

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

H.-W. Wong and R. C. Miake-Lye

hwwong@aerodyne.com

Received and published: 29 January 2010

We thank the referee for providing insightful and constructive comments in improving our manuscript. We have listed our responses to the comments and how the manuscript is revised accordingly point by point below.

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



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 quantities 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.

Response: We agree with the referee that our modeling methodology is largely based on existing algorithms used by the community and our parameter space is within the typical range of interest in previous measurement activities. However, we believe our modeling work is still valuable to the community for the following reasons: 1) as mentioned in the manuscript, systematic study of the parameters affecting contrail formation is still limited, and a single paper describing these effects will be a nice contribution; 2) to our knowledge, most modeling frameworks are based on classical nucleation theory to describe homogeneous nucleation of H₂SO₄-H₂O particles. Our work is among the very few modeling studies that use the kinetic quasi-unary expression developed by Yu, which gives better representation of homogeneous particle nucleation and possibly ice particle formation; 3) we have explored several parameters that were not studied before, such as the effects of initial soot size and near-field dilution profiles.

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

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

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).

Response: We thank the referee for suggesting suitable experimental data for model validation. We have performed additional microphysical calculations to study two of the cases reported in the Schröder et al. paper for model validation. We have included the results from this exercise in the revised manuscript.

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 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

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

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.

Response: We agree with the referee that the mixing in the near field is difficult to capture. This is the main reason why we performed the calculations using three different mixing profiles in the manuscript (Davidson and Wang algorithm, Schumann dilution law, and the hybrid of the two). Based on our calculations, the final state of the mixing is more important than the plume trajectories. We believe this will relieve some of the concerns regarding the mixing profiles used. We do agree, however, that the entrainment of the vortex and other turbulent mixing effects may still be a factor to cause uncertainty of the model prediction, and we have added this statement in the revised manuscript.

Minor remarks 1. Why the mixing line is not straight in Fig. 1c when condensation is not active? Temperature and vapor should diffuse at the same speed (diffusion is essentially driven by turbulence). Is it due to chemical reactions involving H₂O? In this case, it would be worth mentioning it in the caption.

Response: Since both the Davidson and Wang algorithm and the Schumann dilution law are semi-empirical methods set up to match plume properties (temperature and mixing ratio) downstream of the engine, the treatment of the transition from the potential core to the plume is imperfect, i.e., the location that the plume starts to cool is inconsistent with the location that the plume starts to mix with the ambient air. This results in the straight lines for both profiles in the high temperature (> 300K), potential core region (< 14 m downstream). We do not believe these straight lines to affect our

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



microphysics results, however, since we have demonstrated in the manuscript that the final state of mixing, rather than the plume trajectories, is the determining factor to the final properties of contrail ice particles.

2. Just a suggestion: I would split Section 3 into 2 or 3 subsections: highlighting the different parameters that are considered at a time (soot, sulfur, RH) could make the analysis of the results even more effective.

Response: We have divided the discussion section into several subsections in the revised manuscript as suggested by the referee.

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

ACPD

9, C10301–C10305,
2010

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C10305

