Review of "Predicting the morphology of ice particles in deep convection using the super-droplet method: development and evaluation of SCALE-SDM 0.2.5-2.2.0/2.2.1" by Shima et al. (gmd-2019-294)

The manuscript describes the foundation and implementation of a new mixed-phase cloud model based on the superdroplet approach, in which individual Lagrangian particles represent ensembles of real aerosols and hydrometeors (e.g., cloud droplets, ice crystals, graupel). In first applications, dimension-mass and dimension-velocity are compared to literature values, and the sensitivity of the model to three numerical parameters (number of Lagrangian particles, time step, grid spacing) is tested.

First of all, I like to highlight the author's work on the formulation and implementation of their model, which is the first fully Lagrangian mixed-phase model in the literature. Furthermore, I like to emphasize their thorough model description. However, this thoroughness makes the manuscript lengthy, and in some places even repetitive. Therefore, the manuscript feels more like a reference work than a scientific article. Accordingly, besides requesting some rewriting for tightening the manuscript, my only major concern is the lack of comparisons with other mixed-phase models or observational data as outlined below. After my concerns are addressed, I can fully support the manuscript's publication in Geoscientific Model Development.

Major Comments

Comparison with other models or observational data: While the comparison of dimension-mass and dimension-velocity with literature values is already a first step in the right direction, I miss a thorough comparison with other modeling approaches or observational data. Mixed-phase microphysics is a highly complex subject due to a large number of (partially unknown) processes and their (highly uncertain) representation. Comparing different models and integrating observations that include information of particle habits is therefore essential to confirm the applicability of this new approach, and to identify missing or inappropriately represented processes. However, this is probably not within the scope of the study but will be a mandatory part of the further development and institutionalization of this modeling approach.

Combining sections for tightening the manuscript: The structure of the manuscript is very clear, with dedicated sections for the description of the model equations (Sec. 4), their numerical implementation (Sec. 5), and potential changes and additions to these equations (Sec. 9.2). Similarly, modeling results are described in Secs. 7 and 8 and discussed in Secs. 9.1 and 9.3, respectively. While this (more traditional) separation has been used for a long time in scientific writing, more recent publications tend to combine these sections, especially results and their interpretation/discussion, which might increase readability and understanding.

While reading Sec. 4.1, which describes the basic microphysical processes and equations used in the model, I was missing a more rigorous discussion of the choices made, i.e., which processes are included, which are neglected, and the reason for this. Of course, these points are not missing, but they are primarily stated in Sec. 9.2 — 29 pages later. Therefore, I suggest combining Sec. 4.1 with Sec. 9.2.

Similarly, the results of Sec. 7 are discussed in Sec. 9.1, and the results of Sec. 8 are discussed in Sec. 9.3. Again with a large gap that interrupts individual lines of thought. Therefore, I suggest combining Sec. 7 with Sec. 9.1 and Sec. 8 with Sec. 9.3 to increase readability.

Note that I am not sure if a combination of the aforementioned sections can be successful. However, I feel that some tightening of the manuscript might help the reader to grasp the main ideas of the manuscript.

Minor Comments

P. 1, ll. 7 – 8: The manuscript does not really show that the results capture the characteristics of a real cumulonimbus since it lacks a comparison with observations. This statement is also made on p. 7, l. 2, p. 64, l. 23.

P. 4, l. 20: The truth (despite some rare analytical solutions) is usually not even known.

P. 4, ll. 25 – 28: This problem has been described nicely in a paper by Stevens and Lenschow (2001), which I suggest to cite.

P. 5, l. 3: I would write about "a macroscopic description of cloud microphysics" to avoid confusion with other macroscopic properties of clouds, e.g., cloud morphology.

P. 5, ll. 5 – 6: I do not agree that "bulk models do not have a rigorous theoretical foundation". Their theory — although it is based on approximate or idealized droplet size distributions — condenses a lot of our knowledge on microphysical processes.

P. 5, l. 11: I recommend to state explicitly that "aspect ratio" refers to the ratio of the ice crystal axes.

P. 5, ll. 13 – 16: Diffusional growth, spontaneous breakup, and collisional breakup are also an inherent parts of liquid-phase microphysics.

P. 5, ll. 23 – 25: If the development of bin and bulk models started in the 1950s, why do you only cite articles from ≥ 2015?

P. 6, ll. 6 – 8: "[C]urse of dimensionality" needs to be explained in more detail or left out.

P. 6, l. 34 – p. 7, l. 1: You do not resolve fluid dynamics "fully explicitly" since you have a grid spacing that is much larger than the Kolmogorov lengthscale.

P. 7, ll. 1 – 2: You should address that two-dimensional turbulence is different from three-dimensional turbulence.

P. 7, ll. 8 – 9: A particle attribute is added to a bin model?

P. 7, ll. 23 – 26: This paragraph feels like a repetition of p. 7, ll. 1 – 4.

P. 8, ll. 3 – 7: It feels arbitrary that the particle position $\boldsymbol{x}$ and the attributes $\boldsymbol{a}$ are treated separately.

P. 8, ll. 21 – 22: These lines feel like they belong to Section 5.

P. 9, l. 1: Where does this equation come from? Is there a reference?

P. 12, ll. 9 – 13: How do the same $(\beta, s) = (2.22, 1.32)$ result in different $\kappa = 0.375$ and 0.300?

P. 12, l. 19: While condensation freezing requires that the ambient water vapor is supersaturated with respect to liquid water, this is not necessary for immersion freezing.

Eq. (9): This equation is an approximation.

P. 14, ll. 11 – 12: Where does the approximation $C \approx (2a_i + c_i)/3$ come from?

P. 16, ll. 14 – 16, Eq. (29): The text and the equation do not agree. While the text states that the rime mass fraction does not change during sublimation, the equation states that it does not change during deposition.

P. 16, ll. 25 – 26: Dziekan and Pawlowska (2017) wrote about this. I suggest citing them here.

P. 17, ll. 8 – 9: I strongly believe that your collision-coalescence representation also captures selfcollection, i.e., the collision and coalescence of two raindrops.

P. 17, ll. 10 – 15: The collection efficiency is usually the product of the collision efficiency and the coalescence efficiency. The effects described here are only a part of the processes constituting the collision efficiency, e.g., I miss a discussion of the so-called wake effect that increases the collision efficiency of large droplets due to a reverse flow in a large droplet's wake. Moreover, by not explicitly considering the coalescence efficiency, it is assumed to be unity. However, it can be significantly smaller than unity, reflecting that smaller droplets, although they collide with a larger droplet, might not coalesce, i.e., they may just bounce off. I suggest commenting on this simplification.

P. 17, ll. 26 – 27: To what does “latter case” refer to in the last sentence?

Eq. (55): I believe T_s is not defined.

P. 24, ll. 21 – 23: How do we know that these degrees of freedom are unnecessary? This is a highly interesting question. However, simulating a small number of superparticles instead of all real particles is usually a result of limited computing resources, and not a deliberate decision on how many degrees of freedom are necessary.

P. 25, ll. 1 – 6, Eq. (82): This is a helpful equation and an interesting switch of perspective on superparticles, multiplicity, and probability. Intuitively, I might agree that both lines of the equation are equal. However, I am wondering if it is possible to derive the second line from the first in a mathematically exact way.

P. 28, ll. 6 – 7: Equation (7) is stiff even without the curvature and solute terms.

P. 28, ll. 15 – 17: It might be necessary to state that the applied collision-coalescence algorithm is linearized, which circumvents the quadratic nature of the collision-coalescence process.

P. 28, l. 31: This has already been said on p. 26, ll. 17 – 18.

P. 31, ll. 15 – 16: Does this boundary condition result in a significant loss of (almost) weightless aerosols? Or does is only affect precipitating particles?

P. 31, ll. 17 – 23: This heating is not applied to the surface but the air above. Does this heating start at the beginning of the simulation? The timing of the heating might have an important impact on the degree of turbulence in the simulation.

P. 32, ll. 1 – 3: The vertical grid spacing is quite large. It is well known that a too large vertical grid spacing reduces the maximum supersaturation at cloud base (e.g., Morrison and Grabowski 2008). This, in turn, affects the activation of cloud droplets, leading to a smaller number of cloud droplets. Do you see a substantial change in the number of cloud droplets in the sensitivity studies conducted in Section 8.2?

P. 32, Eqs. (91): I am struggling to derive (91) from (84) and (86). Please add some more comments. The same applies to Eqs. (92) and (93).

Pp. 33 – 34: You state that “a 10-member ensemble of simulations” is calculated “by changing the pseudo-random number sequence” in on p. 33, l. 9. This statement is repeated four times on p. 33, l. 18, p. 33, l. 29, p. 34, l. 11, and p. 34, l. 13.

P. 34, ll. 1 – 11: The tested microphysical timesteps are quite small compared to the literature that states timesteps that are typically between 1 and 10 s (especially in box calculations). Therefore, it might be worthwhile to add one or two additional simulations with even longer timesteps to explore the entire parameter space (e.g., DTx4 and DTx8).

Fig. 2: The caption is missing. I assume that this is a formatting error.

P. 43, ll. 11 – 13, Sec. 9.1: Could some of the unrealistic hailstones and graupel particles be explained by the lack of an appropriate shedding formulation that would break these particles after some timesteps?

P. 44, ll. 8 – 9: I disagree. Figure 9 shows that a higher number of superparticles increases the fluctuation/standard deviation in precipitation.

P. 63, ll. 27 – 28: The typical citation for the phase relaxation timescale is Squires (1952).

P. 64, l. 8 – 10: The general estimate of computational costs and its comparison with a Eulerian bin model is legit. However, I feel uncomfortable with the estimate of the collision calculation. The collision calculation is a quadratic problem in both Eulerian bin and Lagrangian schemes. The linearization of this problem applied in the Lagrangian cloud model might also have its caveats, and I believe that a similar implementation into a Eulerian bin model is possible. Accordingly, the last step from $10^5 - 100^5$ to $10^{10} - 100^{10}$ feels unjustified.

Technical Comments

P. 12, l. 23: I assume that “initiated” is better than “updated” here.

P. 13, l. 14: Use a lower case “w” to start this line.

P. 24, l. 4: Remove one “planets”.

P. 27, ll. 3 – 4: I believe it is a “predictor-corrector scheme” and not a “predictor-collector scheme”.

P. 35, l. 16: Replace “amount” with “number”.

P. 38, l. 3, l. 6: Replace “segments” with “slopes”.

References

Stevens, B. and Lenschow, D.H., 2001. Observations, experiments, and large eddy simulation. *Bulletin of the American Meteorological Society*, *82*(2), pp. 283-294.

Dziekan, P. and Pawlowska, H., 2017. Stochastic coalescence in Lagrangian cloud microphysics. *Atmospheric Chemistry and Physics*, *17*(22), pp. 13509-13520.

Morrison, H. and Grabowski, W.W., 2008. Modeling supersaturation and subgrid-scale mixing with two-moment bulk warm microphysics. *Journal of the Atmospheric Sciences*, *65*(3), pp. 792-812.

Squires, P., 1952. The growth of cloud drops by condensation. I. General characteristics. *Australian Journal of Chemistry*, 5(1), pp. 59-86