Reply to the reviewers' comments of the manuscript "Speeding up a Lagrangian ice microphysics scheme.

We thank the two anonymous reviewers and Ilja Honkonen for their constructive reviews and suggestions. This document comprises replies to all three reviews. First we give some general remarks to points which were addressed by more than one reviewer. Then point-to-point replies to all comments are given. For convenience reasons, we add the original comments of the reviewers in bold font.

General remarks

1. Novelty of the results

It was not clear to the reviewers whether the presented results are novel. The development of the SIPMERGE operation and the stochastic nucleation implementation were triggered when we realised that the numerical convergence of homogeneous nucleation can be reached only with a vast amount of simulation particles (p3790, l2-l8). As written in the introduction (p3789, l17-l20), other Lagrangian ice microphysic models by Paoli et al. (2004) or Shirgaonkar and Lele (2006) do not consider homogeneous nucleation. If at all, ice crystal "activation" is included in much simpler forms. So far these models have been used for contrail modelling and not for natural cirrus modelling and therefore do not encounter problems we faced, once the highly non-linear nucleation process is incorporated. We highlight the novelty of the presented techniques with a few sentences in the introduction.

2. General validity of the results

We expanded several sections of the manuscript in order to highlight the general validity of the presented results. First, we describe the box model studies of section 3.1 in much more detail, see also newly added tables 1 and 3. Numerical convergence is reached with less than 50 SIPs for all examined meteorological conditions which cover a fairly broad range of parameter combinations.
Figure 6 and 7 show the fast convergence of the stochastic nucleation implementation for constant updraught speeds of 2 and 20 cm/s. We include a new figure in the revised version which shows the sensitivity to n_{min} also for scenarios with more flexible RH_i-evolutions. As the RH_i-evolution is less smooth than in our original examples, we expect that these simulations are numerically harder problems. Nevertheless, we find again a low sensitivity to n_{min} which, in our opinion, proves the robustness of our proposed approach.

Comment by Ilja Honkonen

About section 4.2:

It seems that the centre of mass of the SIPs is not conserved when joining particles as the position of the new SIP is chosen at random. How large localised effects does this have in the simulation? Only global parameters were shown as proof e.g. in figure 5, how would those parameters differ in grid cells (boxes) where the particles are actually joined? For example all grid cells where particles were not joined during the entire simulation could be excluded from the results of figure 5 to show the effect of joining more realistically. When gathering statistics from SIPs, are their attributes interpolated to the grid cells, for example in order to obtain the total mass in each cell? If this is the case does the SIP splitting and joining procedure conserve the interpolated quantities? For example Lapenta & Brackbill 1996 (http://dx.doi.org/10.1006/jcph.1994.1188) present a method for achieving this, how relevant would preserving also the interpolated quantities be for this work?

Dear Ilja, we appreciate your voluntary comment. Thanks for pointing out the useful paper by Lapenta and Brackbill (1994) to us. It is true that our SIPMERGE operation does not assure that the centre of mass is conserved. However, this is not critical in our model. The coupling between the SIPs (ice mass, latent heat) and grid fields (water vapour, temperature) is explained in the original model description paper (Sölch and Kärcher, 2010). For convenience reasons we extract their section 2.3.1 (ii):

"Within a grid box volume, each SIP interacts with the volume-averaged grid point values of the Eulerian model variables u, qv, p, T, among others. While the exact SIP positions are known, subgrid-scale variability of the resolved model variables is not known without further physical models. Such knowledge would be required to estimate their values at the SIP positions. We claim to use sufficiently fine spatial resolutions in our LES set-ups to keep this potential source of uncertainty small, hence we refrain from interpolating grid point values to the SIP positions. Interpolation would yield smoother fields of model variables, but would not improve the simulation results. We do not recommend applying our cloud-permitting approach using coarser spatial resolutions that compromise the resolution of a single cirrus cloud without carefully checking the effects of the unresolved variability, in particular concerning relative humidity and vertical velocity."

Discrete position of SIPs are necessary to solve the transport equation. Intra-grid cell gradients of the Eulerian fields can be disregarded for our choices of mesh resolution. Note that the simulation of the contrail-cirrus shown in Figure 1 uses a mesh size of 10 m. This is sufficient to resolve much finer scales than with bulk microphysical models; see the model comparison in Figure 1 in this document. We added a sentence after the description of the SIPMERGE operation.

Comment by Reviewer #1

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".

We changed the title to "Optimisation of the simulation particle number in a Lagrangian ice microphysical model".

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.

We include a new figure in the revised manuscript which shows the memory consumption of various simulations. From this, the real memory consumption per simulation particle is derived. Furthermore, the plot shows that a large part of the memory is occupied by the SIP data, thus highlighting the substantial of a reduction of SIP number.

EULAG-LCM is used for various applications in 2D and in 3D. Several studies treat young contrails which interact with trailing wake vortices behind aircraft where the dynamical timestep is chosen smaller than the microphysical timestep (Unterstrasser and Sölch, 2010; Unterstrasser et al., 2013). In simulations with natural cirrus formation the microphysical and nucleation timesteps are usually smaller than the dynamical timestep (as seen here as well). In all cases the simulations benefit from the newly introduced techniques, however to varying degrees. Thus we do not think that it is necessary to measure computational speed up in terms of FLOPS which is certainly a more precise measure than simply looking at the overall computational time.

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.

We changed the title, as we do not discuss parallelisation issues here.

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?

There are no such techniques used in the cited papers. See general remark 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.
The attributes (mass, shape, aggregation status, position, number of represented ice crystals,...) are already listed in section 2. As mentionned earlier, EULAG-LCM is used for a wide range of applications. Depending on the application we save information on the particle shape or origin. In the source code, the various options are switched on/off via preprocessor directives. So the exact number of attributes and SIP memory consumption can change slightly. This is why we favour the term "around 15". Together with the newly added figure, however, the memory consumption of a single SIP should be clearer.

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 the real atmosphere.
The number of bins κ we use in the Appendix is not related to the number of bins typically used in spectral models. The Appendix describes how a certain ice population (ice mass and number given, here a lognormal distribution is assumed) is initialised with a certain amount of SIPs. The two parameters κ and $\nu_{\max}$ determine the number of initialised SIPs and are not used in the Lagrangian model otherwise.

Both methods, Lagrangian and bin models have their advantages and benefits in certain applications which can be disjunct.
For example it is straightforward and memory-efficient to discriminate ice from various sources (homogeneous or heterogeneous nucleation, air traffic induced clouds) in Lagrangian approaches. One simply adds a flag variable. This extra memory need of one or two bytes is nearly negligible compared to the overall SIP memory consumption of more than 100 bytes. Thus we could run simulations where we discriminate between individual contrails in a contrail cluster, in our example up to ten different origins. Imaging that for each individual contrail a new class of prognostic variables had to be included in a bin model such analyses would not be performed with a bin model or other grid-based bulk models. Note that in a bin model the memory requirement would scale linearly with the number of origins considered. Furthermore, the computational time increases, as more prognostic variables are solved. In the Lagrangian approach, the computation time is certainly unaffected by introducing an origin flag.
Generally, Lagrangian models have advantages when large parts of the simulation domain are void of ice crystals. In these regions no memory is consumed. So Lagrangian approaches do well in setups like in section 3.1, as bin models would save the information "no ice" by storing a lot of zeros.
Thus no generally valid and fair comparison can be made between Lagrangian and bin models. However we hope that including the new figure in section 1 helps to better assess the memory consumption of our LCM model.

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.
We add tables which summarise relevant simulation parameters for all presented simulation series.

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.
We added explanations in the beginning of section 3 and section 4. Note that riming is not included as LCM is a model for pure ice clouds. Melting (called sublimation for ice clouds) is indeed included in the model. Aggregation is included in the model, however switched off in the present study.

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.
Only crystals of similar size and with identical flags are merged (see p3798, l17-l22). Identical here means that the crystals have the same origin and assumed crystal habit, e.g.
SIPMERGE is important and especially active during formation of natural cirrus. SIPs are merged during the ongoing nucleation events. At that time crystals are similar. Usually, there is no need to merge SIPs in aged mature cirrus, when habits of ice the population may have taken diverging evolutions.

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.
The experiment should illustrate how many SIPs are sufficient to resolve deposition and sedimentation. $A1_S$ uses the same parameters as A1. As splitting is applied in the $A1_S$ run it is not meaningful to list the initial SIP number as done for runs A1-A5. Nmax can also be read off from the Figure 2 (blue curve).

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).
If one is interested in evaluating total or average values it is sufficient when statistical convergence with respect to these quantities is reached. As mentionned in the final paragraph of section 3.1., evaluation of PDFs may require higher SIP number levels. Clearly statistical convergence of the examined quantities must be checked in any future application.

Spichtinger and Gierens, 2009, ACP, Part 1b uses the bulk microphysical model introduced in Spichtinger and Gierens (2009). The study by Unterstrasser and Gierens (2010) presenting the original contrail-cirrus simulation uses this bulk microphysical model. The master thesis Lainer (2012) (already mentionned in the introduction of the manuscript) compared contrail-cirrus simulations of

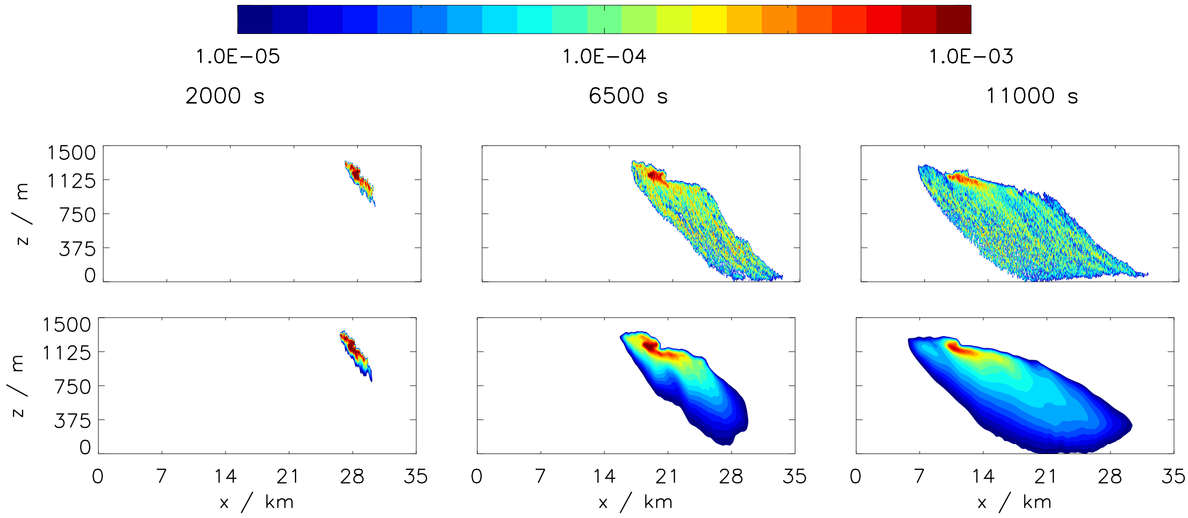

Figure 1: Top row: EULAG-LCM simulation; Bottom row: EULAG-BULK simulations. Figure is similar to Figure 1 in the manuscript except for colour bar range and few meteorological parameters.

the LCM and the bulk microphysical approach. Figure 1 (adapted from Lainer (2012)) gives an impression on what type of small-scale fluctuations can be represented with each model. The bulk results are much smoother. The study you mention and their evaluation of inhomogeneity with a bulk model is limited to coarser scales than what LCM resolves in the contrail-cirrus application. Note that in both model versions the mesh size is 10m.

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 timestep, and vertical layer thickness.

EULAG-LCM simulates pure ice clouds. Physics of warm and mixed-phase clouds are not incorporated. Thus it is not planned to simulate anvil cirrus with such high vertical velocities.
The proposed improvements (SIPMERGE and reduced/removed sensitivity to n_{min}) make it technically possible/feasible to reach convergence with respect to Δt_{NUC}. Nevertheless, in each future application convergence with respect to Δt_{NUC} must be assured again by tests. This is common to all model types (bulk, bin, Lagrangian) dealing with natural cirrus formation. The important improvement here is that the sensitivity to n_{min} (a parameter only necessary in particle-based microphysics schemes) is lowered.

If authors don't perform additional comprehensive simulations, please show readers the general dependences of required model timestep, 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.

As stated above, in each future application convergence must be assured again by tests. Future simulation setups can be diverse and it is not the aim of this study to give general guidelines for any conceivable setup.

Minor comments
1. P.3788 line#9. Instead of 'moderate number', describe specifically.
We think that the word "moderate" is specific enough for the abstract.

2. P. 3788 line#11 delete 'realistically'.
Done.

3. P. 3788 line#12 instead of 'several strategy', describe specifically
In the same sentence we list these strategies: SIP merging and splitting.

4. P. 3788 line#14 sentence 'These may well ' is lengthy in abstract.
We deleted this sentence.

5. P. 3788 line#21 interaction of) interaction among
Done.

6. P. 3788 line#25 parameterized
We use the British English version "parametrised".

7. P. 3789, line#1. Sequence from aerosols to bulk model is confusing. Is the description about bulk model is necessary?
We deleted the first of the two sentences.

8. P. 3789, line#20, Is the description about the model for warm clouds is necessary?
Yes. The description extends the review of existing particle based microphysical schemes to warm cloud physics.

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, .
It is not uncommon to use 'we' in scientific publication. It is true that the cited studies are collaborations with fellow scientists. As a synthesis of these independent studies it was discovered that SIP numbers can be reduced. This is a fundamental message of the present study and was not achieved in the cited studies. Therefore it is fair enough to say "we".

10. P. 3790, line#1, I confuse the relationships between 'statistical convergence' and 'model outcome'. Describe specifically.
We refrain from changing the paragraph as we think the phrasing is specific enough in this introductory section. **11. P. 3790, line#5, 'we' should not be used here again.**
Changed.

12. P. 3790, line#21, 'The single simulation ice particle', here define 'SIP' again in addition to the definition in abstract. 'act') 'acts'
Done.

13. P. 3791, line#5, I don't use 'unit 1'. Instead, I use the term 'dimensionless'.
Changed.

14. P. 3791, line#21 'initialized'
We use the BE version 'initialised'.

15. P. 3791, line#23, define 'RHi' here instead of line #24.
Done.

16. P. 3792, line#3, 'NSIP-dependency' on what ?
We added "of the physical results".

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.
Technically you are right that the ice crystals sublimate. However, they first have to fall into the subsaturated layer. So sublimation is a direct consequence of sedimentation. So we prefer to call this sedimentation loss in contrast to crystal loss in the main part of the contrail which is due other physical mechanism like Ostwald ripening (Unterstrasser and Gierens, 2010; Lewellen, 2012). We added some clarifying explanations in the manuscript.

18. P. 3793, line#3-4, the sentence is not clear since authors use 'on the other hand' twice.
No. It says 'on the one hand' and 'on the other hand'.

19. P. 3794, line#16-17, associated with major comment #7, figures to show change of PDF by the proposed method is necessary.
We add a new figure which shows the NSIP-dependency of a PDF of ice water content.

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.
Thanks for discovering this typo! We changed it to m^{-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.
At that point of the manuscript we would like to simply present the facts (p3795, l19 - p3796, l6). If we add explanations at that stage we fear that a reader gets lost. The response of the system to variations in Δt_{NUC} and n_{min} is more easily described once the equation (2) is introduced in section 4.3.

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?
Yes. It is described in section 2.3.5 of Sölch and Kärcher (2010).

23. P. 3797, line#15-17, There is no description about 'the monotonic function c'.
We tested several monotonic functions (linear, quadratic, square root) which had no effect on physical outcome. So we think it is enough to say that some monotonic function does the job.

24. P. 3798, line#13 already what?
We deleted 'already'.

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.

This problem is inherent to Lagrangian particle models which resolve natural cirrus formation which is not treated in other Lagrangian ice microphysics models. Thus, the observed challenges and proposed solution are novel. See also general remark 1.

26. P. 3801, line#16-17, 'However ...'. Reference is necessary to show the degree of physical uncertainties.
We added two references.

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 $\pm 5\%$ of Ntot.
Figure 1 of Yang et al. (2012) shows a comparison of model output from various ice microphysics models. The overall errors reported there are much larger than the errors here. So the slight differences in predicted number of ice crystals are definitely tolerable and it is not necessary to introduce a quantitative measure.

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).
It is true that physical results of integrated quantities are similar for cases with 2 or 100 SIPs in the case of the full 2D model simulation. 2 SIPs are clearly not enough to represent a smooth size distribution in a GB. So convergence is due to the fact that the atmospheric fields are smooth. Thus we established the term "control volume" in this respect and performed box model applications (p3793, line 17ff). In the box model sensitivity simulations, 50 SIPs was the lowest value we used and the results are practically identical to simulations with more SIPs. By the way, the box model study and its description were expanded in the revised version. In the revised version the lowest NSIP is 30-40. In the conclusion we keep the statement, that less than 50 SIPs are necessary.

29. P. 3803, line#15, why italic 'partitioning'?
We changed to normal font.

30. P. 3803, line#26-28. Please estimate the memory utilization.
A few lines above in the conclusions we state that, in the best case, the introduction of the stochastic nucleation implementation reduces the SIP number by a factor of 100. This clearly implies that also the overall memory consumption is reduced. We think that this message is clear and evident even without providing some quantitative estimates which anyway depend on the simulation setup.

Comment by Reviewer #2

General comments: This paper presents numerical techniques to optimize the computational efficiency of a Lagrangian particle tracking model EULAG-LCM. It is shown that in certain test cases, for the deposition/sublimation/sedimentation processes, the model solution already converges with a moderate number of SIPs, while a much larger number of SIPs are needed to represent the ice nucleation process. The authors also employ SIP merging/splitting to reduce the SIP number and introduce a stochastic component in the ice nucleation implementation that can reduce the numerical sensitivities to the minimum ice number represented by a SIP (nmin). Overall the manuscript is well written and the presented techniques are potentially useful for other modelers. I recommend publication of this manuscript once the authors have addressed my comments below.

Major comments:

1. As the first reviewer already pointed out, the present manuscript lacks a sufficient review of the existing techniques employed in other Lagrangian particle tracking models. Are there similar methods already being used by other Lagrangian particle tracking models?

See general remark 1 and several answers to comments of Reviewer 1.

2. All the sensitivity tests shown in figure 2 are based on the same model setup described in section 3.1. In my opinion, the results will be likely dependent on the assumptions made in this part. For example, the relative humidity (RHi) is set to a constant value. If I understood it correctly, the deposition/sublimation and ice nucleation would not be limited by available water vapour as in reality. It is totally fine for an idealised experiment, but if another RHi profile were selected or if the profile were varying, how would it affect the total SIP number, mean NGB and the overall speed-up? More discussions are suggested in this regard.

We are not sure, whether we understood the question correctly. So we have two answers:

1. Only the background relative humidity is kept constant, i.e. the synoptic scale updraught speed w_{syn} is zero. The ice crystals do interact with the water vapour field and change the humidity field. Inside the contrail-cirrus relative humidity quickly relaxes to saturation due to the high ice crystal number concentrations. Contrail-cirrus evolution is indeed limited by available water vapour. Because of this, total extinction decreases after a few hours as shown in Fig. 2. On some occasions we added the word "background" to relative humidity in the text to avoid possible misunderstandings.

2 . It is correct that section 3.1 discusses the NSIP-sensitivity only for one physical setup of the full 2D-model. Within the master thesis of M. Lainer (cited in the paper) several more setups were tested, such that we believe the presented 2D results are generally applicable. In the present manuscript however, the 2D model sensitivity series should only give a simple intuitive example. The general validity of the findings is discussed in section 3.1. Instead of presenting further 2D examples we show results of a LCM box model version which has conceptual advantages as described in p3793, l17-l26. The section from p3793, l24 to p3794, l9 presents box model studies where we tested a multitude of physical setups and found no dependence on NSIP within the tested range (50 to 200 SIPs). In the revised manuscript the box model study and its description are substantially expanded (see also general remark 2 in the beginning of this comment).

3. How is the numerical coupling between ice nucleation and other processes (e.g., deposition/sublimation, vertical diffusion, and sedimentation, etc.) handled in EULAGLCM? The coupling method will affect the numerical convergence (in time) of the model (e.g., modeled ice number). Since different models apply different numerical coupling between processes, such information will be helpful for other modelers to understand the results.

In LCM the ice phase is treated with a Lagrangian approach, i.e. ice crystals are represented by simulation particles. Other quantities are handled as Eulerian fields (water vapour, aerosol concentration and the basic meteorological variables). The Lagrangian approach uses operator splitting. The processes are computed in the following order: dissolution, nucleation, deposition, aggregation, advection. Finally, source terms for the water vapour continuity and temperature equations are calculated by summing over all SIPs in each grid box. If the nucleation timestep is smaller than the dynamical timestep, the first three processes (dissolution, nucleation, deposition) are sub-cycled. Water vapour and temperature fields are updated after each nucleation timestep. More about the coupling can be found in the original LCM description (Sölch and Kärcher, 2010).

4. I would suggest the authors to move the SIP initialization (Appendix A) part into the main text, because the information therein is essential for understanding the whole paper.
When preparing the original manuscript we were not sure whether we should at all describe the technical details of the SIP initialisation for a given analytical size distribution. As you find this part essential for understanding, we feel confirmed including it. However, we think that moving the description from the appendix to section 3.1 (only for these simulations the SIP initialisation is relevant) would distract the reader from the more significant statements in this section (namely the moderate SIP number demand for resolving deposition and sedimentation).

Minor comments:
P3790 L1: statistical converge -> statistical convergence
Thanks.

P3795 L10: How does RHi evolve in the two cases?
The evolution of Rhi for wsyn = 2cm/s is depicted in Fig.7. Clearly the initial slope is steeper if the updraught is stronger, however qualitatively similar. In the revised manuscript further simulations with more variable RH_i-evolutions are discussed.

P3797 L19-20: If the total simulation time is 6 hours, the SIP splitting will only be called around 11 times. Will a higher call frequency improve the speed-up?
In the example presented here, the application of the SIP splitting does not aim at speeding up the simulation. The splitting actually slows down the simulation, as new particles are created. Here the simulation $A1_S$ with SIP splitting serves as a reference run for the simulation A1 to A5 (see p3793, l6-l16). For the presented physical setup, splitting of SIPs is certainly not necessary. In future applications, SIPSPLIT may be necessary to fully resolve parts of the clouds with low SIP numbers. and the interval of SIPSPLIT calls may be adopted to the simulation setup. In situations with strong wind shear, the spreading is higher and SIPSPLIT may be called more often.

P3799 L7: If ensemble experiments were performed, will the random positions of new SIPs increase the spread of results from individual ensemble members?
See answer to the comment of I. Honkonen in the beginning of this reply.

P3800 L5-7: How is the nucleation rate defined? What is the concentration of ice nucleating aerosols (homogenous solution droplets and heterogeneous ice nuclei)?
EULAG-LCM is able to treat heterogeneous nucleation, but we do no consider it in this study. We use the parametrisation of Koop et al. (2000) for homogeneous nucleation. We added this information in section 4.3. Note that the highly non-linear behaviour of the nucleation rate invokes the numerical challenges we tackled here. Heterogeneous nucleation is physical less understood and parameterisations are often simpler, thus numerically not as challenging. Generally, the proposed improvements can also be used to treat future (more complex) physical parameterisations of heterogeneous nucleation.

The concentration of homogenous solution droplets is prescribed with a high number ($n_a = 100\,\mathrm{cm}^{-3}$) such that nucleation is not limited by aerosol number. We added the information in table 1 of the revised manuscript.

P3802 Conclusion: The current results are obtained from some specific model setup/simulations. Please discuss whether the conclusion can be applied to other cases, such as a case with a lot of pre-existing particles, or with strong wind shear, or with a strong perturbation of supersaturation.
As written above, strong wind shear might make it necessary to call SIPSPLIT more often. This can be easily detected by inspecting the N_{GB}-statistics.
Box model simulations (see comment above) show that the results are more generally valid.
Ice crystals forming "earlier" by heterogeneous nucleation or ice crystals falling into the region of interest can modify the RHi-evolution. We added box model studies in section 4.3 where we prescribed strong perturbations of RH_i. Probably the prescribed modifications are larger than what can be induced by pre-existing crystals. Even for these harder problems, the sensitivity to n_{min} is low, proving the robustness and general relevance of the proposed approach.

P3807 Table 1: Please check Nint values in Run A4 & A5. The current values seem wrong to me.
They are correct. A4 & A5 use the same κ as A1. A4 has less SIPs than A1, as each SIP holds more ice crystals (ν_{nuc} is higher). A5 is opposite.

P3810 Fig1: Please define x and y axes
They are defined as "x / km" and "z / km". We hope this is enough for understanding.

P3811 Fig2: I would suggest to add the legend in the figure.
P3812 Fig3: Same comment as on Fig 2.
We added a legend in Figure 2.

P3814 Fig5: Better use colored lines.
We use colours and added a legend.

References

Koop, T., Luo, B., Tsias, A., and Peter, T.: Water activity as the determinant for homogeneous ice nucleation in aqueous solutions, Nature, 406, 611–4, 2000.

Lainer, M.: Numerische Simulationen von langlebigen Kondensstreifen mit Lagrange'scher Mikrophysik (in German), Master's thesis, LMU München, Meteorologie, 2012.

Lapenta, G. and Brackbill, J. U.: Dynamic and Selective Control of the Number of Particles in Kinetic Plasma Simulations, J. Comput. Phys., 115, 213 – 227, doi: http://dx.doi.org/10.1006/jcph.1994.1188, 1994.

Lewellen, D. C.: Analytic solutions for evolving size distributions of spherical crystals or droplets undergoing diffusional growth in different regimes., J. Atmos. Sci., 69, 417–434, doi:doi:10.1175/JAS-D-11-029.1, 2012.

Paoli, R., Hélie, J., and Poinsot, T.: Contrail formation in aircraft wakes, J. Fluid Mech., 502, 361–373, 2004.

Shirgaonkar, A. and Lele, S.: Large Eddy Simulation of Early Stage Contrails: Effect of Atmospheric Properties, 44 th AIAA Aerospace Sciences Meeting and Exhibit, 0, 1–13, 2006.

Sölch, I. and Kärcher, B.: A large-eddy model for cirrus clouds with explicit aerosol and ice microphysics and Lagrangian ice particle tracking, Q. J. R. Meteorolog. Soc., 136, 2074–2093, 2010.

Spichtinger, P. and Gierens, K. M.: Modelling of cirrus clouds - Part 1a: Model description and validation, Atmos. Chem. Phys., 9, 685–706, 2009.

Unterstrasser, S. and Gierens, K.: Numerical simulations of contrail-to-cirrus transition - Part 1: An extensive parametric study, Atmos. Chem. Phys., 10, 2017–2036, 2010.

Unterstrasser, S. and Sölch, I.: Study of contrail microphysics in the vortex phase with a Lagrangian particle tracking model, Atmos. Chem. Phys., 10, 10 003–10 015, URL http://www.atmos-chem-phys.net/10/10003/2010/, 2010.

Unterstrasser, S., Paoli, R., Sölch, I., and Kühnlein, C.: Dilution and Extent of Aircraft Plumes at Cruise Conditions: Effect of Wake Vortices, Atmos. Chem. Phys. Discuss., 13, 30 039–30 096, doi:doi:10.5194/acpd-13-30039-2013, 2013.

Yang, H., Dobbie, S., Mace, G., Ross, A., and Quante, M.: GEWEX Cloud System Study (GCSS) cirrus cloud working group: Development of an observation-based case study for model evaluation, Geosci. Model Dev., 5, 829–843, 2012.