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Atmospheric
Chemistry
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Discussions

Interactive comment on "Study of contrail microphysics in the vortex phase with a Lagrangian particle tracking model" *by* S. Unterstrasser and I. Sölch

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This is the reply to the comments of reviewer #1. The comments are repeated (bold font) for your convenience. Our answers are printed with regular fonts.

General comments

This manuscript examines the evolution of ice crystals in a model contrail during the vortex phase by means of two-dimensional Large-Eddy Simulations. The main objective of the study is to analyze the sensitivity of ice crystal loss to the atmospheric conditions (essentially temperature and relative humidity)

and to the initial number and size distributions. Two different treatments of ice microphysics are considered: a bulk microphysical scheme and a Lagrangian particle tracking approach. The manuscript is largely centered on the comparison of performances of the two models and in particular their ability to predict the ice crystal loss correctly. The main outcome of the study is that the recently developed Lagrangian particle tracking method provides a better representation of sublimation which in turn allows a more accurate prediction of the fraction of surviving ice crystals. This is relevant to the initialization of the following dispersion phase where transition of contrails into cirrus clouds occurs. I think this study is worth publication in ACP as a useful improvement of previous models developed by the authors. On the other hand, for a sound assessment of the sensitivity analysis and in view of parameterizations of the vortex phase into largescale models (as mentioned by the authors in the Discussions), further analysis and refinement of some of the hypotheses underlying this study are still needed in my opinion. A part for the choice of using 2D dynamical models to represent inherently three-dimensional phenomena, I think the initial condition needs to be improved. It is very unlike that the spatial (and size) distribution of ice crystals at the end of the jet phase is uniform within the oval. Hence, questions may arise if the sensitivity of ice crystals loss to these initial conditions can be even higher than the sensitivity to the different microphysical models. I know it is difficult to answer unless the jet and vortex the phases are explicitly solved together, however a comment on this uncertainty should be added in the Conclusions.

We agree that we used a simple initial spatial distribution of the ice crystals. Thus we carried out simulations with various spatial distributions modifying the locations od the ice crystals relative to the vortex cores and the concentration levels. We studied how the extent of the secondary vortex and the micropyhsical evolution inside the primary wake is affected by these modifications. Reviewer #1 addressed the same issue (see Point 6 of his Comment) and there we give a detailed answer including several figures. Generally the issues raised by the two reviewers partly overlapped. Since the reply to reviewer #1 is longer, several issue raised by reviewer #2 are already precisely addressed there. At several places in the manuscript we now mention the uncertainties due to the simple spatial distributions and also the shortcomings of the 2D-approach.

Specific points
Before publication the authors should clarify some points on their modeling approach.

1) It should be made clear that the different behavior between the Eulerian (continuum) and Lagrangian approaches is due to the different representation (and transport) of the ice phase NOT to the modeled microphysical processes which are the same in the two cases (deposition/sublimation in the present set-up).

We added a sentence in the beginning of section 2.3 in order to clarify that both microphysical models treat the same processes (deposition, sublimation and sedimentation), and differences between the models arise due to the different representation (and transport) of the ice phase. We added:
We want to emphasise that in both models the same microphysical processes are considered. Differences in the simulation results only arise due to the different approaches to represent the ice phase and its transport.

2) The reader has also the impression that the Lagrangian particle tracking is in general superior to its Eulerian counterpart. If this comes to be true for

the present twomoments bulk model, it cannot be generalized (at least without a valid argument) to more sophisticated Eulerian approaches such as those based on discretization of par- ticle size distribution into bins. (Since these methods explicitly solve the tails of the size distribution it is likely that they handle sublimation much better than moments-based methods).

It was not intended to state that the LCM is superior to all Eulerian approaches including bin-resolved microphysics. We went over the text again to make sure we solely refer to Eulerian models equipped with a two-moment bulk model and explicitly discuss this issue in the discussion section. We added: *Gierens and Bretl (2009) discuss the sublimation parametrisation issue in two-moment bulk models. Testing the sublimation parametrisation against an analytical solution of the growth equation they found that there is no unique function relating crystal loss to mass loss. This reveals a fundamental limitation to ultimately correct a two-moment bulk model. Since sublimation is a dominant feature in the simulation of the vortex phase a Lagrangian treatment of the ice phase or size-resolved microphysics should be applied.*

Moreover in the introduction of section 3 we added: *We want to emphasise that several particular comparison results only hold for the two specific microphysical models used here and cannot be generalised to any pair of Lagrangian-based and bulk model. Especially the two-moment bulk model results depend on the choice of the sublimation parameter* α *as outlined in section 1.3.*

3) The authors correctly mention that Lagragian particle tracking methods do not suffer the large numerical dissipation of Eulerian models (unless very accurate numerical schemes are used for them, I would add). This is true for purely Lagrangian methods but in mixed Eulerian-Lagraign methods (as in the present study) caution has to be taken to generalize to situations where

the particles have a feedback on the gaseous phase via exchange of mass, momentum and energy (water vapor/ice mass exchange in the present study). The reason being that the sources/sinks in the vapor mass balance (e.g. due to condensation/evaporation) are available at the particle position; at some point they have to be redistributed at each node of the grid, which necessarily introduces diffusion in the Eulerian-Lagrangian formulation. How do you cope with this phase exchange? Which kind of spatial reconstruction (linear, Gaussian, etc) do you use ? Please specify.

The reviewer is right, that providing a consistent treatment of turbulent diffusion in a mixed Eulerian/Lagrangian approach is an important issue. To clarify our approach to cope with the phase exchange, we added to section 2.2: *Within a grid box volume, each SIP interacts with the volume-averaged grid point values of the Eulerian model variables. While the exact SIP positions are known, subgrid scale variability of the resolved model variables is not known without further physical models. We refrain from interpolating grid point values to the SIP positions, as we claim to use sufficiently fine spatial resolution. Interpolating would yield smoother fields of model variables, but would not improve the simulation results physically.*

4) I do not understand the sensitivity analysis on Np at the end of Section 2.4 (page 14648-14649). If you are trying to demonstrate the results do not depend on the number of stochastic particles (which are artificial numerical tools) you should change either SIP or the factor 1000 in the formula Np=1000*SIP, conditioned to Np=constant in each grid. This is because Np (the number density in the grid) is a physical quantity and has to be conserved.

SIP is not the name of a variable, it is just the abbreviation of simulation ice particle. It was stated that we use Np=1000 SIPs (there is no multiplication sign in the manuscript). Np is the number of SIPs in each gridbox, not the number density. So the test was performed the way you expected it to be done. We left the text unchanged.

5) The Lagragian particle tracking method has been first used in numerical simulations of contrails by Paoli et al., J. Fluid Mech., 2004 (jet phase), please add it in the references. (Although the microphysics was much simpler than that referred to in Solch-Karcher paper, in the present set-up where deposition is the only activated microphysical process, the two formulations are equivalent.)

We now mention the publication in section 1.3: *In contrail studies a mixed Eulerian/Lagrangian two phase flow approach was first used by Paoli et al. (2004), although there the jet phase was considered.*

6) Page 14654, lines 21-29. I cannot find the figure you are referring to in this para- graph.

We present detailed figures for the case RHi=120% only (Figure 5 and Figure 6). We refrained from showing similar figures for the simulations with Rhi=105%. The results of these simulations are summarised in Figure 7.

7) Why is the velocity field in Fig. 1 so wiggled ? Did you add noise to the initial Hallock-Burnham vortex profile ?

Yes. White noise was added across the total domain which is apparent in Figure 1 and is also mentioned in section 2.4.

Minor remarks

The misplaced words and typos have been fixed.

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