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Interactive Comment

Interactive comment on "Speeding up a Lagrangian ice microphysics code" *by* S. Unterstrasser and I. Sölch

Anonymous Referee #1

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General Comments The authors shows the artificial inhibitions in the deposition, sublimation and nucleation processes, which is caused by purely numerical parameters in Lagrangian cloud microphysics models. After that, they identify those artificial errors can be reduced by choosing appropriate number of simulation ice particles (SIP) and the model time-step required in the each microphysical process. In addition, they propose a method to solve these problems using numerical techniques: SIP merging, SIP splitting, and stochastic nucleation method. These contents can be published in GMD after several revisions.

Major comments. 1. Title does not reflect the contents of the manuscript. In particular, the term "speeding up" is in a broad sense. Title should be restricted and contain how authors speed up their model. For example, I propose a title such as "Optimization of the number of SIPs in a Lagrangian ice microphysical model using SIP merging/splitting method".

2. Authors should describe about the architectures they used when estimating calculation time. In addition, floating point operation (FLOP) is necessary when authors estimate the computational cost, which is expected to be reduced by the proposed methods. Authors notice several times that memory utilization, which depends on the number of SIPs, is also the issue. It is better for readers to show the dependence of the memory utilization on the number of SIPs. I recommend authors to refer to Shima et al. (2009), Quat. J. Roy. Meteor. Soc to estimate the computational cost. If the title remains "speeding up the model", authors need to mention about the efficiency of parallel computing (e.g., weak scaling or strong scaling efficiencies). Otherwise, discussion about the issue is not necessary because the issue is far from atmospheric science.

3. Although authors propose SIP merging/splitting method and stochastic nucleation method, I cannot evaluate whether these techniques are new or not because there is a lack of reviews about techniques used in other Lagrangian particle models. For example, how about the number of SIP in Lagrangian models described by Paoli et al. (2004) and Shirgaonkar and Lele (2006), which are introduced in Section 1?

4. Exact prognostic variables used in this simulation should be described in detail in Section 2 or Appendix since the term "around 15 attributes" is ambiguity. In addition, the predicted attributes are important for the evaluation of convergence of the simulated results in the sensitivity experiments. I guess the number of bins k is used only in initialization. Please refer to the typical value of k in spectral bin microphysical models such as Khain and Sednev (1996) or others. In a section for discussion or the end of Section 4, in the estimation of appropriate value of NSIP, comparison of the number of the prognostic variables used in the Lagrangian cloud microphysics model with those in other spectral bin cloud microphysics models is to be discussed. Then it is better for readers to consider the availability of Lagrangian cloud microphysics model to simulate

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the real atmosphere.

5. Numerical settings in Section 3.1 should be described in detail to be self-contained in this manuscript unless this manuscript is part 2 of the paper, which is referred to. Simulations should be reproduced by any other scientists. For example, at least the following should be described: initial conditions and boundary conditions of the simulation case, pressure of the model domain, initial perturbations used in the initiation of cirrus clouds, and parameters in the log-normal size distribution of cloud ice. In addition, please refer to the papers, which describes the definition of the optical extinction.

6. This manuscript propose a method of SIP splitting and merging to optimize the number of SIPs. Please summarize the cloud microphysics processes, which decrease or increase the NSIP, in Section 2 for readers to understand easily the time advance of NSIP in cloud growth. In particular, it is confusing whether NSIP decreases by falling out from the model domain or some cloud microphysics processes. I guess there are no physical processes to reduce NSIP except for aggregation, riming, and melting, those are not included in this model. In addition, there is no description about how the shape or bulk density of ice particles diverges in deposition and sublimation while authors apply the SIP merging method to only crystal sizes. There exist additional question that how much variability of the shape exist in nature and how representative this simulation cases is. If this simulation case produces ice particles in the narrow range of the dimension of shape or bulk density, SIP merging for shape or bulk density may be necessary to reduce computational cost.

7. There is less description about why the experiments A1 to A5 are required and I don't understand why A1s uses the values of vmax, k, Nmax listed in the table 1. In particular, readers often do not know which values are usually used in Lagrangian models. Better way is that firstly authors show a simulated results with a standard numerical settings used in previous study (e.g. Unterstrasser and Gierens, 2010). After that authors point out several numerical issues to be solved in this study. Correspondence of the objectives and their solutions should be clarified. By the way, in Fig. 1,

there are patches of extinction x in the fall streaks in the A4 experiment as also mentioned by authors. These patches are removed (smoothed) by introducing variability to the pathway of ice particles growth induced by turbulence using SIP splitting method. However there is less differences in total extinction, ice crystal number, and ice water path among sensitivity experiments as shown in Fig. 2 and Fig. 3. These results suggest that choice of total or average values of physical parameters is not good to discuss about statistical convergence. Evaluation of inhomogeneity would be required particularly in the case where small scale fluctuation has important roles to determine the structure of cloud system (e.g., Spichtinger and Gierens, 2009, ACP, Part 1b).

8. In Section 3.2, only 2 simulation cases for vertical velocity is not enough because results substantially differ when changing vertical velocity. I understand 2 cm s-1 is a typical value of vertical velocity originates from synoptic scale disturbances and 20 cm s-1 is a typical value of vertical velocity originates from gravity wave. This range of vertical velocity covers broad regions of cirrus formation as shown by Karcher and Strom (2003), ACP. However, large value of vertical velocity more than 100 cm s-1 is also important for anvil cirrus associated with deep convection. Morrison and Grabowski (2008), JAS, doi:10.1175/2007JAS2374.1 is a good example to discusses about the convergence of numerical results with various values of vertical velocity, model time-step, and vertical layer thickness. If authors don't perform additional comprehensive simulations, please show readers the general dependences of required model time-step, nmin, NGB,M1, NGB,M2, NGBS1, and NGBS2 on vertical velocity. As illustrated by Joos et al. (2008), JGR, doi: 10.1029/2007JD009605, nucleated ice number concentration is roughly estimated by saturated vapor pressure (number density) and vertical velocity. Authors will estimate those dependences using this relationship.

Minor comments 1. P.3788 line#9. Instead of "moderate number", describe specifically. 2. P. 3788 line#11 delete "realistically". . 3. P. 3788 line#12 instead of "several strategy", describe specifically 4. P. 3788 line#14 sentence "These may well ∼" is lengthy in abstract. 5. P. 3788 line#21 interaction of ⇒ interaction among 6. P. 3788 line#25 parameterized 7. P. 3789, line#1. Sequence from aerosols to bulk model is confusing. Is the description about bulk model is necessary? 8. P. 3789, line#20, Is the description about the model for warm clouds is necessary? 9. P. 3789, line#27, “we” should not be used here to avoid subjective view. In addition, “these studies” contains works achieved by other authors such as Lainer, Karcher, . 10. P. 3790, line#1, I confuse the relationships between “statistical convergence” and “model outcome”. Describe specifically. 11. P. 3790, line#5, “we” should not be used here again. 12. P. 3790, line#21, “The single simulation ice particle”, here define “SIP” again in addition to the definition in abstract. “act” ⇒ “acts” 13. P. 3791, line#5, I don’ use “unit 1”. Instead, I use the term “dimensionless”. 14. P. 3791, line#21 “initialized” 15. P. 3791, line#23, define “RHi” here instead of line #24. 16. P. 3792, line#3, “NSIP-dependency” on what ? 17. P. 3792, line#19-24. Really? In Fig.3 There is no IWC below 400m at t = 4hr. This means that sedimentation of ice particles does not contribute to the loss of particle mass. I guess the loss of extinction after t = 150min is caused by sublimation of ice particles. 18. P. 3793, line#3-4, the sentence is not clear since authors use “on the other hand” twice. 19. P. 3794, line#16-17, associated with major comment #7, figures to show change of PDF by the proposed method is necessary. 20. P. 3795, line#6, I don’t understand why nmin is high value such as 10-100cm-3 because typical value of the number concentration of ice crystals is 1∼100 liter-1, which is 0.001∼0.1 cm-3. 21. P. 3795, line#19-26, I need explanation about mechanism of the sensitivities in addition to the statement of fact as shown in Figures. 22. P. 3797, line#5, I understand the role of fluctuation but I don’t understand how fluctuation is represented in the Lagrangian model. Is it like a Monte-Carlo calculation for the transport of SIPs by turbulence? 23. P. 3797, line#15-17, There is no description about “the monotonic function c”. 24. P. 3798, line#13 already what? 25. P. 3800, Section 4.3, please review techniques used in other Lagrangian models. Is this technique new, really? Because I feel that this implementation is quite natural and common among Lagrangian models. If not, please review that other models uses enough long time-step or large grid box or simulate those cases with high vertical velocities to avoid the problem. 26. P. 3801, line#16-17, “However ∼”. Reference is necessary to show the degree of physical uncertainties. 27. P. 3801, line#17-18, “further decrease ∼ meaningless”. I almost agree with authors but it is necessary to determine something of threshold to evaluate statistical convergence such as a certain criterion of dt = dtc ,which permits the error of ± 5% of Ntot. 28. P. 3802, line#19, “Less than 50SIPs in a grid box∼”. I don’t understand the meaning of local size distribution. Because there exist red patches in the fall streaks. In addition, why the value of “50”? In Fig. 2, mean NGB shown by green line falls to 2 at the end of its simulation period but the total extinction and the ice crystal number concentration is close to other simulations. On the other hand, blue line keeps the value of approximately 100. Moreover, I don’t know which amount of NSIP is used for attributes of size distribution and others (e.g., shape, bulk density and so on). 29. P. 3803, line#15, why italic “partitioning”? 30. P. 3803, line#26-28. Please estimate the memory utilization.

Interactive comment on Geosci. Model Dev. Discuss., 6, 3787, 2013.