

## ***Interactive comment on “Parametric studies of contrail ice particle formation in jet regime using one-dimensional microphysical modeling” by H.-W. Wong and R. C. Miake-Lye***

**Anonymous Referee #1**

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### General comments

This manuscript describes a parametric study of contrail formation in the jet regime. The analysis is carried out using a one-dimensional microphysical model (in fact, a box model with parameterized one-dimensional diffusion) and includes the effect of soot number concentrations; sulfur content and ambient relative humidity. The results are coherent with previous results in the literature, for example the fact that ice mainly forms by freezing of water around soot particles (at least in the range of parameters that are pertinent to present aircraft engines).

The authors may be right when they say that a systematic parametric analysis of quan-

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tities affecting contrail formation has not been done, so exploring such effects and condensing them in a single paper is certainly a useful exercise. For the same reason however, the paper leaves the reader with the impression of some lack of originality in the sense that it is not clear which are the advances –for example in terms microphysical modeling and methodology used- over the existing and well established literature (as also mentioned by the authors). Once exception is the interesting case of very high soot number concentrations that is treated at the end of the paper since it suggests a possible mitigation strategy, although the suitability of this scenario with present engine technology is not obvious at the moment.

Otherwise the manuscript is clear and well written, so I have no specific objection to publication. The authors should however respond/clarify the following points before the paper is published.

### Main remarks

1. My first concern is the absence of validation. There is no or little discussion in the paper. The authors say that experimental measures are technically problematic or impossible during the first second but what about the range 1-5 seconds? I know that the “quest” for data from in situ measurements is often frustrating but some data are available in the literature. I would suggest for example to look at Fig. 1 in the paper by Schröder et al. (J. Atmos. Sci., 57, 464). Although these data are specific to given flights, you could easily adapt the initial conditions and rerun your box model so as to give at least an argument of validation (in terms of range and shape of ice particle size distribution for example).

2. My second concern is about the treatment of mixing. Dilution is just a global quantity that serves to characterize mixing (essentially turbulent mixing). Using a one-dimensional dilution to represent all the mixing process between exhausts and ambient air in an aircraft wake is, in my opinion, a very strong approximation even for a simple coflowing jet because (i) it doesn't represent the radial gradients of concentrations and

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their effect on chemistry/microphysics and (ii) because buoyancy due to temperature and density gradient is neglected. For an exhaust jet immersed in an aircraft wake this is even more critical: in a two-engine aircraft, you can fairly represent the engine jet as a coflowing jet only for the first 4 to 5 wingspans behind the wing (1 second, start of the jet/vortex interaction zone). Afterwards, the jet is necessarily entrained by the vortex, so the flow topology and the associated mixing changes dramatically compared to coflowing jets. I think that Davidson and Wang algorithm may not be representative of mixing in the range 1 to 5 seconds. Schumann dilution law is a fit from various flight measurements, so it may capture some effects of this interaction, but again it gives a global description of mixing (peak value of inert gases) and doesn't explicitly represent mixing in the radial direction. Full three-dimensional large-eddy simulations (LES) would of course capture these affects but they are too expensive in the context of a parametric analysis of chemistry/microphysics. Other "mixed" approaches can be used: for example using precomputed trajectories from a (single) LES (that carry the information of inhomogeneity of concentrations and temperature) as input to complex chemistry/microphysics models like the one used here (see e.g. Paoli et al., *Met. Z.*, 17, 131).

I would like to see this kind of discussion on the impact of mixing on contrail formation included in the paper as one of the contributors to uncertainty (especially if you claim a possible strategy for contrail mitigation). One good place to do it is the Introduction or when commenting Fig. 1 and in the Conclusions.

Minor remarks

1. Why the mixing line is not straight in Fig. 1c when condensation is not active? Temperature end vapor should diffuse at the same speed (diffusion is essentially driven by turbulence). Is it due to chemical reactions involving H<sub>2</sub>O? In this case, it would be worth mentioning it in the caption.
2. Just a suggestion: I would split Section 3 into 2 or 3 subsections: highlighting the

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different parameters that are considered at a time (soot, sulfur, RH) could make the analysis of the results even more effective.

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Interactive comment on *Atmos. Chem. Phys. Discuss.*, 9, 22337, 2009.

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