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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Numerical simulations of contrail-to-cirrus transition – Part 2: Impact of initial ice crystal number, radiation, stratification, secondary nucleation and layer depth" *by* S. Unterstrasser and K. Gierens

Anonymous Referee #2

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General Comments:

This paper builds upon the work presented in part 1. Sensitivity studies of the contrail-to-cirrus transition to initial ice crystal number, radiative heating within the contrail, atmospheric stability, layer depth of the supersaturated region and secondary nucleation are presented. A significant conclusion is that initial ice crystal number has a profound impact on the properties of the contrail throughout its evolution, suggesting that contrail mitigation may be possible through the use of jet engine modifications that lead to lower soot emissions.

This paper relies more heavily on written descriptions of model results, without accompanying figures to explain the results, than Part 1. Although the descriptions are usually good, some of the language (e.g., p. 14962, line 25, "The different sedimentation strength and intrinsic timescales become apparent in the vertical profiles of τ_{hor}.") suggests to the reader that the authors are describing figures that are included within the paper. The lack of accompanying figures can be disconcerting to the reader, and sometimes makes it harder to follow the text. This particular example is especially hard to follow as τ_{hor} does not appear to have been defined previously within part 2. Another example occurs on p. 14970, line 2: "The simulations show that...". In these examples it would be helpful to at least mention to the reader that these results are not shown in the paper so that they aren't tempted to look for non-existent figures.

Like part 1, this paper provides useful and new information regarding the factors influencing the development of contrails into cirrus, and I recommend that it be published. Some suggested revisions are included in the specific comments section below to clarify some parts of the manuscript.

Specific Comments:

As suggested in my review of part 1, when describing temperature, please change temperature (T) throughout the paper to temperature at cruise altitude (T_{ca}).

p. 14962, lines 9-15: The discussion regarding the relation between total ice mass and number of crystals is not clear. The text states that the sedimentation flux out of the contrail core region decreases with increasing number of ice crystals, because the fall speeds increase with crystal size. This implies that crystal size decreases with increasing crystal number. But the text also states that higher crystal concentrations cause a faster uptake of water vapour from entrained fresh air. Why would this faster uptake imply that the particle sizes would be smaller when crystal concentrations are higher?

p. 14965 and Figure 4: What is the radiation reference simulation mentioned in Figure

4? How does it differ from the "standard simulation with turned off radiation routine" represented by the black solid lines?

p. 14965, line 25: Wouldn't the solar zenith angle conditions be different between midlatitude winter and summer? Would changes in mean solar zenith angle between summer and winter affect any conclusions of this paper?

p. 14966 and Figure 3: What do the black lines (solid and dashed) in the top right profile in Figure 3 represent? These lines do not seem to be described in the Figure 3 caption or in the text.

p. 14967, lines 13-14: "the optical depths are higher/smaller than in the cases without radiation." How can optical depths be simultaneously higher and smaller?

p. 14967, lines 17-29, p. 14968, lines 1-3: The authors describe the unusual circumstances of one of the model simulations that lead to an exception to the rule that more radiative heating leads to higher optical thickness. Doesn't this case imply that the radiative differences between the summer and winter simulations are not important? It is clear from the description here that the authors are arguing against this possibility, but how is the reader to know this case is truly an extreme outlier? Why not run several summer versus winter simulations to get more representative values of summer versus winter values of optical thickness?

Section 6, p. 14974: It is not clear from the text why a synoptic-scale updraught and then a downdraught were added within the same simulation. Are the vertical motions an attempt to reflect the true vertical motions an evolving contrail would encounter (in other words, would most contrails typically encounter both a period of updraught or downdraught during its lifetime), or are they simply convenient for the purpose of demonstrating the effects of these motions on heterogeneous nucleation? Would the results of the simulation be any different if the contrail encountered the downdraught before the updraught?

Section 8, pp. 14978-14980: This section is somewhat repetitive of section 7. Perhaps they should be combined into one section?

Typographical errors and other minor editing points:

p. 14957, line 21: Please change "only sketch it." to "only sketch it here."

p. 14957, line 27: Change "switched only" to "switched on only".

p. 14962, line 10: Change "meachisms:" to "mechanisms:".

p. 14963, line 1: Change "less ice crystals" to "fewer ice crystals".

p. 14963, line 21: Change "an one-dimensional" to "a one-dimensional".

p. 14965, line 16: Figure 4 is referenced before Figure 2 and Figure 3.

p. 14965, line 23: The term radiation scenario is not defined in the abstract. If it must be defined for the reader here, then it probably should also be defined in the abstract.

p. 14966, line 27: Change "to weak" to "too weak".

p. 14970, line 3: Change 0.5 s^{-1} to 0.5$\times 10^{-2}$ s^{-1}.

p. 14985, Figure 2: Remove the extra sentence "The original flight level is at z=800 m." from the caption.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 14955, 2009.