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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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The main subject of this manuscript, the loss of ice crystal number in the vortex phase of an aircraft contrail, is a significant one because it can affect subsequent contrail properties and development lasting for hours. It is a topic that has been discussed in several previous papers but is complex enough that significant issues remain. The starting point here is a previous study in a series of three papers by the first author and colleagues that used simplified microphysics and fluid dynamics to allow a large parameter space of contrail behavior to be simulated out to times of several hours. This, in my opinion, contributed significantly to the published literature of contrail studies.

The present work is far more limited in scope: using a more explicit microphysics

scheme, but treating only a modest number of cases and only in the vortex phase. The main conclusion reached is essentially that the previous bulk microphysics scheme was inadequate to correctly predict the crystal loss in the vortex phase, the more explicit scheme here giving significantly different results. As the authors note in the introduction, this work is not the first to use more explicit microphysics for such simulations or to demonstrate the sensitivity of crystal number loss to the sophistication of the microphysics scheme. Given the extent of the first author's previous contrail studies, however, I think it is still useful to publish this work in ACP as a correction to the previous study, provided those corrections can be clarified. The parametric studies included – sensitivity tests to temperature, relative humidity, initial ice crystal number and size distributions – are of more questionable utility: at the qualitative level only the sensitivity to initial size distribution study really covers new ground and several shortcomings in the simulations leave the quantitative accuracy of the results in doubt.

Specific comments/revisions/questions:

(1) Given the main conclusion here, the authors should provide more explicit guidance on which new results from the previous recent study (Unterstrasser et al 2008, Unterstrasser and Gierens 2010a,b) in their opinion still stand, which are cast in doubt and which should now be retracted.

(2) In the previous bulk microphysics model the crystal loss was treated with an ad hoc parameterization with some particular choice of exponent (eq. 5 here). The particular comparison results here are thus not valid for bulk models in general but just for that particular choice and this should be made clear in the abstract and conclusions. For example it is not a general feature that two-moment bulk microphysics overestimates the crystal number loss relative to size-resolved microphysics as is found here; the earlier study of Huebsch and Lewellen showed the opposite, for example, in comparing results of a different two-moment bulk microphysics with size-resolved results. It would be useful if the authors could discuss whether it might be possible to alter bulk models to give a more correct prediction of the crystal loss or whether there is some

fundamental limitation preventing such correction.

(3) There are serious issues involved in providing a consistent treatment of turbulent diffusion in a mixed Eulerian-Lagrangian model. How have the authors insured that the terms added to include turbulent dispersion to the Lagrangian treatment of ice advection (mentioned in line 25 p14646) give dispersion rates consistent with those computed for water vapor (or perhaps total water) in the Eulerian framework? Discrepancies between the two can in some formulations lead to spurious changes in ice growth/sublimation. Also, some of the differences between the "bulk" and "explicit" microphysics simulations here (e.g., the changes in total IWC) are due largely to changes in the dispersion treatment rather than the microphysics; the authors should clarify how much of the changes seen in crystal number are due to this source rather than solely microphysics as is implied.

(4) The term "large-eddy simulation" (LES) implies that the most important dynamics being considered is modeled by the Navier-Stokes equation, resolved on the chosen grid. Given that the treatment here is (a) strictly 2D and (b) the decay/diffusion of the vortices is artificially imposed rather than simulated it is not appropriate to refer to the treatment as LES. Also, given the fall of the vortices during the simulation and the influence of line vortices at large distances, the small domain size chosen (relative to the vortex spacing) may adversely affect the simulations (unless perhaps the boundary conditions –unspecified in the text – are particularly sophisticated).

(5) The early part of the vortex phase is not simulated, but rather an idealized velocity profile is prescribed, with a very crude initialization of the ice crystal spatial distribution superposed (uniform 20m radius disks surrounding the vortex cores). Simulations of the earlier phases (by several groups) particularly of the engine exhaust dispersion/roll-up does not give distributions like that shown in fig.1. How does this affect the quantitative results later on? The results shown for sensitivities to initial ice crystal number and size distribution can both be expected to be strongly influenced by this choice of spatial initialization.

(6) The results given for the "secondary wake" in several places in the paper seem on particularly poor footing given that the fraction of crystals ending up in the secondary wake will depend not only on the initial distributions but on the turbulent mixing/detrainment of the "primary wake", both crudely treated here. This implies a corresponding uncertainty on the statistics of the primary wake as well.

(7) The comparison invoked with the 3D results of Paugam et al. to bolster the detrainment results here and support the 2D treatment (line 25 p14658 and line 26, p14660) is not very effective because Paugam et al. use a quite different (but also idealized) starting point (Gaussian distributions of crystals centered on the vortices rather than uniform spots) as well as a different stratification level. Likewise in the earlier comparison with 3D results of Huebsch and Lewellen the conditions are not close to being matched so that the conclusion cited from that comparison here (line 23 p.14642) is not supported except in a very crude sense.

(8) Why do the results given in fig.7 seem discontinuous between RHi 110% and 105%? The former results lie above those from RHi = 120% and increase with decreasing r, the latter lie below and decrease. The words in the text (line 14 p14655) need to be made consistent with the situation as well.

(9) Is it purely a coincidence that, after quite different time evolutions, the BM and LCM results end at the same points for the RHi = 105% and 110% cases in fig. 2?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 14639, 2010.