

# ***Interactive comment on* “Numerical simulations of contrail-to-cirrus transition – Part 1: An extensive parametric study” by S. Unterstrasser and K. Gierens**

## **Anonymous Referee #2**

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### **General comments:**

The paper describes a study of the transition of contrails into cirrus as simulated by an LES model. The sensitivity of several contrail physical properties to changes in basic meteorological factors including the ambient relative humidity, temperature and vertical shear were investigated, and the impacts of these physical quantities on contrail evolution were characterised. The study is one of the first to simulate and to study systematically the transition of a contrail through the dispersion and dissipation stages into contrail-cirrus. Because it is a preliminary study, the authors use a somewhat simplified simulation that neglects synoptic or mesoscale vertical motions, making it easier

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to understand how the basic meteorological factors influence contrail-to-cirrus formation. This paper provides a useful study into the factors influencing the development of contrails into cirrus, and it can help to formulate contrail parameterisations in global models. Thus, I recommend that it be published. A few parts of the paper should be rewritten, however, to make it easier for the reader to follow. The suggested revisions are described in the specific comments section.

### Specific comments:

Abstract, p. 14902, lines 6-7: It would be more accurate to say that vertical wind shear has a smaller effect on "total extinction" instead of "most contrail properties".

Introduction, p. 14903, line 11: The authors suggest that contrail-cirrus might tend to have smaller optical depths than natural cirrus, but never state in the discussion or conclusions that the results of this LES model confirm or deny this hypothesis.

p. 14904, lines 24-26: The last sentence in this paragraph might be the most important sentence in this subsection because it shows how this study differs from (and is an improvement upon) previous contrail studies (which only focus on one phase of contrail evolution). The problem is the sentence is awkwardly written and hard for the reader to follow. Please rewrite this sentence to make the authors' point more clear.

p. 14908, lines 25-26: If the "optical depth" is evaluated in this model along a horizontal viewing direction, wouldn't it be better to call this quantity "optical thickness" because "depth" infers a vertical dimension?

p. 14910, lines 15-18: It may be helpful to the reader to mention here that part 2 does consider atmospheric vertical motions and their impact on environmental temperature and humidity.

p. 14911, lines 17-19: The authors mention the possibility that heterogeneous nucleation is important to contrail evolution, but do not simulate the process in any of the runs in Part 1. No further mention of this point is included within the paper. Some

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discussion of heterogeneous nucleation should be added either to the discussion or conclusion sections. At least, the authors should mention that heterogeneous nucleation is considered in part 2.

p. 14913, lines 25-26: "whereas in favourable conditions contrails" - does favourable conditions = more moist conditions?

p. 14913, lines 28-29, p. 14914, line 1, Discussion, p. 14928: The authors mention more than once in the paper that sub-visible contrails exist, they may be numerous and affect climate. However, the existence of sub-visible natural cirrus has been discussed in the literature for many years (e.g., Dessler et al., JGR, 2006, vol. 111, D08203, doi:10.1029/2005JD006586.) Some discussion of sub-visible cirrus would strengthen this paper, especially in the context of how this contrail coverage may affect the overall global distribution of sub-visible near-tropopause ice cloud coverage.

p. 14914, figure 3: Why is there no brown line in the bottom row of Figure 3? Why does the simulation with RHi = 130 (brown line) appear to end at 10000 seconds in Figure 2? Why is this truncation of the simulation not mentioned in the paper?

subsection 3.3.3 Total ice crystal number, pp. 14917-14918: The authors state that crystal loss due to "turbulent sublimation" is believed to be overestimated in their model, yet they give no reason why they believe the crystal loss is too large.

p. 14920, lines 6-10: This paragraph is nearly incomprehensible to me. Where is the "effective" entrainment rate for IWCpre defined? This rate "is about a factor of 5 lower for all times", compared to what? I find the whole discussion of the thought experiment to be difficult to follow, and it is not clear how it helps the paper. I suggest that the discussion be removed.

p.14921, lines 21-25: The authors state that relative humidity is an "equally" significant parameter (compared with shear), but then show in Figure 9 that curves of same humidity (same colour) are more closely grouped together than curves of same shear

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(same linestyle). Wouldn't this suggest that relative humidity is an even more significant parameter than shear?

p. 14923, lines 5-7: Where is the integration of E over time illustrated in the paper? To what is this sentence referring?

Captions of several figures: I assume that the temperature (T) referred to in several of the figures is the temperature at cruise altitude? If so, then change T to  $T_{ca}$ .

### **Typographical errors and other minor editing points:**

p. 14902, end of line 16: Remove the comma after "change both".

p. 14903, line 1: Please change "loose" to "lose".

p. 14903, line 8: Change "they can be formed" to "they can form".

p. 14904, line 20: It may be better to say "that can run" instead of "which allows to run".

p. 14905, line 19: Change "One simulation" to "A simulation".

p. 14906, line 2: Change "decreases" to "decreases linearly".

p. 14908, line 19: Remove the tilda from  $n(L)$ .

p. 14913, line 1: "This is also special of contrails," This clause is unclear. Do the authors mean to say that horizontally integrated extinction is especially useful for describing contrails in particular?

p. 14918, lines 22-23: "which allows to compare the results" is awkward. Please rephrase this clause.

p. 14922, line 17: Change "could be defined" to "is defined".

p. 14923, end of line 7: "In a further study, E has to be related". Has to be related? Do the authors mean "E is related"?

p. 14926, line 8: Change "see table" to "see table 3".

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p. 14926, line 12: Change  $10^{-3}$  to  $10^{-5}$ .

p. 14926, beginning of line 22: Change "of the research aircraft" to "on the research aircraft".

p. 14926, line 23: Figure 14 is presented in the text before Figure 13.

p. 14928, line 4: Change "forecast" to "forecasts".

p. 14928, line 8: "supersaturation si" Isn't "s" already being used to define shear? Perhaps a different symbol could be used to represent supersaturation?

p. 14931, end of line 25: Change "Less" to "Fewer".

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