

Interactive comment on “Three-dimensional numerical simulations of the evolution of a contrail and its transition into a contrail-cirrus” by R. Paugam et al.

Anonymous Referee #2

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General Comments

The manuscript describes the modelled microphysical evolution of a contrail up to the diffusion regime in three dimensions. The results include the particle size and total mass evolution, which are enough to define the optical properties of the contrail. Optical depth calculations are also presented, showing plausible values. The approach of the study is to cover one representative case of the aircraft-atmosphere initial conditions, which is a good starting point that still allows contrasting the predicted mass, particle size and optical properties with measurements, unfortunately this is not included in the study and for this reason my biggest concerns about the article's publication in

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its present form are related to the validation of its results.

The reader gets the impression that the authors' strategy was to reference cherry picked values from other studies and that only some selected variables were compared. Everybody knows that, in order to produce useful output, sophisticated models, like the one used in this study, require appropriate assumptions and tuning, for which it is important to make the best use of the available measurements, especially in the validation stage. With respect to the latter, three points are mentioned in the manuscript:

1) The fact that this study's predicted particle size distributions resemble Schröder et al.'s measurements. 2) The claim that "it has been observed that the optical depth in old contrails is conserved" 3) The claim that this study's calculated maximum optical depth (~ 1) coincides with satellite retrievals.

Schröder et al.'s study, although based on a small number of measurements, gives a description of the general evolution of contrail size distribution, which makes its results very useful for comparisons with the present study. Unfortunately 2) and 3) have serious issues. In 2) no clear reference to quantitative retrievals is given, probably the authors refer to Jensen et al.'s article, but in that case they should be aware that it does not make much sense to use Jensen et al.'s baseline simulation, which corresponds to a specific measured contrail, as a benchmark for a general model of contrail evolution. With respect to 3) the referred paper (Sussmann and Gierens) does not seem to contain that piece of information.

The fact that 2) and 3) do not present solid grounds, does not invalidate the results of the study, but they leave the reader with the impression that the calculations are closer to a "thought experiment" than to a tool with the potential of producing the required parameterizations in large scale climate modelling. There are obviously a limited number of available contrail field campaigns, but I do not think it as an excuse for not attempting to contrast the study's output with available measurements. From the end user's perspective, Fig. 14 really needs validation because even if the total mass and the

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averaged size distribution were correct, if the vertical distribution of particles is wrong a parameterization of the following stages would be severely affected. Please find a way to address this, look for measurements from lidar, Doppler radar, etc.

The authors emphasise, in the introduction, the potential impact of contrails on the planet's radiative balance, and, in the conclusions, they point out the importance of providing parameterizations of the contrail-cirrus evolution to be used in GCMs; if this the final aim of the authors, two main concerns appear immediately, the first one, that given the computational expense of high-resolution 3D-models, how feasible will it be to extend their method in order to cover contrail-cirrus observed lifetimes of several hours. In other words, is there a roadmap of how their present results will evolve into a parameterization for large scale models? It is very important to include the description of other approaches or subsequent models that might achieve this. These questions are extremely relevant to the audience of an atmospheric journal.

For these reasons I think that the manuscript should be rethought in order to make its claims and its results more consistent. What I propose is:

- i) Find appropriate support from measurements to demonstrate that your results are realistic OR change the title and approach of the article to make the reader see your results being more an exercise of feasibility than a tested tool to model contrail evolution.
- ii) If you change the title of the article you might consider waiting until your capabilities to model longer contrail lifetimes improve before including the idea of “transition into a contrail-cirrus” in it.

Specific Comments

Given the large number of typos and grammatical mistakes in the manuscript I would suggest the authors in the future to ask for proofreading help, as this is not the job of the reviewers, I omit this kind of corrections in the present review.

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In section 1.1 the description and explanation of the net radiative forcing of cirrus is extremely deficient and confusing, as it implies that cirrus infrared absorption is restricted to the cases in which “large particles” are present. The authors make a very valid point when emphasizing the dependence of the ice cloud net radiative forcing on their particle size distribution, but the fact that the net radiative forcing of ice clouds during sunrise or sunset can be negative while the daily average still remains positive is not a contradiction produced by the current uncertainties related to ice cloud microphysics, as the authors try to imply.

In section 1.2 please make it clear what is the contribution of your work to previous knowledge, and which are the advantages of your method, and include this in the abstract, otherwise the reader has not idea of what has improved since Jensen et al.’s (1998) article.

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Line 3: I could not find the optical depth (~ 1) value that you refer to in the Sussmann and Gierens article. As mentioned before, this is a very serious issue because this value is implied in your introduction as one of the main confirmations of your results.

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Second paragraph:

a) Jensen et al.’s article does not claim that no extra particles can be nucleated, quite the opposite, he explains that new particles would definitely be nucleated but that he decided not to include nucleation processes after the contrail’s formation in his model setup.

b) When you point out the “agreement” between the optical depth evolution predicted by your experiments and by Jensen et al.’s, could you explain why this agreement should be expected? Jensen et al.’s experiments corresponded to one particular contrail, his experiment tried to reproduce the particular atmospheric conditions in which

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that contrail was formed, it included processes (like infrared absorption) that are not considered in your runs, whereas the approach of your experiment is exactly the opposite, to represent the evolution of a contrail in the most general way with as little atmospheric interaction as possible.

c) The average and maximum optical depths reported by Atlas et al. correspond to 7 particular contrail streaks with four of them being around or older than 1.5 hours, why did you choose these values to validate the “order of magnitude” of your calculated optical depth? Was it under the assumption that the optical depth remains constant for up to 1.5 hours? How do you then take into account in your comparison the fact that the base of the 3 youngest fall streaks shown in Atlas et al.’s Fig. 5 descended at ~ 1 m/s if your Fig 14 shows that the crystals should be smaller than 45 microns? The fact that you chose to contrast the optical depth and not the consequences of the other variables retrieved by Atlas et al. will make the reader very sceptical about your results.

Other suggestions:

I find very interesting the fact that, in Fig. 9, the last size distribution develops bimodality, is it possible to describe in the manuscript the factors that control this behaviour? I think this will enrich the article. I also find your prediction of the constant optical depth during the first half hour interesting, and it should be possible to find similar studies to that of Atlas et al. to support your conclusions or to modify your setup in order to emulate the conditions under which contrail properties have been measured.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 20429, 2009.

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