Reply to the comments of Referee #1 (RC1) on article gmd-2023-26

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Thank you for taking the time to read through our manuscript and provide us with positive and helpful comments. Below are our point-by-point responses to your comments.

1. Maybe the only general comment is whether the authors could reduce the length of the manuscript, or consider a more clear separation between the discussion of general aspects of the SDM and the particularities of the implementation in Fugaku, as I tried to do in the first paragraph of this letter. I wonder if section 6 could be shorten. As said, this is a minor point and I realize it is difficult, but somehow I think the reader might be better guided through the paper to concentrate on the aspects that might be more interesting for her or for him.

   I found the conclusions, however, very well and they helped me to end the paper with a good idea of the various parts.

   Thank you very much for your suggestion. We have simplified the Discussion (Section 6) and other sections as much as possible for clarity and to reduce redundancy.

2. Title, abstract, line 1, and through the paper, the authors use the term ”ultrahigh-resolution” and I was wondering if they could substitute that term by another one that is more informative. It seems that, in line 9, the authors refer explicitly to centimeter to meter scale, so why not say ”centimeter-to-meter scale resolution” or ” submeter resolution” ?

   Otherwise, what would come after *”ultrahigh resolution”* when we reach the following step towards higher resolution?

   As you suggested, we have revised the term to ”meter-to-submeter scale.”

3. Title, abstract, line 5, and through the paper, the authors use the term *”sophisticated”*. I was not sure what it means. Does it mean that it considers more physical processes, or a better model of them, or does it refer to the technical implementation from a computer science point of view?

   Thank you for your comment. In the revised manuscript, we have considered words that improve clarity. As other papers have also used the term ”sophisticated” microphysics model to describe a model that introduces a more complex process, we have retained it in the revised manuscript. Alternatively, our study focused on improving many aspects, such as numerical precision, algorithms, and computational performance. Thus, we have made some revisions to clearly state that.

   In addition, based on your suggestion, we have revised the title of the manuscript to ”Overcoming computational challenges to enable meter-to-submeter-scale resolution cloud simulations using the super-droplet method”

4. abstract, line 4, the authors say *”does not make any assumption for the droplet size distribution”*. Since some aspects of the dynamics within the superdroplet or the interaction between superdroplets are still modeled, as discusses in section 2 and 3, I wonder if it might be better to say ”makes less assumptions about the droplet size distribution and it is more physically sounded”, or something similar.

   As suggested, we have revised the statement to ”makes fewer assumptions for the droplet size distribution.”

5. Abstract, line 18: instead of *”perfect weak scaling”*, it might be stronger and clearer to say 98% weak scaling up to the corresponding number of nodes or cores, as it is done in the conclusions.

   Thank you for your suggestion. We have revised the phrase to ”98 % weak scaling” in the revised manuscript.

6. Introduction, line 35, the authors refer to Schulz and Mellado, 2018, but the reference Schulz and Mellado [2019] might be stronger for their case. Schulz and Mellado [2019] studies one micro-physical effect, namely, sedimentation, and shows that sedimentation is more important than previously thought, which strongly supports the efforts presented in this SDM manuscript to better represent the DSD.

   Thank you for the suggested reference. We have cited Schulz and Mellado (2019) instead of Schulz and Mellado (2018). We have also added the following statement to L35: Schulz and Mellado (2019) investigated the joint effect of droplet sedimentation and wind shear on cloud top entrainment and found that their effects are equally important.

7. Introduction, line 34 say *"which sets the eddy viscosity constant"* referring to DNS. I wonder if this sentence is needed. This seems to suggest that DNS is one type of LES, which is not the way DNS is used in turbulence research, where the concept originates from, since there is no eddy viscosity in DNS [Orszag and Patterson, 1972, Moin and Mahesh, 1998, Pope, 2000, Mellado et al., 2018]. DNS rescales the original in terms of size or in terms of physical properties to study Reynolds number effects, and remains accurate in the smallest resolved scales, which LES does not.

    Thank you for highlighting this. In the revised manuscript, we have explained DNS and LES, based on your comment and Mellado et al. (2018).

    DNS solves the original Navier–Stokes equation but changes only the kinematic viscosity (or Reynolds number) among the atmospheric parameters.

    LES solves low-pass filtered Navier–Stokes equation for unresolved flow below filter length.

8. In several places through the manuscript, the authors refer to particle-in-cell (PIC) methods. What are the differences between the superdroplet method (SDM) and particle-in-cell methods?

    Thank you for your comment. The PIC method is used to solve a specific type of partial differential equation, which describes a coupled system of particles and cell-averaged variables. SDM can be regarded as an application of the PIC method to solve cloud microphysics and macrophysics. In the original manuscript, we did not introduce an abbreviation for the super-droplet method, but in the revised manuscript, we have defined the abbreviation properly and have defined the PIC method and its relation with SDM more clearly.

    added to L42: In this study, we focus on the super-droplet method (SDM) as one of the particle-based Lagrangian cloud microphysics schemes.

    added to L72: In general, the method of solving partial differential equations, which describes a coupled system of particles and cell-averaged variables, is called the particle-in-cell (PIC) method. The SDM can be regarded as an application of the PIC method to solve cloud microphysics and macrophysics.

    simplified L72: Although such dynamic load balancing is adopted in some plasma simulations (Nakashima et al. 2009) when the PIC method is applied, dynamic load balancing is not a good option for weather and climate models.

9. Section 2, line 118, the authors write *"the anelastic equations assume horizontally uniform mean fields and are not appropriate for computing wider domains"*. I wonder if this sentence is needed. The mean fields need not be the reference fields that are used in the anelastic formulation, and one can have anelastic formulations of statistically inhomogeneous flows (a cloud bubble).

    Thank you for your comment. We agree that the description of anelastic equations in Section 2 is redundant and confusing. In the revised manuscript, we have clearly stated the differences between fully compressible and anelastic equations.

10. Equation (1), what are the assumptions for using this equation? The same applies for instance to the description of collision-coalescence and relates to point 3 before, namely, that there are still some assumptions in the SDM and it might be convenient to explain them as clearly as possible to better interpret the results and the limits of applicability.

    In the revised manuscript, we have stated the process ignored and assumptions made for microphysics to be solved explicitly. We added the phrases in blue, as follows:

    L132: In this study, only the following warm cloud processes were considered: movement, activation/deactivation and condensation/evaporation, and collision–coalescence. Spontaneous and collisional breakup process were not considered in this study.

    L136: The ith SD moves according to the wind and falls with terminal velocity assuming that the velocity of each SD reaches the terminal velocity instantaneously.

    L146: The terms a/R and b/R^3 represent the curvature and solute effects, respectively. The ventilation effect is ignored in Eq. (2).

11. Section 3.3.1, *"The effective resolution is* $6\Delta - 10\Delta$ *for planetary boundary layer turbulence"* I assume that this is for the second-order methods that the authors use, but they might indicate it explicitly here because the effective resolution might be substantially smaller in pseudo-spectral schemes or similar, which are often used in boundary-layer meteorology

[Sullivan and Patton, 2011, Pope, 2000].

Thank you for your comment. Second-order spatial discretization for the pressure gradient was employed in this study. Thus, the dynamical core used is (overall) second-order spatial accuracy. However, as discussed by Nishizawa et al. (2015), the pressure gradient term is mainly for fast acoustic waves and has less effect on slow modes, such as mixing, i.e., energy spectrum. Thus, we believe that our simulations have a certain degree of numerical accuracy. As you highlighted, large-eddy simulations using the (pseudo) spectral method are numerically more accurate and might have an advantage in effective resolution because explicit filtering can be used. This has been stated in the revised manuscript.

12. In section 6.4.1, I was wondering how much computational time or memory save is gained by using FP32 instead of FP64 in that part of the microphysics. I was trying to have a sense of priorities in addressing the standing challenges. For instance, the disk space challenge indicated in section 6.3 seems most important, but I might be wrong.

    In implementing SDM, most of the information about SDM is the arrays for SD attributes because other information, such as intermediate values of SD position, which is required for the second-order Runge-Kutta method (Heun's method) for SD tracking, is mainly stored in a local stack in each OpenMP thread, and because the size of the working arrays, such as for SD sorting is small, as discussed in Section 3.3.5. The amount of memory that SDM uses is estimated as

    meshes × SDs per cell × attributes × information of each attribute × (1+extbuf);

    here, extbuf is the ratio of the extra buffer arrays to the number of SDs. Thus, the amount of information would be halved if information on all attributes is halved. This applies if we reduce the numerical precision from double to single precision. Alternatively, the computational cost is a bit more complicated, for example, for condensation/evaporation and activation/deactivation. The number of iterations for Newton's method would likely change if the tolerance relative error is squared. We speculate that the number of iterations increases by 1 or 2 because Newton's method is quadratically convergent.

    In the case of SCMS, the elapsed time only for microphysics using four nodes of FX1000 and with FP64 was 248(349) min without (with) collision-coalescence calculations in non-cloudy volumes. The memory for only microphysics was 54.9 GB. On the other hand, the elapsed time with FP32 was 150(274) min, and the memory was 29.7 GB. The computational-cost reduction on collision–coalescence is insignificant because the amount of information of a key for SD sorting did not change when we used FP64 to FP32, and the multiplicity remained INT64 due to a small number of SDs. In the revised manuscript, we have added the above discussion to L811.

    The problem for checkpoint/restart files can be avoided by consulting the computer center, and we have modified this in the revised manuscript. However, we believe that it is an important subject, and a data-scientific approach may help mitigate it.

13. Conclusions, line 1129 *"In contrast, the constant multiplicity method is a natural choice for DNS"*. I think I did not understand this sentenc.

    Thank you for your suggestion. We have removed this sentence.

14. I think the conclusions do not refer to section 3.3.2 "Super-droplet movement", which I thought was interesting in general, not only for the particular implementation described here.

    We are glad to hear that you are interested in our manuscript. In the original manuscript, we summarized Section 3.3.2 at the end of the first paragraph in the second item. In the revised manuscript, we have added the following: For SD movement, to maintain many SDs within convective clouds and reduce the nonphysical variance of SDs, the 3D CVI of the second-order spatial accuracy on the C-grid was derived to ensure consistency between SD number and air densities. Then, we stored the relative position of SD in a block by a fixed-point number using FP32 to ensure a uniform representation precision in the domain, which keeps the numerical representation precision for meter-to-submeter resolution while using a low-precision format.

References

Schulz, B. and Mellado, J. P.: Competing effects of droplet sedimentation and wind shear on entrainment in stratocumulus, J. Adv. Model. Earth Syst., 11, 1830–1846, https://doi.org/10.1029/2019MS001617, 2019.