Reply to referee 2 - paper acp-2009-394

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1 General comment

We would like to thank you for your comments that helped to improve and clarify the objectives of the paper. The manuscript was largely rewritten in many parts, in particular it has now been made clear in the Introduction and in the Conclusions that, although the final objective of our research is to come up with a parameterization of aircraft emissions, the present study is an exercise of feasibility of high-resolution three-dimensional LES of contrails. We also changed the title of the manuscript as "Influence of vortex dynamics and atmospheric turbulence on the early evolution of a contrail" to point out that our study covers the vortex and dissipation regimes (up to a a wake age of 30 minutes) when the driving mechanisms for the contrail are the wake vortex dynamics and the atmospheric turbulence. We removed the term "diffusion regime" that is more appropriate to the phase when the contrail transforms into cirrus and processes like radiative heating and sedimentation are effective.

We understand your concern about validation, which, as you mentioned, is a common issue in many (all) numerical simulations of contrails given the limited amount of experimental data. We agree that we did not make a validation against a specific case which would need a detailed climatology of the case. As now well stated in the Introduction, one of the objectives of the paper is to set up a numerical tool that is able to reproduce the main features of the evolution of a contrail (mainly those controlled by the dynamics). We selected background and aircraft data that correspond to typical scenarios found in the literature and tried to demonstrate that the properties of the contrail predicted by the simulations are in between the range of published values. Hence, we took your suggestion of looking for more data from observations (in addition to the particle size distributions by Schroeder et al. 2000) and included the vertical and horizontal spreadings measured by Ref. [3] using ground-based lidar for a number of contrails. The plane cuts of number density and ice crystal radius in Fig. 17 (old Fig. 14) show the vertical spreading is in the range of those observations (we cannot compare horizontal spreading because we have no shear).

Concerning your remark on the roadmap for including this simulations into global models, we clarified in Sec. 2.1 that the idea is to use LES to get raw data such as dilution time scales and other small-scale parameters needed by the model proposed by [2].

Concerning your remark on tunable parameters, the range of processes encountered in contrail formation is so wide that it is impossible to solve all of them or to run a full parametric study (the cpu cost of the complete 3D simulation chain $VD1 + D1$ was 747 hours on the $NEC - SX8$ at Meteo France). On the other hand, the main assumptions of our approach are (i) the use of monodisperse (cell-averaged) particle size distribution and (ii) the choice of the initial vortex perturbation. The first one can provide inaccurate results on late time in regions of very low number density, and so the model is being improved with the transport of higher order moments of the distribution. Regarding the second assumption, the use of a single Crow perturbation may lead to thinner secondary wake than expected (section 3.1) and this is a point that will be investigated in future study by using a full turbulent spectrum in the initialization of the vortex phase.
Otherwise, in our opinion the use of 3D simulations goes in the direction of reducing rather than increasing the uncertainty in the treatment of the vortex dynamics that is intrinsic, for example, in 2D simulations of contrails [4, 6]: these approaches necessarily neglect or parameterize important mechanisms that can be relevant to the prediction of ice crystal distribution in the wake (as now extensively explained in Sec. 3.2 of the revised manuscript).

2 Specific remarks

We fixed several errors and typos.

P20430-Sec. 1.1: We agree the discussion was confusing and we eliminated it.

P20430-Sec. 1.2: One improvement is certainly the use of high-resolution 3D simulations for 30 minutes run time. The other novel part of our study is that the atmospheric turbulence is forced during the dissipation regime and so its statistical properties are kept constant (i.e. not decaying) during the simulation.
20443: There was a mistake in the reference. The correct paper is Ref. [5].

20446 (a,b,c): Concerning the optical depth conservation, you are right that we are on different time scales and different mechanisms compared to those of Ref. [4] and [1]. In spite of this difference of scales, we argue that the conservation of τ is due to the concurrent effect of diffusion, which leads to the drop of particle number density n, and condensation, which leads to an increase of r^2 as suggested by Eq. 17. For the sake of completeness, we reported in Tab. 3 more values of τ found in the literature (including the work by Ref. [5] at a wake age of 50 seconds). Again, comparing numerical results to these data does not define a proper validation (in the sense of comparison with a controlled experiment), and if the word "validation" was used, it was badly employed.

3 other suggestions

The bimodal size distribution at the end of simulation $VD1$ reflects the enhanced condensational growth of crystals that are inside the secondary wake (second peak of the distribution): those particles start mixing with the (supersaturated) ambient air much earlier and more efficiently than those placed in the primary wake (in particular those trapped inside vortex rings)

References

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