Response to Reviewers

April 2023

Summary

We thank the reviewers again for proposing changes and additional work that have made this article a stronger and more convincing piece of science. Of note, we now include a detailed section 3 (prepared by a newly added coauthor O. Bulenok) that compares SDM results to a set of analytical collision-coalescence-breakup solutions from Srivastava 1982 and discusses the convergence properties of the SDM breakup algorithm in this context. In addition, we attempt to reproduce figure 10 from Straub 2010 and comment on the ability of the SDM algorithm to reproduce this stationary PSD. The sampling appendix describes the necessity of mass-weighting in a multimodal fragment size distribution, as suggested by Axel Seifert. Among several modifications to the text, we have updated figures 1 and 2 to clarify the role of the receiver droplet and correspondence of the fragment size distribution to resulting droplet sizes.

Anonymous Referee #1

Stochastic sampling of the fragment distribution. Existing fragment size distribution parameterizations of Low and List (1982) and McFarquhar (2004) strongly suggest that each fragment regime (filament, disk, sheet) has underlying physics which is captured by the distinct size-modes in fragments distribution (McFarquhar (2004) section 2a). In this case, in Appendix B it's not clear how using a Cumulative Distribution Function as a function of size, rather than the fragments distribution themselves, you intend to capture well these potentially distinct and important modes.

We recognize that our description of how such the fragment sampling step works was misleading. We have made substantial revisions to appendix B (lines 453–454, 469–472) to clarify how the CDF is used to account for all size-modes of the distribution. We concede that there are other means available to sample from the fragmentation functions (lines 480–481).

Decision pathway, Figure 2 diagram (L69-L75): To prevent defining a new superdrop size category, you eliminate completely the smaller superdrop (receiver), and so it is now treated as superdrop size category that holds all the 'satellite' unified droplet fragments - is this correct? [Figure 2, lower right arrow, lower/smaller superdrop]. If so, this is different from suggested by Low & List and applied by Seifert et al. (2005) and/or McFarquhar (2004), where the two remanent drops per breakup even are not eliminated. Thinking about a deeper convective setup with sub-cm/cm -size drops in mind: your algorithm eliminates completely these huge (receiver) drop. These drops are quite low in concentration but should have significant effects over fields like drizzle/precip radarreflectivity and differential-reflectivity. You should mark this as an assumption to be justified / preliminary results.

The intuition that the "receiver" droplet is eliminated and instead holds droplet fragments is correct. However, the fragments that it represents are not only satellites, but could also take the size of parent 1' or parent 2' (in figure 1). Figures 1 and 2 have been updated to denote how the potential resulting droplet sizes correspond to the fragment size distribution, and the caption of figure 1 and the text (lines 68–73) now clarify that the fragmented receiver could take the size of the satellites or the larger fragmented parents. Indeed, these larger parent (or receiver) drops might have significant effects. We now clarify additionally in appendix B (lines 469–472) that the modes of a fragment size distribution with multiple modes must be mass-weighted such that these important large droplets are not underrepresented.

Abstract / Conclusion (L310). The term 'rain suppression' is used in the abstract and conclusion (elsewhere) in a way it might be seen as one of the primary goals of the study. First, the term 'rain suppression' is mostly used in Atmospheric science to reflect increase in aerosol loading, followed by increase in cloud droplet number concentration. This has both microphysical (adjustments) and radiative implications. Second, CB is an integral physical and mathematical part of the overall CC process, and thus it needs to be seen as an essential complementary process that delays precipitation growth due to CC. Both CC and CB clearly depend on physical properties of two interacting drops, hence the importance of the study is in determining realistically what are the relative roles of possibly opposing effects like large/small relative terminal velocity, collision efficiency, coalescence efficiency and characteristic fragments number and size at any such collisional even. The result (outcome) might than show: physically-based delay in growth rate of drizzle/precipitation -size particles a part of the CC process. At the limit of given 'enough' time for CC, the solution converges to near steady-state size distribution. This describes more reliably the presented results.

Thank you for pointing out misuse of the term "precipitation suppression" in the manuscript. We have edited the abstract (lines 9–10, 12) and the conclusion (lines 398–400) incorporating some of the suggested terminology changes.

Abstract / Conclusion (around L320) / L260 / elsewhere. The authors proposed the CB algorithm "to be instrumental in further research on secondary ice production and mixed phase processes". This is unnecessary and unjustified stretch. First, the proposed CB algorithm/assumptions, being an integral development/part of the CC process, are not validated even for relatively simple warm-phase 1D ('rain-shaft') setup. Second, referring to the Phillips et al. (2018) secondary ice production (SIP) suggested mechanism: the proposed SIP is primarily related to the process of supercooled drops freezing, during which part of the frozen shell fragments to produce ice-splinters (see the diagram in his Figure 7). Moreover, since the probability for heterogenous freezing increase with drops volume, the freezing of 'satellite' (small) droplets fragments after collisional breakup are significantly less likely to happen in the relative warmer section of the mixed-phase region, for which the SIP mechanism is suggested. Third, the fragmentation discussed in this study results from different underlying physical mechanism compared to the freezing-drop fragmentation process (mode-1, section 5 in his paper). The fragmentation resulted from collisions between frozen-drops (denser) and more fragile (less dense) ice particles like graupel/ ice/snow (mode-2), resulted primarily from the difference in terminal velocities. Hence, a dedicated microphysical model needs to predict simultaneously these degrees of freedom correctly as a function of modal size and density, which are far more complex than described in this manuscript

Thank you for pointing out the inconsistency in our reference to Phillips 2018 and the discussed SIP mechanism. We have updated the reference to Phillips 2017, which describes ice-ice collisions, include additional references to SIP publications, and have similarly clarified the text (lines 350–352) and the conclusions (lines 408–415) to specify the role of collisional breakup in an otherwise complex multiphase process. We remove the claim about SIP from the abstract.

Equation 6 (around L117): It is not clear how multiplicity, being equivalent to number concentration, can be equal to zero. I understand the sink term of the collisioncoalescence can (potentially) deplete all the droplets within a superdrop category, where in that case it can be used as a criterion for sub-stepping. But then why you reinitialize the multiplicity with the one from the larger size superdrop category. Please explain.

The sink term in collision coalescence (or collisional breakup) can indeed deplete all the droplets within a superdroplet category. This criterion is NOT used as a criterion for sub-stepping, in fact, but can and does occur. In the original Shima et al 2009 implementation, a superdroplet whose multiplicity becomes zero is removed from the system. Because our implementation seeks to preserve the number of superdroplets in the system, we instead split the remaining superdroplet into two identical superdroplets (re-initializing with half the multiplicity and the same attributes), as described near line 125. We require integer multiplicities, thus if the one remaining superdroplet has a multiplicity of only 1, then the second droplet remains at multiplicity zero and is carried around as an inactive tracer.

Minor comments which warrant response:

- L136: Why is that? Is this a choice for computational efficiency, or currently a specific limitation? This suggest 'PySDM' cannot use collision kernels with turbulent enhancement effects reflecting real clouds, and hence cannot represent potentially important drizzle/precipitation acceleration processes. A specific feature of that acceleration is CC of comparable size drops at the vicinity of turbulent eddies. We do not currently include this implementation in PySDM, and neither does the implementation of Shima et al, but it would indeed be possible to do so for turbulent collision kernels. We clarify in the paper now that neglecting these collisions is specific to the kernel used (lines 198–200).

- Figure 3: The remapping of the superdrop phase space to 128 size bins looks quite wiggly, and probably would need some attention once you compared to observed DSDs. We agree, but believe that the resolution is currently sufficient to communicate the key concepts about sensitivity of the algorithm to coalescence efficiency.

- L219: Please indicate where the microphysical processes algorithms come from (reference/s)? We now specify (lines 301-302) that the kappa-Kohler theory is used for aerosol activation and that the procedures are based on those of the program libcloudph++ (10.5194/gmd-8-1677-2015). However, the SDM solves for condensation directly without further simplification or parameterization, thus we have no further references to cite. The implementation of condensation is available in the source code and documentation of ‘PySDM‘ for interested parties.

- L258-L259 and elsewhere: It is written in multiple places (including pointing out to various references) that ‘Superdrop’ / SDM is ‘high-fidelity’ both in warm-phase and mixed-phase. In that case, except for scalability issues which are less relevant in case of 1-D/’rain-shaft’ or 2-D model setups, it’s not clear what are the challenges and complexity that prevents one from comparing development work to obs using idealized setup. This is a minor comment given the manuscript clearly indicates this development work is preliminary incremental path forward subjected to validation. Thank you, we have now clarified lines 348–350. The core challenge is in representing the dynamics of the flow field and coupling evolution of the flow-field to particles, rather than in representing the particles themselves.

- Figure 7: The separate collision and coalescence panels are redundant, as we saw similar drizzle precip mass in Figure 6. Maybe a different colormap/scale will help. Moreover, for an overlapping single contour of rain and cloud, one cannot relate the rate to specific cloud/rain regime. We have decided to include both panels for completeness (now figure 12). The contours have been adjusted as well, and are meant to provide intuition about the presence of hydrometeors rather than characterize specific regimes.

- I’m relatively new to working with the SDM microphysics, but I have some experience with Seifert et al. (2005) collisional breakup parameterization implemented in a spectral bin microphysical scheme. The figure below depicts a fully-interactive 3D model with basic/medium -complexity mixed-phase microphysics, tested in an idealized 3D squall-line with 120-m/1-km vertical/horizontal resolution (idealize in the sense it simulates a section of a much larger midlatitude squall-line). Comparing 100 random samples of surface precipitation size distribution from the stratiform area (in both model and obs), the results (yet to be published) shows reasonable realistic comparison. I would be happy to see and experience comparable setups / results using any SDM code base. Thank you for sharing these interesting results! Unfortunately ‘PySDM‘ does not include coupling to a large eddy simulation or other flow solver at this time, so we would be unable to reproduce your simulation in anything other than a prescribed-flow setting. SDM implementations coupled to flow solvers do exist however, such as SCALE-SDM in Professor Shima’s group.

Anonymous Referee #2

Treating the outcome of a filament breakup event through only two size categories for a colliding superdroplet pair (without introducing a new superdroplet) is a significant simplification and physically inconsistent (at least locally). I think Axel Seifert also made a similar comment. Clearly, there is a need to compare the proposed simplification with a reference run without that simplification. Do we get similar results and similar convergence properties for both approaches? Since the paper's primary focus is to introduce a new breakup algorithm, such a comparison is required. It would be difficult to convince readers of the applicability of the proposed algorithm in more realistic cloud simulations without justifying this simplification in a simpler setup like the current one. Implementing an approach where a new superdroplet is created during breakups would be straightforward in the present box or one-dimensional configuration.

We now include two comparisons to published data in order to justify and validate the algorithm. The first is presented in a new section 3, and includes comparisons to analytical solutions from Srivastava 1982. The second is included in figure 9, discussed in lines 272–317, and compares the steady state size distribution using the Straub 2010 parameterizations to the results published in the same article. As a rigorous validation of superdroplet-creating representation has not been carried out to our knowledge, we found these comparisons against published results to be the most convenient and convincing. Unfortunately it is not straightforward to implement creation of new superdroplets in the code-base PySDM used for these studies due to its parallel-computing properties.

The collisional breakup introduces an additional element of stochasticity through random sampling of a fragment size distribution. Hence, it's essential to know the convergence properties (with the number of superdroplets) of the mean and variance of drop statistics. No such test is presented in the paper.

Our new comparisons to published data (Srivastava 1982 in section 3, Straub 2010 in figure 9) now plot the mean and spread of the SDM model results using different numbers of superdroplets. A detailed discussion of the convergence properties is likewise presented in section 3.

The collisional breakup has almost negligible influences on cloud/rain properties in the one-dimensional test presented here due to a shallow cloud condition. The authors could also test the scheme in an idealized two-dimension deep convection with only warm phase physics. It would help understand the performance of the scheme in more realistic dynamics and the influence of associated feedback.

We hoped to follow the reviewer's suggestion of an idealized two-dimensional deep convection setting, but were unable to find or replicate a validated prescribed flow setting from the literature. Instead, to address these concerns, we have increased the updraft velocity and domain size of the presented one-dimensional setting to 6 m/s and 5km (respectively). We also now present vertically-averaged particle size spectra sampled at various times during the simulation in figure 10. We discuss in lines 342-344 the implications of property-dependent breakup on the spectra during the brief 5 minute window from 900s to 1200s.

Minor comments warranting response:

- Figure 4b: Why is a higher Ec value (0.99 vs. 0.95) used here than the deterministic fragmentation function case? Thank you for the catch – both cases now use Ec=0.95, and we have switched to an exponential fragment size distribution in 7b (only one parameter instead of two).