

## Review of “Ice-supersaturation and the potential for contrail formation in a changing climate” by Irvine and Shine (ESDD, 2015)

This paper focusses on the question, how large the potential influence of projected temperature and humidity changes in the upper troposphere may be on future ice supersaturation and persistent contrail climate impact. To this end, a multi-model analysis of the respective parameters, in particular of the parameterized frequency of ice-supersaturated regions, is made from standard climate projections available from CMIP-5. Conclusions for actual aircraft induced impacts in the future must remain speculative, as the effect of projected air traffic changes is not included. This limited approach may look trivial to some, yet I think it is very helpful to understand and to assess this somewhat neglected aspect of a complex issue, viz., contrail climate impact research. The paper is well-written, honest and balanced in its conclusions, and the physical reasoning for explaining the results is well-conceived (I'm particularly fond of section 3.2!). I know of two previous studies to address a similar issue (Marquart et al., 2003; Minnis et al., 2004), of which the latter is not mentioned in this paper (perhaps because it does not address ice-supersaturation explicitly?). Yet, I encourage the authors to add a discussion (if possible) of Minnis et al.'s results, which seems possible as they also show dedicated results for mid-latitudes.

The present paper should certainly be published after a minor revision.

### I) Major comments

- The definition of a model-dependent threshold to mark actual ice-saturated regions is crucial, yet it is motivated adequately in section 2.2., and may stand as a standardisation setting for the present paper.
- While this is a very detailed comment, referring to the beginning of section 2.2, it is of general relevance. Frankly speaking, I think the term “ice-supersaturated regions” forms a clean-cut definition of a region where the air is saturated with respect to ice. Yet, in the context of this paper it is employed to indicate “regions potentially carrying persistent contrails and contrail cirrus” by adding a temperature threshold criterion. There's nothing wrong with this, the reasoning for the definition modification (p. 324, l. 1) being quite comprehensible, but you might adjust the wording in p. 323, l. 29 to avoid the formally self-contradictory definition of ice-supersaturation used now.
- There is no mentioning throughout the paper of the topically similar work of Minnis et al. (2004), who used measured humidity trends in the upper troposphere to project contrail changes. I strongly suggest to discuss the results of the present paper in context of those observation-based findings, at least in the concluding section.

### II) Minor remarks

1. p. 318, l. 24: From my point of view, contrail cirrus climate impact cannot be regarded to make a “large” contribution to anthropogenic climate change. Thus, I suggest to limit this sentence to “Because they make a substantial fraction to aircraft climate impact (e.g. Lee et al., 2009), many ...”
2. p. 319, l. 7: The authors may consider here additional references to Schumann et al. (Journal of Aircraft, 2000), who gave observational evidence for the impact of engine efficiency, and Marquart et al. (2003), who made dedicated sensitivity tests for the respective effect on contrail radiative forcing.

3. p. 320, l. 2: To emphasize the link of ISS to contrail cover, it may worthwhile to add the following text and reference: "However, the close link and comparability between ISS and potential contrail cover has been clearly demonstrated by Burkhardt et al. (2008)."
4. P. 322, l. 1: "...historical simulation simulates the present-day climate ..." sounds funny to me, perhaps change to "... historical simulation tries to reproduce the present-day climate ..."
5. p. 322, l. 28: I would like to see a reference here.
6. p. 323, l. 3: "high humidity regions"? Do I guess correctly that you are meaning "humidity at high altitudes" (or "upper tropospheric humidity")?
7. P. 323, l. 22: "Air traffic ...", please try to unravel this sentence by simplification.
8. p. 327, l. 21: I think I generally understand the general reasoning with respect to model biases in this subsection. Still, it strikes me why (e.g.) MPI-ESM-MR can reproduce closely the ERA-Interim ISS frequency in northern polar latitudes (Figs. 2a, 2e), when it captures specific humidity quite well but has a -5K cold bias in that region. To my impression this should imply extreme (relative) dryness. It may be helpful, beyond giving largely general statements, to unravel the combination of effects for this or some other appropriate example.
9. p. 329, l. 26: "... may be less significant in terms of persistent contrails ...", do you mean that some or many of the additional contrails will be too thin to increase the contrail coverage? This may be true but not for sure (see Marquart et al., their Fig. 3). Perhaps, limit the statement to "... less significant in terms of persistent contrail climate impact ...", which is fully in line with the reasoning of this paragraph.
10. p. 331, l. 6: Please, change to "Our analysis ...", as the statement doesn't hold for general ISS research.
11. p. 331, l. 19: This sentence confused me a little bit, what is meant by "other levels"? And why should the agreement between models facilitate an extrapolation of findings at one level to other levels, anyway?
12. p. 332, l. 3: If there is anything to be gained from existing publications on the GFDL-ESM2G simulations that may help to understand the strange behaviour of that model in the tropical upper troposphere, it ought to be mentioned here. If not, it would be regrettable, but not due to your fault, so leave it this way ...
13. Section 4: I see some reason to mention Fig. 3 from Marquart et al. (2003) in this concluding discussion section, because it supports a lot of expected consequences for contrail cover formulated here.

### III) References (only if absent from the paper manuscript)

- Burkhardt, U., et al., 2008, Contrail cirrus supporting areas in model and observations. *Geophys. Res. Lett.*, 35, L16808, doi:10.1029/2008GL034056.
- Minnis, P., et al, 2004: Contrails, Cirrus Trends, and Climate, *J. Clim.*, 17, 1671-1685.
- Schumann, U., et al., 2000: Experimental test of the influence of propulsion efficiency on contrail formation, Parameterisation of contrails in the UK Met Office climate model. *J. Aircraft*, 6, 1083-1087, doi: 10.2514/2.2715.