerning application to radiative fog. The conclusion must be revisited too, considering this aspect.
- Concerning the supersaturation parametrization again, that would make it clearer if the method called « explicit supersaturation calculation » was replaced by « diagnostic of supersaturation » to be distinguished from the prognostic approach (as the prognostic approach is also explicit). This would require to replace EXP with DIA in all the text and figures. For the prognostic supersaturation, it is not clear if the supersaturation is advected ? If not, it would be better to call it « pseudo-prognostic » as in Thouron et al. (2012). P9, there is a confusion between δ and s used previously. What is their difference?
- Concerning the comparison of different activation parametrizations, I remain convinced that it is mainly reduced to a sensitivity test to the CCN concentration as the activation spectra of Fig.A1 show. As authors do not want to change this test for users' need, it is important to insist more on the CCN concentration change. Users must be warned that the choice of the activation method changes significantly the CCN concentration.
- The conclusion needs to be revisited by replacing experiment names with physical terms, and by considering the sensitivity study to saturation scheme mainly for 2-moment schemes, which constitutes the main new result.
- Also there are a lot of mispelling errors. A careful reading by a native english speaker remains necessary.

More specifically :

1. p 2 l l6 : you can add a reference to the Meso-NH model : Lac et al., 2018 : Lac, C., J.-P. Chaboureau, et al., Overview of the Meso-NH model version 5.4 and its applications, *Geosci. Model Dev.*, *11*, 1929-1969, 2018.
2. p 2 l 14 : « focusing on the influence of drag effect ~~on~~ **and** droplet deposition »

3. p 2 l18 : Most of the 2-moment schemes used for fog consider radiative cooling as a term of the supersaturation equation. This remark is not relevant.
4. P 2 l 19 « in its **development and** mature stage »
5. P 2 l 28 : add Thouron et al. (2012) to Lebo et al. (2012). Therefore in the next sentence, you can shorten with : « Following these studies ... »
6. p 10 l 5 : do you use cyclic conditions ?
7. P 11 l1-4 : not clear. Please rephrase
8. For parts 4.2, 4.3 and 4.4 the numbering is not correct as you have only a single subpart : 4.2.1, 4.3.1, 4.4.1
9. P 12 l 5 : « In this section … » : necessary to add « with a 1-moment scheme in a LES »
10. P 12 l 16 : instead of Mazoyer et al. (2017) you can add the new reference : Mazoyer et al. (2019) just accepted which is an experimental study : https://www.atmos-chem-phys-discuss.net/acp-2018-875/
11. P 13 l 4 : « drops rapidly **in PRG and EXP** »
12. P 13 l 9 : I do not understand why differences of RH at 2m between SAT and (PRG,EXP) do not lead to differences on dissipation time at the ground.
13. Figure 5 is not nice and subfigures on the right are too small. Is it necessary to present the 3 hours for the right part with the budget ? Only 6 UTC would be sufficient as in Fig. 9.
14. P 14 l 7 : « mature phase **before sunrise**, and mature phase after sunrise »
15. P 14 l 9 : you cannot say that the differences between the runs are negligible as budgets discriminate 2 sets : SAT and (EXP,PRG)
16. p 15 l 13 : « differences for activation **in a 2-moment scheme** might be crucial » : that is why explanations are confusing and parts 4.2 and 4.4 must be merged.
17. P 16 l 15 : this result is not new. As a minimum add a reference as Boutle et al. (2018)
18. p 17 l 1 : where are the observed values ?
19. P 18 l 2 : « N2EXP suffers the most ... » : it is a negative assessment, but what is the reference ?
20. P 18 l 12 : where do you show temporal evolution of supersaturation ?
21. Part 4.4.1 : Is it the same time step for the coarser resolutions ? Otherwise the differences could be due to the impact of the time step instead of the impact of the resolution. If it is the same time step, it is necessary to specify it. If not, you have to run the coarser grids with the same time step (which will not cause instability problems).
22. P 20 l 19, P22 l 2 and P 23 l 1 : « microphysical parametrizations » is too vague and must be replaced by « supersaturation calculation »
23. P 21 : What's about the ratio between N2SAT and N2PRG according to the resolution ?
24. In the conclusion, you have to forget abbreviations N1EXP, N2EXP … and to explain the results in physical terms. Also when you discuss supersaturation calculation, you have to be clear between 1-moment and 2-moment microphysical scheme.
25. P 22 l 22 : add « in agreement with previous studies »

Misspelling : there are a lot of errors, the reading was not assiduous. Only a few ones are reported below.

- after a «:», you have to use a lowercase letter : in many parts of the text
- p1 l 2 : cycle
- p 3 l 8 : provide**s**
- p 3 l 18 : start**s**→**ed**
- p 12 l 6 : « **which** differs »
- p 20 l 20 : resolution**s**, remove one « the », « comparison with » ...