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Interactive comment on "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* Shin-ichiro Shima et al.

Anonymous Referee #2

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The manuscript describes a new Lagrangian particle-based microphysics model, its numerical implementation and first results for a 2D convective cloud. To my knowledge this is the first (fairly) complete implementation of the super-particle approach for mixed-phase clouds. In that sense the contribution is highly appreciated and is potentially a landmark paper for the cloud modeling community. The manuscript is impressive in many ways. First of all, because the model succeeds to predict the typical mass-dimension and velocity-dimension relationships of ice particles in the atmosphere based on physically reasonable and fairly simple assumptions. Second, it

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proves that the Lagrangian particle approach is computationally feasible and affordable for mixed-phase convective clouds. Third, it outlines the necessary future development and research to make further progress in understanding the properties of ice particles in mixed-phase clouds.

The manuscript provides a detailed and clear model description of the microphysical assumptions and equations and their implementation. This is very valuable for the community and in the best spirit of GMD that models should be well documented and their assumptions and limitations should be openly discussed. Even more, also the model development process is documented in the sense that the step from version 2.2.0 to 2.2.1 is described in the manuscript. This is very enlightening, because the base version 2.2.0 has some quite noticeable deficiencies, which are removed in version 2.2.1 by making some fairly small adjustments and fixes to the microphysical equations. I think it should be highly appreciated that the authors present their work in this way instead of simply desribing the newest version 2.2.1.

As a results the manuscript is a mix of a model documentation, a progress report and a research plan. Hence, it lacks the conciseness and structure that one would expect from a more focused scientific paper. In my opinion this is not a problem at all, especially not in a journal like GMD. Therefore I would recommend to keep the structure pretty much as it is now and make only some minor changes to improve readability.

Major comments:

- I was quite confused when reading section 2 from 2.1 to 2.7, because I did not understand how you can ensure mass conservation with this set of prognostic attributes. It only became clear when I read section 2.8 and understood that there are no partially melted wet ice particles (yet) in the model. I would strongly recommend to move that statement from section 2.8 to section 2.1 that particles are either liquid (and fully described by radius r) or ice (and described by major and minor axes a and c and the density ρ^i).

- From Figure 8 and 19 I would conclude that snow (aggregates) is falling too fast in SCALE-SDM, i.e. the green data point to not coincide with the empirical relations for aggregates. Can you explain this bias in the fallspeed of snow? I think this should be discussed in the paper.

- Maybe related to that: Wouldn't it be more accurate to use an ellipse instead of the circumcircle for the area in Boehms formula (section 4.1.3, page 11, line 20)? Do you take into account the turbulence correction for large Reynolds numbers in Boehms equations? The latter is actually necessary to limit the fall speed of large aggregates and match the observed terminal fall speed of aggregates.

Minor comments:

- page 5, line 5-7: I agree that a rigorous theory for bulk models is still lacking, but it would nevertheless be appropriate to reference the review by Beheng (2010). This paper gives an overview of the steps that have been made towards such a theoretical foundation, at least for liquid clouds and rain.

Beheng, K.D. (2010). The Evolution of Raindrop Spectra: A Review of Microphysical Essentials. In Rainfall: State of the Science (eds F.Y. Testik and M. Gebremichael), Geophysical Monograph Series, Wiley.

- page 6, line 4: 'approximated by a histogram', here I would recommend to replace 'histogram' by 'finite volumes or finite differences'.

- page 5, line 8: 'breakdown of the Smoluchowski equation'. Not all readers might be familiar with the notion of the breakdown of the Smoluchowski equation. A reference other than Smoluchowski (1916) or an additional sentence would be helpful.

- page 9, section 2.7: It should be mentioned that the assumption that particles move at their terminal fall velocity is an approximation. In the framework of a Lagrangian particle model this can quite easily be improved by considering the adjustment towards the new terminal fall velocity, e.g., after a collision event (see e.g. Naumann and Seifert

2015).

- page 13, section 4.1.6: When I first read this paragraph I was surprised that the ventilation is missing and is not even mentioned. It would be good to mention this approximation already here and not only later in section 9.2.4.

- page 14, eq. (13): Why is the minimum mass m^i_{min} necessary in this equation? Is this because homogeneously frozen droplets may not contain any insoluble aerosol mass and then you would eventually have a super-droplet with zero mass? Does that m^i_{min}-particle not grow immediately when it is advected into cold, ice-supersaturated conditions and produce unrealistic ice? It does remember its freezing temperature, but it is already ice and would therefore grow immediately when the environment is supersaturated with respect to ice. I don't understand how this is implemented.

- page 15, eq. (21): Why is it necessary to impose this explicit limit to water saturation? If water droplets are present, then the supersaturation should be limited to due the rapid condensational growth. If no water droplets are present and no CCN can be activated, then the limit to water saturation might be unphysical.

- page 15 and 16: For depositional growth it is assumed that particle are spherical for D smaller than 10 microns (top of page 15), but for sublimation it is assumed that particles become spherical only when smaller than 1 micron. Why this asymmetry/hysteresis?

- page 16, line 14: 'rime mass fraction does not change during sublimation'. According to equation (29) rime mass fraction does not change during deposition ($dm > 0$) and only change during sublimation ($dm < 0$). Do you mean 'rime mass fraction does only change during sublimation'.

- page 17, line 16: 'remove k from the system'. Do you remove the particle because you have not yet introduced the multiplicity in those equations? Isn't it confusing to give here a Monte-Carlo algorithm without multiplicity, which is (as I assume) not used in SCALE-SDM. Maybe it should be emphasized (again) that this is the underlying

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theoretical model, but not the numerical implementation.

- page 21, line 16: Why $c_j + min(a_k, c_k)$? Shouldn't it be $c_j + max(a_k, c_k)$ for the longest possible minor axis?

- page 24, line 4: 'other planets planets'. Two times 'planets'.

- page 28, section 5.5.5: Would it be possible to discuss the time step of the Monte Carlo scheme in some more detail? Or is this basically the same argument as in Shima et al.(2009) on page 1313?

- page 27, line 3: 'predictor-collector', maybe 'predictor-corrector'?

- page 44, line 11-12: 'Figure 10 clearly indicates that the super-particle number concentration must be larger than 128/cell'. This is not obvious to me. From Figure 10 I would conclude that 64/cell or even 32/cell is actually fine. Can you explain how you determined the value of 128/cell.

- page 60, line 14: 'approximating the particle is spherical' -> 'as spherical'

- page 60 and elsewhere: I find collision-riming and collision-aggregation awkward wording. Riming and aggregation are always due to collisions. Hence, the prefix 'collision' is not necessary.

- page 60, line 25: First sentence of 9.2.7 'We assume that collision-riming's collection efficiency'. Should this read aggregation instead of riming?

- page 62, line 9: 'Seifert et al. (2005)'s model'. This is actually the Low and List (1982) breakup model combined with Beard and Ochs (1995) for small drops. Seifert et al. (2005) did not add anything new to the physics of the breakup process.

- page 62, line 13-15: I would recommend to delete the two sentences starting with 'On average,...'. This is very questionable, has not been shown in the paper and would, in my opinion, be just a compensation of errors. Such a compensating effect is not a good reason to ignore breakup processes.

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