

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 : The manuscript presents a study of condensation and activation parameterizations for a LES of radiation 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. Therefore these questions of activation are central. The relevance of saturation adjustment for LES has been raised by Thouron et al. (2012) for stratocumulus and Lebo et al. (2012) for deep convective clouds. Since these studies, it is the first time that this question is dealing with fog. So this study could be an original contribution to the modelling community. But the study suffers from a lot of weaknesses and is not convincing. Therefore it misses the objective. Whilst the topic is interesting, and could be ultimately worthy of publication, I feel major modifications to the manuscript are required, and substantial inputs are necessary before publication.

My major concerns are :

- The case is an observed fog event, but you never show observation so there is no reference. Therefore you cannot say that liquid water content is overestimated in some configuration.
- You draw conclusions with only one case. For instance 6.9 % just corresponds to one case and you generalize this result to characterize the impact of adjustment saturation for fog (in the abstract/conclusion). In the same way, for the sensitivity of the time step, you claim that you test a larger time step without showing the result, and you state that the effect is not negligible. This is not scientific and admissible. A broad range of time steps needs to be compared. Additionally, what is the sensitivity to the spatial resolution ?
- The objective to evaluate the impact of saturation adjustment was promising but disappointing as you do not compare explicit vs saturation adjustment for 2 moment microphysical scheme, despite the fact that 2 moment microphysical schemes are the most frequently used in LES of fog. At least a N0 test with Twomey or Cohard and saturation adjustment needs to be added, to be compared to N1 or N2. Moreover a more complete study of this topic would include a pseudo-prognostic approach of supersaturation (Thouron et al., 2012).
- The comparison of different activation parameterizations (4.3) is reduced to a sensitivity test to the CCN concentration, and contributes nothing new. Why have you not chosen more equivalent activation properties, for instance if the 3 curves pass by the same point $S=0.1\%$ $N_{CCN}=100\text{ cm}^{-3}$ (Fig.A1) in order to compare the 3 parameterizations ? Because the 3 activation schemes present different curvatures according to S, and this point is not discussed.
- There are a lot of inaccuracies.

More specifically :

1. The introduction has been neglected and does not raise the scientific questions. The fact that most of LES of fogs produce an overestimation of cloud concentration and mass is one argument to justify this study (See Mazoyer et al., 2017).
2. p2 : Stolaki et al. (2015) used 1D simulations
3. p2 l 7 : What is Salsa ? Reference ?

4. p 2 l 11 : Mazoyer et al. (2017) needs to be added
5. p 2 l 20 : Thouron et al. (2012) is the first paper raising the question of how relevant the saturation adjustment is for LES of clouds. The paper draws extensively on Thouron et al. (2012) but it is not sufficiently referenced in different parts.
6. p2 l31 : What does revision 2675 mean ?
7. p 3 : some information about PALM is missing : What are the numerical schemes used ? Is the turbulence scheme 1D or 3D (does it parametrize horizontal turbulent fluxes) ? More important : what are the parametrizations for the computation of cloud optical properties ?
8. p 7 : The explicit supersaturation calculation corresponds to the scheme B in Thouron et al. (2012) (diagnostic of supersaturation). They have shown that this method is very sensitive to small errors in temperature and mixing ratio. Spurious values of supersaturation have a significant impact on CCN activation. They showed that it also overestimates CCN activation at the top. All this information should be recalled as well as the reference.
9. P7 line 15-17 is not clear. Could you improve the explanation if you want to justify that a pseudo-prognostic approach is not interesting or necessary.
10. Tab 1 and Part 4 : please add and analyze a new test N0 with Twomey or Cohard and saturation adjustment.
11. Fig 3 : you say « height averaged » and then 2m and 20m. So what ?
12. Fig.4 : do time marks refer to C1 or REF ?
13. P11 l 4 : why are the time steps in the plural ? Can you also explain shortly why they are so small ?
14. P 12 l 17 : it is C1 minus REF, isn't it ?
15. P12 l 21-22 : How are these higher liquid mixing ratios produced ?
16. P 12 l 27 : Again why is the time step approximated ?
17. P12 l 26-35 : This paragraph is not acceptable as you conclude on a sensitivity of the time step without showing any result.
18. P13 l 4 : what is the reference to say that liquid water is overestimated ? Why do not you use the observed value ?
19. Fig 7 : n_c is a 3D field. So is it a vertical and horizontal average, or is it for the first vertical level ?
20. P 14 l 21 : as it is the explicit method, why do you take care of maximum supersaturation ?
21. What is new from Fig. 9 and 10 ?
22. p 16 : Could you conclude that the radiation impact of n_c is more important than in the sedimentation process ?
23. Fig 9 : it would be better to put the total tendency in b than in c, as profiles are too intermingled in c.
24. Fig 10 : Deactivation means evaporation ?

Misspelling :

- p1 l 20 : aerosols
- p2 l 9 : as as
- p12 l 21 : diminishes
- p14 l 18 : is → are
- p 15 l 16 : shows