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

RC2: The manuscript presents a study of condensation and activation parametrizations 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.

Author's answer: First of all, we would like to thank the reviewer for the detailed and constructive feedback. Especially the high expert competence of the review allowed us to overcome the weaknesses, to extend the study by reasonable points and to focus the scientific result.

RC2: 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.
Author's answer: Indeed, the simulated fog case was an observed event during CESAR. A detailed comparison to measurements is given in Maronga and Bosveld, 2017. However, the relevant (in terms of our study needed) quantities, such as droplet number concentration, liquid water mixing ratios and liquid water path were not measured. In our statements, which claim an overestimation of certain configurations, we therefore refer to theoretical considerations.
However, we agree that in some passages that this was not clear enough or not sufficiently proved. Thus, we modified those passages and only make valuations were there are justified.
Modification(p2, l2): Rewrite passages, which claimed an overestimation and could not be sufficiently proved.

RC2: 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?
Author's answer: We agree with you general objection that drawing quantitative conclusions by two simulations is not admissible. Due to that we removed the passages were we generalized our results. Further, we labeled our results as that was they are: Findings from high-resolved LES study with (typical) continental aerosol conditions. From that we can only conclude that similar cases might show a similar trend but may differ in their concrete numbers. Moreover, as you suggested in the next comment we added a prognostic approach for simulating supersaturation. Due to carefully double checking we noted a bug in our model code and must repeat one simulation (old C1, renamed in EXP in the revised manuscript). Accordingly, the quantitative results changed but the general qualitative findings remain untouched.
Furthermore, we removed the conclusion that differences getting smaller by a larger time-step, as it was not sufficiently proved and mixed with a comparison to simulations with a different grid-

spacing. Therefore. the effect was not isolated to the time step.
Due to computational costs (one simulation requires approximately 48h on 3072 cores on a supercomputer) a broad range of time steps could not be conducted with this setup, since only (due to dynamical stability reasons) a reduction of the time-step is allowed.
However, we added a sensitivity study with a spatial resolution of 4m and 2m in chapter 4.4 as there changes to the grid-spacing should have the strongest effect.
Modification(chapter 4.2 and 4.4): As this referee comment involve major modifications we kindly refer to the revised manuscript and the attached manuscript, which highlights all changes in comparison to the first version individually.

RC2: 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).
Author's answer: We decided to follow the reviewer suggestion and added three more high-resolved simulations.
Firstly, we extended the part of the study where the influence of different condensational growth parameterizations are isolated and investigated (in terms of using a 1D-microphysics with fixed number concentration). Here, we also added a prognostic approach for calculating the supersaturation, which drives the strength of the diffusional growth.
Secondly, as the reviewer proposed we added the saturation adjustment case with a activation scheme of Cohard et al., 1998. Moreover, we also applied the same activation scheme by using the prognostic approach for calculating supersaturation. By doing so, we introduced (following Thouron et al., 2012.) a new section where the influence and feedback of different supersaturation calculation on the droplet activation (by using the scheme of Cohard et al., 1998) is discussed.
For that we compared N2EXP to the new simulations N2SAT and N2PRG. The new introduced simulations are summarized in the Table (bold marked). Note, that though these major modifications we decided to rename the simulation to make it more intuitively.

#	Simulation	Activation Scheme	Nc	Na	Condensation Scheme
1	SAT	None	150	none	Saturation adjustment
2	EXP	None	150	none	explicit
3	**PRG**	**None**	**150**	**none**	**prognostic**
4	N1EXP	Twomey	Not fixed	842	explicit
5	N2EXP	Cohard et al	Not fixed	842	explicit
6	N3EXP	Khvorostyanov and Curry (2006)	Not fixed	842	explicit
7	**N2SAT**	**Cohard et al.**	**Not fixed**	**842**	**Saturation adjustment**
8	**N2PRG**	**Cohard et al.**	**Not fixed**	**842**	**Prognostic**

Modification(chapter 2.2.2 and chapter 4.4): [..] As this referee comment involve major modifications we kindly refer to the revised manuscript and the attached manuscript, which highlights all changes in comparison to the first version individually

RC2: The comparison of different activation parametrizations (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 % NCCN=100 cm-3 (Fig.A1) in order to compare the 3 parametrizations ? Because the 3 activation schemes present different curvatures according to S, and this point is not discussed.
Author's answer: Our idea here was to show the differences between different activation schemes initializing in such a way (by using the in literature described values, see Cohard et al., 1998, Khvorostyanov and Curry, 2006 and Pruppacher et al., 1998 (chapter 2.2)) that they are describing the same aerosol environment. So basically we didn't change the aerosol concentration, since we leaved this parameter untouched.
However, considering the activation spectra displayed in A1 we agree that there is mainly a offset between the schemes of Cohard et al., 1998 and Khvorostyanov and Curry, 2006. In contrast to that to the Twomey-scheme we see both, an offset as well as a different curvature. As you suggest we could modify the activation spectrum (or more precisely the parameter describing the aerosol environment) in a such a way that they pass by the same point. But again, the overall goal was more from the view of a users, which is maybe interested in which total differences can be produced by using this or that scheme. As this get not clear enough in the first version of the manuscript, we modified the revised version accordingly.
Modification(Chapter 1): [..]

RC2: There are a lot of inaccuracies. More specifically:

RC2(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).
Author's answer: After major modifications also the introduction was carefully revised. Furthermore, the study of Mazoyer et al., 2017 was added. But more important, the missing scientific question was clarified.
Modification(Chapter 1): [..] As Mazoyer et al. (2017) and Boutle et al. (2018) stated that both, LES and NWP models tend to overestimate the liquid water content and the droplet number concentration for radiation fog the following questions are derived from these shortcomings:
(i) Is saturation adjustment appropriate as it crucially violates the assumption of equilibrium? How large is the effect of different supersaturation calculations on diffusional growth?

(ii) What is the impact of different activation schemes on the fog life cycle for a given aerosol environment?

(iii) As the number of activated droplets is essentially determined by the supersaturation, how large is the effect of different supersaturation modeling approaches on aerosol activation and therewith on the strength and life cycle of radiation fog (cf. Thouron et al., 2012)?

In the present paper we will try to answer these questions employing high-resolution LESs based on an observed typical deep fog event with continental aerosol conditions. The paper is organized as follows: Section 2 outlines the methods used, that is the LES modeling framework and the microphysics parameterizations used. Section 3 provide an overview of the simulated cases and model setup, while results are presented in Section 4. Conclusions are given in Section 5.

RC2(2): p2 : Stolaki et al. (2015) used 1D simulations
Author's answer: This is right. It is corrected in the revised revision.
Modification(p2, l2): [..] while using the one-dimensional mode of the MESO-NH model [..]

RC2(3): p2 l 7 : What is Salsa? Reference?
Author's answer: That was right, an complete reference was missing. SALSA is a sectional module for a size resolved treating for aerosols.
Modification(p2, l7): [..] Sectional Aerosol module for Large Scale Applications (SALSA) (Kokkola et al.,2008) [..]

RC2(4): p 2 l 11 : Mazoyer et al. (2017) needs to be added
Author's answer: Added in the revised manuscript.
Modification(p2, l8): [..]Mazoyer et al., 2017 conducted similar to Stolaki et al., 2015 simulation of the ParisFog with the MESO-NH model but using the 3D-LES mode, and focusing on the influence of drag effect on droplet deposition

RC2(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.
Author's answer: We agree, and have added this reference in the missing passages.
Modification(p?, l?): e.g. p2, l30, p7,l3, p8, l6

RC2(6): p2 l31 : What does revision 2675 mean?
Author's answer: Our LES-model PALM is maintained with the trac-system. Due to that every change in the model code or corresponding files is explicitly identified with the revision number. With that number it is also possible to get the for this studies used model code from our web page, which is mentioned in the acknowledgments.
Modification(p2, l31): None**.**

RC2:(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?
Author's answer: We have added the missing information about PALM and how optical properties of clouds and how they are treated in the radiation model. By doing so we were as short as possible to avoid to lengthen the manuscript, but more important as precise as necessary.
Modification(p3-4): [..] PALM is discretized in space using finite differences on a Cartesian grid. For the non resolved eddies a 1.5-order flux-gradient subgrid closure scheme after Deardorff (1980) is applied, which includes the solution of an additional prognostic equation for the subgrid-scale TKE. Moreover, the discretization for space and time is done by a fifth-order advection scheme after Wicker and Skamarock (2002) and a third-order Runge-Kutta time-step scheme (Williamson, 1980), respectively. The interested reader is referred to Maronga et al., 2015 for a detailed description of the PALM model.

[..]This favors an improved calculation of the effective radius, which is calculated by

$$r_{eff} = \frac{3 q_l \rho}{4 \pi n_c \rho_l}^{1/3} \exp(\log(\sigma_g)^2),$$

where q_l is the liquid water mixing ratio, rho the air density, ρ_l being density of water and σ_g=1.3 the geometric standard deviation of the droplet distribution. The effective radius is the main interface between the optical properties of the cloud and the radiation model RRTMG. Note, that 3D radiation effects of the cloud are not implemented in this approach, which however could affect the lateral edges.

RC2(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.
Author's answer: We agree, and added a prognostic approach for treating supersaturation to our work. This includes a new chapter discussing the effect of different methods for supersaturation calculation on CCN activation.
Modification(chapter 2.2.2 and chapter 4.4): [..] As this referee comment involve major modifications we kindly refer to the revised manuscript and the attached manuscript, which highlights all changes in comparison to the first version individually.

RC2(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.
Author's answer: Our primary reasons for not using a prognostic approach for solving the supersaturation was that a small grid spacing is the method of choice to migitate the error introduced by spurious cloud edge supersaturations (e.g. Hoffmann, 2016). As we already used this lowest feasible grid-spacing for simulating such a case (simulating this fog event with Δ=1m occupies 3072 processor units for approximately 48h on a supercomputer).
However, since spurious supersaturations also occur for small grid spacing's since it is more a question of the ratio of advection and condensational phase relaxation time scales we decided to implement and test this method in our model and include the results within this manuscript.
Modification(chapter 2.2.2 and chapter 4.4): [..] As this referee comment involve major modifications we kindly refer to the revised manuscript and the attached manuscript, which highlights all changes in comparison to the first version individually.

RC2(10): Tab 1 and Part 4 : please add and analyze a new test N0 with Twomey or Cohard and saturation adjustment.
Author's answer: We added and analyzed a case with saturation adjustment and the activation scheme of Cohard et al., 1998. Moreover, we also added a case using the prognostic approach by using the same activation scheme. This involves a new chapter, describing the feedback of different supersaturation calculation methods on droplet activation similar to Thouron et al, 2012.
Modification(chapter 2.2.2 and chapter 4.4): [..] As this referee comment involve major modifications we kindly refer to the revised manuscript and the attached manuscript, which highlights all changes in comparison to the first version individually.

RC2(11): Fig 3 : you say « height averaged » and then 2m and 20m. So what?
Author's answer: We agree that this description was wrong. It is a horizontal average at different heights.
Modification(Fig. 3): Time series of horizontal [..]

RC2(12): Fig.4 : do time marks refer to C1 or REF?
Author's answer: Due to major modification's of the manuscript this passages is removed.
Modification(p?, l?): [..]

RC2(13):P11 l 4 : why are the time steps in the plural? Can you also explain shortly why they are so small?
Author's answer: The revised version uses the singular. During the time integration the time step is calculated dynamically. For calculating the length of the new time step our model consider the CFL-criterion (Courant et al., 1928) as well as the diffusion-criterion (e.g. Jacobson, 2005, chap 6.4.4.1)

and afterward takes the minimum of both. Both of them led to a decreased time step by decreasing grid spacing and increasing wind speed. In our cases the grid spacing is relatively small with some moderate wind speed. We had to use a case where the wind speed is strong enough to generate turbulence, otherwise our LES were not able to simulate such a case, which then can favorably be done by DNS.
Modification(p?, l?): [..] time step [..]

RC2(14):P 12 l 17 : it is C1 minus REF, isn't it?
Author's answer: Yes, it is. However, due to major modifications part is removed from the revised manuscript.

RC2(15):P12 l 21-22 : How are these higher liquid mixing ratios produced?
Author's answer: This is explained by smaller evaporation rates in the case of C1. Due to that the case C1 exhibits in higher levels during the lifting phase of the fog slightly larger values for the liquid water mixing ratio, as evaporation is the dominant process.
Modification(p?, l?): [..]as evaporation is the dominant process during the dissipation phase.

RC2(16):P 12 l 27 : Again why is the time step approximated?
Author's answer: Again, the time step is not fixed. Instead it is calculated new at every time step. Therefore, there is no constant value during one simulation, instead if it is set manually. The latter should only be done if one is sure that the aforementioned criterion are not violated by the manual set time step. But I agree that 'approximately' is the wrong term to describe a well known value. Instead I calculated the average time step of a 4m simulation which was 0.58 s.
Modification(p12, l7): [..] on average 0.58 [..]

RC2(17):P12 l 26-35 : This paragraph is not acceptable as you conclude on a sensitivity of the time step without showing any result.
Author's answer: We removed this paragraph from the manuscript. However, this issue is discussed in more detail by answering the second Referee Comment, what we gladly refer to.
Modification(p12 l 26-35): [..]Removed this section.

RC2(18):P13 l 4 : what is the reference to say that liquid water is overestimated ? Why do not you use the observed value?
Author's answer: There is no observed value for this fog event. Our assumptions that the value of the saturation adjustment is overestimated is based on theoretically consideration and on literature found information that conditions for applying saturation adjustment are violated here. However, since this is no evidence for an overestimation in comparison to the real value we replaced this phrase by "higher".
Modification(p13, l4): [..] higher in the case of saturation adjustment.

RC2(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?
Author's answer: It is a horizontal and vertical average for the whole fog layer. Corrected in the revised version.
Modification(Fig. 7): [..] (as a horizontal and vertical average of the fog layer) [..]

RC2(20):P 14 l 21 : as it is the explicit method, why do you take care of maximum supersaturation?

Author's answer: We revised this passage as we must admit that it was confusing to speak about maximum supersaturation for the explicit method, which is commonly used for activation parameterization in case of saturation adjustment. Our aim here was to show that we were able to reproduce typical observed values for the supersaturation. However, for that we do not need to refer to the maximum value. Mainly, those observed values are measured at a height of 2m. Accordingly, in the revised manuscript we connect the observed values with the shown values of simulation in 2m.
Modification(p?, l?): [..] while in case EXP and PRG average supersaturation of 0.05% in 2 m occur, which corresponds to typical within fog.

RC2(21): What is new from Fig. 9 and 10?
Author's answer: In Figure 9 and 10 the microphysical tendiencies are discussed in detail. In contrast to Fig. 5 they consider a full two-moment microphysics scheme, i.e. that also the droplet number concentration is altered. Due to that it could exemplary shown what processes and how strong certain processes influence the

RC2(22):p 16 : Could you conclude that the radiation impact of n_c is more important than in the sedimentation process ?
Author's answer: This is in interesting objection. Since, we focused here on the impact of microphysical parametrization (and the effect of the radiative impact of n_c is considered within the radiation model) we have not done studies yet to quantify the feedback to e.g. radiative cooling. To isolate this processes (since there is a feedback mechanism: radiative cooling produces higher supersaturation → leading to more activated droplets → leading to an decreased average radius (sine the surplus water vapor is distributed on more droplets) → slower sedimentation and → causes stronger radiative cooling, since the effective radius is decreased → leading to new (maybe stronger) supersaturation) more studies must be conducted to answer this question appropriately. Moreover, for the sedimentation process a similar feedback mechanism is involved. which might be shortly outlined as: if the number of droplets decrease due to sedimentation → the water vapor surplus is distributed on less droplets → leading to higher average radius → lesser optical thickness and → stronger sedimentation.
To get an quantitative idea which of those processes is more important determining the life cycle of the fog would include two more simulation in which the number concentration is kept constant on the one hand for the radiation effect and on the other hand for the sedimentation process.
Modification: None.

RC2(23): Fig 9 : it would be better to put the total tendency in b than in c, as profiles are too intermingled in c.
Author's answer: We agreed and modified the figures as we put the total tendency in an own plot.
Modification(Fig. 9 & 10) [..] Modified Fig. 9 and Fig. 10.

RC2(24): Fig 10 : Deactivation means evaporation?
Author's answer: Yes, it does. Due to reasons of consistency it is adapted to equation 2.
Modification(FIG10): [..] deactivation → 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

All misspellings are corrected in the revised version.