GMD manuscript #2020-404: replies to the second round of reviews

Michael Olesik, Sylwester Arabas and co-authors

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We hereby express our appreciation for the reviewers' and editor's feedback.
Below, we include point-by-point replies to all comments provided by the reviewers. Quoted text of the reviews is typeset in teal and with larger margins.
A revised manuscript with changes automatically highlighted using the latexdiff tool is enclosed with additions marked with blue colour and deleted text in red. Additions to the bibliography are not colour-highlighted, but all references to new bibliography items are within blue-underlined blocks in the text.

comments by Anonymous Referee #2, 31 Aug 2021

> Second review for the manuscript [gmd-2020-404] entitled "On numerical broadening of particle size spectra: a condensational growth study using PyMPDATA 1.0" written by Olesik et al.
>
> On the first stage, as a reviewer, I requested the authors to improve the manuscript by performing some additional experiments, because some studies have already shown the importance of vertical advection in the broadening of drop size spectra during condensational growth. The authors did perform extra experiments in which the vertical advection of drops and water vapor mixing ratio are allowed, and added a section to the manuscript. In their experiments, vertical grid spacing is the most important parameter in controlling the DSD broadening, as already pointed out by Morrison et al. (2018) and Lee et al. (2021). Although they added the results including the effects of vertical advection, however, this study still contains some serious problems described below. My recommendation is to reject this manuscript for publishing on GMD.
>
> First, there are few links between the box model study (Section 2) and the column model study (Section 3). The two studies use totally different environmental setups, and furthermore, while the box model study focuses on the variant MPDATA flavors, the column model study fixes the numerical method (except the

> number of iterations) and just focuses on the sensitivity on experimental parameters such as time step, vertical grid spacing, and spectral spacing. I do not feel that this manuscript is well organized, and therefore, it is hard to understand what the authors want to say.

The two main goals of the paper can be summarised as follows. First, to comment on several studies where unspecified variants of MPDATA were applied for calculations of condensational growth and led to misleading general statements on the performance of the scheme. Second, but also to support the first point, we aim at providing an abridged tour of MPDATA features and nuances relevant to the problem of condensational growth. To this end, the box-model part (Section 2) features a simplest possible test case enabling depiction of the algorithm options and characteristics. All figures presented in the paper are readily reproducible using pure-Python open-source code aiding potential developers in building upon the presented developments. Revising the paper, we aimed at improving the flow, yet the concept of two separate sections remained.

> Second, the relative dispersions presented in this study have a parabolic shape, minimum in the middle of column. I cannot find such a shape in any other previous studies, even in the reference the authors mentioned (Arabas et al. 2009). I am not sure whether the parabolic relative dispersion is attributable for their imperfect experimental settings such as excluding the vertical advection of potential temperature, but it is clear that the authors should be more careful in explaining the results.

As an example, in figures 2, 3 & 6 in Lu and Seinfeld (2006)[1], which are based on LES simulations of with bin microphysics, the "parabolic" shape is also evident. The hypotheses put forward by Lu and Seinfeld (2006) are that:

- "*the peaks of d at cloud top are most likely due to the entrainment mixing of quiescent unsaturated free tropospheric air with saturated cloudy air*";
- "*the peaks of d and σ near the cloud base are a result of "entity" mixing, as described by Telford et al. [1984].*";
- "*the minima of d and σ at midcloud are the result of competition between downdraft broadening and condensational growth narrowing.*".

The employed single-column kinematic framework clearly does not include the processes featured in the above argumentation, what in fact is interesting from the point of view of validating these hypotheses. Yet, as it has been clearly indicated in the manuscript, the main point in comparing the obtained profiles with results from more complex simulations and measurements is to confirm if the test case covers the parameter space relevant to the studied problem. A reference to the discussion in Lu and Seinfeld (2006) has been added.

[1] `https://doi.org/10.1029/2005JD006419`

> Third, I feel that the manuscript includes too many rhetoric and unnecessary conjunctions, which make the manuscript difficult to read.

We have aimed at correcting it by shortening sentences and being more specific in numerous statements.

> As for minor problems, 1) Figure 10 is very hard to understand, and 2) the authors seem to have made some mistakes in controlling the experimental parameters to make Figure 12 (e.g., Δt in the second column of the first row and the third column of the third row).

Figure 10 is a three-dimensional perspective view of a snapshot of the simulation state. It is only meant to provide a qualitative and hopefully intuitive picture of the modelled system. The discussion of the simulation results is based on subsequent two-dimensional plots in figures 11 and 12.
It has been indicated in the text that the timestep for each run is adjusted to maintain stability constraint of the scheme. The value indicated in each plot is the actual value used after the adjustment.

comments by Anonymous Referee #4, 3 Sep 2021

> General comments:
>
> The authors explored the application of the Multidimensional Positive Definite Advection Transport Algorithm (MPDATA) in simulating the condensational growth of cloud droplets. They have demonstrated that the MPDATA is able to reduce the artificial diffusion of bin-method at the cost of computational resources.
>
> The upwind scheme should be equivalent to a high-order scheme. What is the advantage of using upwind scheme instead of high-order schemes, computational cost?

MPDATA relies on iterative application of upwind with the first iteration employing physical velocity and subsequent corrective iterations using antidiffusive velocities. The resultant scheme, depending on the variant chosen, is second- or third-order in time and space, while maintaining the key properties of upwind, namely positive-definiteness and stability constraints.

> The authors have demonstrated that the “best variant” is 10 times more costly than the upwind scheme, how feasible is it to use the “best variant” in 3-D cloud models?

In the section on computational cost in the revised manuscript, we added references to the studies of Liu et al. (1997)[2] and Onishi et al. (2010)[3] where analogous upwind-normalised measures were reported. The methods analysed in these two studies had roughly three and four times higher cost than upwind, and these methods are used in 3-D cloud models. Clearly, the computational cost is not the only relevant measure: memory footprint, parallelisation opportunities, convergence rate, timestep constraints, monotonicity – all these factors need to be taken into account for a comprehensive assessment of a scheme, what is now also underlined in the discussion of the computational cost.

> This manuscript reads like an early draft that is far from being ready to be peer-reviewed. To improve the readability of the manuscript, I would suggest the authors summarize the key points of those cited papers and merge those paragraphs that only consists of two sentences in the introduction. Also, the English writing of this manuscript can be improved substantially. I've listed several examples that can be improved in the instruction. But the authors should go through the entire manuscript and try to make the writing more concise.

We have addressed the points raised by both reviewers in this regard and redacted multiple passages aiming at correcting the language and improving conciseness.

> Theories and numerical methods for "condensational growth of cloud droplets" is well-established, which are even validated against the laboratory experiment. I would suggest the authors consider using common nomenclatures such that the work can be understood by a wide range of audiences in the cloud-physics community.
>
> I cannot recommend the publication in its current form. A major revision is required for further consideration.

The manuscript has undergone a major revision. While we do aim at catering to a wide audience range, our key goal remains to provide a detailed set of instructions for geoscientific model developers - in line with the journal scope. To this end, we propose to retain within the manuscript discussion of such aspects as handing mass-doubling coordinate transformation within the upwind numerics, an aspect that on the one hand seems basic, on the other hand has not been explicitly covered in earlier works on the topic. Having picked test cases from literature, we hope that further links with the common nomenclature can also be obtained through the referenced works.

> Specific comments: 1. L.80: Why is the Eulerian scheme is robust in representing particle collisions? First, the Eulerian scheme is a mean-field approach, which is

[2] `https://doi.org/10.1175/1520-0469(1997)054<2493:VOMFCO>2.0.CO;2`

[3] `https://doi.org/10.1299/jee.5.1`

not able to represent fluctuations during collisions. Second, it is suffering from artificial numerical diffusion in modelling the collision process (Li. et al, 2017)[4].

This sentence was removed. The two aspects the Reviewer point out are certainly true. There are also aspects which make bin representation "robust" such as consistency of response to subgrid dynamics in a CFD solver or simply implementation aspects, yet we opt not to discuss it in the present paper (the reference to Li et al. 2017 has been cited in the Background section).

2. L.90-91: I don't understand the logic of transitioning to Kelvin effect here. Could you elaborate more on this transition?

Added to the discussion of eq. 2.3.

3. L.112: Do you mean "more significant .... than ..."?

Corrected.

4. L.115-117: I couldn't find the verb of this sentence. Could you please rewrite it?

Rephrased.

5. L.125: Please check the grammar.

Corrected and shortened.

6. L.132: ... p to x with x being ...

Changed.

7. L.149: Do you mean "as the following"?

Rephrased.

8. L.151: What is "GC" exactly? Is "G" the same as the

This is now clarified in a newly added sentence: "*Note that the values of the Courant number itself are not used, only the product GC of the coordinate transformation term G and the Courant number C.*"

[4] Li, X.-Y., Brandenburg, A., Haugen, N. E. L., and Svensson, G.: Eulerian and Lagrangian approaches to multidimensional condensation and collection, J. Adv. Model. Earth Syst., 9, https://doi.org/10.1002/2017MS000930, 2017.

9. L.152: Why is the Courant number is “the velocity …”? Do you mean “… to warrant … ”?

Simplified the sentence.

10. L.161-163: Could you please reword this sentence to improve the readability? For example, we can use short sentences and connect them in a logical way?

Shortened.

11. L.169: You may define what the “S” is first.

Added clarification referring to relatie humidity.

12. L.174: Please cite the reference where you got these number.

Added reference to East & Marshall 1954[5].

13. L.186: What is “(1; 26) μm”?

Shortened and rephrased: ”*The domain span is 1–26 μ m.*”.

14. L.187: Please check the grammar. Do you mean “in the first …”?

Rephrased.

15. L.188: the second. What is “in the range of (0.03; 0.07)”?

Rephrased: ”*variable Courant number approximately in the range of 0.03 to 0.07 for $p = r$*”

16. L.189: Please rewrite the sentence and make it clearer and avoid mistakes in grammar.

Rewritten.

17. L.191: shows.

Corrected (made the subject plural).

18. L293: Double-Pass Donor-Cell (DPDC). Please check other parts of the manuscript thoroughly.

Spelled as suggested, also reserved the term ”pass” for DPDC now ”iterations” are used whenever generally referring to MPDATA iterations.

[5] https://doi.org/10.1002/qj.49708034305

19. L.381: If the “best variant” is 10 times more costly than the upwind scheme, how feasible is it to use such a scheme?

The use of upwind as a benchmark...

Technical corrections:

20. In the title and other parts of the paper, please replace “particle size spectra” by “particle-size spectra” as only two nouns are allowed to be connected at a time in English grammar.

Corrected.

21. MPDATA (Multidimensional Positive Definite Advection Transport Algorithm) should already be fully spelled out in the abstract.

Corrected.

22. Please use consistent key terminologies to improve readability, such as “particle-size distribution” or “particle-size spectra”.

Corrected.

23. L.45: Shouldn’t the reference be in a bracket?

Corrected.

24. L.47: large-scale models; climate-timescale simulation

Both corrected.

25. L.48 : What does “there” refer to? Please check the grammar in this sentence.

"there" removed.

26. L.49: What is “particle size-spectrum dynamics ”?

Rephrased with "*resolving particle-size spectrum evolution*".

27. L.50: What is “size effects”? Do you mean simulation domain-size? Also, please check the grammar of this sentence. For example, “. . . that. . . are”?

Surplus "size" removed, sentence rephrased.

28. L.53: a population.

Corrected.

29. L.58: What is “the effects”?

Rephrased removing the word ”effect”.

30. L.62: “see” − > “we refer to”. Please also check the format of citations in this sentence.

Changed and fixed the citation enumeration.

31. L.63: the application.

Corrected.

32. L.71: size distribution of cloud droplet. Please see my comment 1 in this section.

Changed to ”droplet-size spectrum”.

33. L.79: large-scale.

Corrected.

34. L.81-84: I suggest the authors to shorten the sentences such that they are concise and easy to read.

Shortened and split in two.

35. L.86: … can likely be…

Changed.

36. L.88: Brown (1980) also covers …

Changed.

37. L.90-91: due to its …

Changed.

38. L.96: “which is focused on the evaporation of an “aerosol cloud”” does not read right. Also, what is “aerosol cloud”?

Shortened to ”which is focused on evaporation”.

39. L.98: compared to.

Corrected (in several other places as well).

40. L.99: was used.

Corrected.

41. L.102: Tsang and Rao (1988) pointed out that ... the upwind scheme... the prediction accuracy of the mean radius.

Changed as suggested.

42. L.105: ... in a chapter focusing...

Corrected.

43. L.106: was presented.

Corrected.

44. L.108: What is "particle size computations"? The latter lists ...

Rephrased (and corrected latter to first as it was swapped).

45. L.110: ... the condensational growth...

Corrected.

46. L.120: what is "sic!"?

Removed for clarity (was meant to underline no typo despite large value reported)