Responses to the Reviewer 1 comments. Original comments in italics, our responses in regular font.

General comments:

The manuscript presents a new approach for the treatment of aerosol activation in Lagrangian Cloud Models (LCMs), a novel and promising approach for the simulation of cloud microphysics based on individually simulated super-droplets (SDs). The novelty (and advantage) of the new activation approach is that SDs are only introduced if the supersaturation exceeds a certain threshold. This is fundamentally different from previous activation approaches, in which SDs needed to be simulated even before activation. As pointed out in the manuscript, this new approach is not suited for simulating cloud- processing of aerosols. Applications in which cloud-processing of aerosols is of minor interest, however, benefit from reduced computing time as well as a smaller memory demand. Furthermore, the authors introduce further improvements necessary for the correct determination of supersaturations in LCMs: a velocity interpolation scheme which conserves the incompressibility of the flow as well as a technique to cope with spurious supersaturations. Since these latter refinements do not require the proposed aerosol activation scheme, they are a recommendable addition to all current LCMs.
All in all, this manuscript is well written, presents novel and useful methods, and is of interest to the entire LCM community. Accordingly, I recommend publishing this paper in Geoscientific Model Development. However, I would like the authors to address some minor comments, which will only increase the value of this already nice manuscript.

We greatly appreciate the Reviewer's positive comments. We believe addressing the minor comments listed below resulted in a significantly improved presentation. The Reviewer's effort is greatly appreciated.

Minor Comments

• p. 2, l. 25: Please define "multiplicity". It might be understandable but there are also synonyms used in the literature (e.g., weighting factor).

We added a sentence with the definition.

• p. 4, Eq. (4): There is a "+" missing between "ri" and "r0".

Yes, we are sorry. That happened when we moved the text from a Word document we had been using when drafting the manuscript into the latex file. The error has been corrected.

• sec. 2.3: Although the Twomey activation approach is new to warm-cloud LCMs, there is already an analog in ice-cloud LCMs. Sölch and Kärcher (2010) describe how they introduce new SIPs (simulation ice particles – the ice-cloud equivalent to SDs) to the model domain based on an underlying nucleation scheme, which exhibits many similarities to Twomey activation. Additionally, Unterstrasser and Sölch (2014) describe how a stochastic representation of that nucleation scheme can improve the model's statistics. I think these publications should be mentioned and discussed in the manuscript.

Yes, we were aware of the similarity between ice initiation and Twomey activation of cloud droplets, but failed to point this out. This is corrected in the revised manuscript by bringing the Soelch and Kaercher reference. We do not feel that referring to the other paper is needed.

• p. 6, ll. 11 – 12: Is the sentence "Without ... past." true if entrainment/mixing is considered? The diluted number mixing ratio cannot reveal the previous maximum supersaturation. (Although the Twomey activation scheme will still be applicable.)

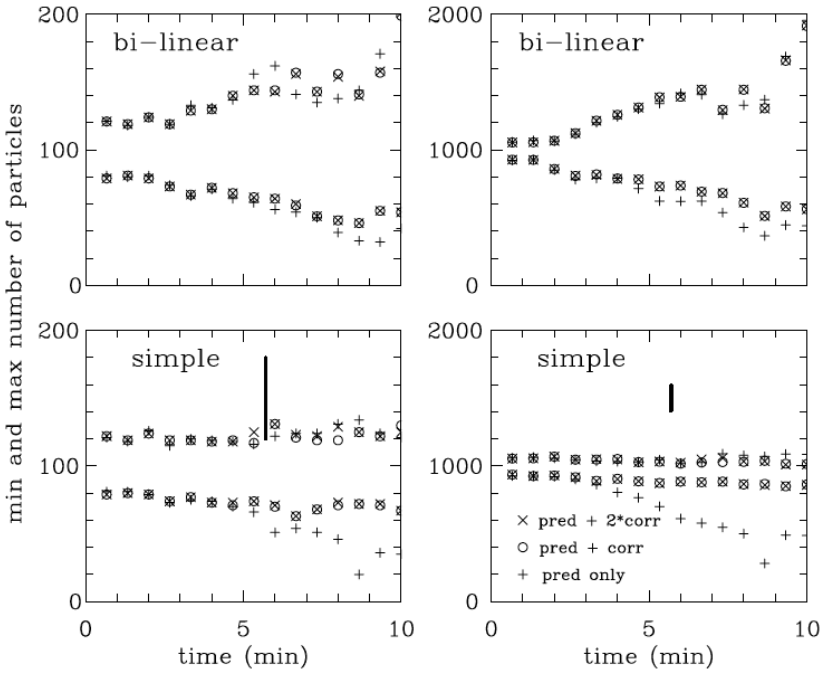
We believe the sentence in question is correct but it requires additional explanation. The diluted number mixing ratio represents the history of the supersaturation because it reflects a combination of past supersaturations in the two volumes: the undiluted cloudy volume and the cloud-free volume with un-activated aerosol. That said, we expect that details of a possible additional activation after an entrainment event might differ between the explicit activation scheme in the SD model and when the Twomey approach is used. This is especially true when CCN varies in the vertical. The same problem occurs in the Eulerian bin microphysics. We decided not to include the above discussion in the manuscript and we left the original text unmodified.

• p. 7, ll. 12 – 14: I agree with the sentence "This is ... to another". However, the same multiplicity for all SDs might be disadvantageous for the initiation of collision and coalescence (see Unterstrasser et al., 2017).

Yes, this is correct. We added a comment on that.

• sec. 2.4: The suggested interpolation scheme should be used in all LCMs. However, there is one suggestion: Please add two plots to Fig. 4, which show the results for 100 SDs per grid cell, which is the typically applied SD concentration in current LCM simulations. This plot will be of great value to judge if there is a big impact of thoughtlessly applied tri-linear velocity interpolation in the published literature.
(Although I assume that there will be no impact visible due to the LCM's inherent fluctuations (now with a much higher standard deviation of around 10 %).)

To address this point, we replaced the figure for the test with 1,000 SDs with a figure showing results for both 1,000 and 100 SDs. The new figure is included below. The discussion of the figure was modified, but the key point remains unchanged: bi-linear (or tri-linear in 3D) interpolation leads to unphysical concentration fluctuations when compared to the simple scheme.



• p. 10, l. 29: Why is the calculation limited to cloudy regions (ql > 0.01 g kg-1)? Shouldn't the results be independent of the region within the model domain?

This is just a small technicality. Passive particles were included only in part of the domain (as mentioned in line 20/21 of the original submission) to reduce the computational cost. If the entire domain is included in the calculation of the extrema, then the minimum would be zero, correct? Thus, we only use "cloudy points" defined using the specified threshold. We do not feel this needs to be dwelled upon in the text.

• p. 14, ll. 19 – 21: Please give more details (or a reference) how the water condensation is split into 10 substeps. Based on the given text, I cannot imagine how this procedure is applied.

Details of the sub-stepping in the UWLCM model are discussed in Arabas et al. (GMD 2015). We modified the sentence that mentions the sub-stepping (motivated also per Rev. 2 comment) and refer to Arabas et al. paper.

• sec. 3.2: Please add some details on the number of SDs initialized in each grid box or the maximum number of SDs per grid box created by the Twomey activation scheme. These details follow later (p. 17, l. 6) but I expected them to be in the setup section.

The text (with some modifications) was moved from page 17 to section 3.2. Also, per Rev. 2 request, we added information on the activation in the UWLCM as only part of SDs in the traditional approach become cloud droplets.

• Fig. 9/10: How do you define activated particles? Using the Twomey activation scheme, this is a straightforward task. But how do you proceed in the UWLCM?

Activated droplets in UWLCM are defined as those that have radius larger than the activation radius. This has been added to the text.

• *Fig. 9/10: Could you please comment a little more on the strong oscillations in the σ plots? It seems that those time series jump between two solutions.*

The oscillations are related to statistical fluctuations due to finite number of SDs as the amplitude is reduced roughly in proportion to the square root of SD number. It is important to note that the center of mass is calculated on the Eulerian grid, that is, it jumps from one grid box to another as the thermal moves upwards. The period of these oscillations in Figs. 9 and 10 (about 10 sec) matches the propagation of the center of mass over the grid (updraft velocity of about 2 m/s and grid length of 20 m). A comment on the has been added to the revised text.

• *p. 21, ll. 8 – 14: I agree that using the Twomey activation scheme will reduce the number of SDs in the model domain and, hence, computing time and memory demand. However, these considerations deserve some more thoughts. Models are usually parallelized using a 2D domain decomposition applied to the Eulerian fields but also the SDs. Accordingly, if there is a cloud in just in one subdomain, massive load imbalance will occur slowing down the whole computation. To benefit most of the new Twomey activation scheme, new parallelization strategies for the treatment of SDs need to be developed, e.g., a uniform distribution of SDs over all cores independent of their physical location in the model domain to avoid load imbalance issues.*

This is a valid point. However, the domain decomposition in physical space is used as a parallelization strategy in finite-difference models only. A parallelization for the Lagrangian thermodynamics should be developed outside of the domain decomposition and then load imbalances would not be an issue. We added a comment on that to the manuscript.

We addressed all technical comments through appropriate modifications of the text.