

Review of “Sensitivity of radiative properties of persistent contrails to the ice water path” by R. Rodriguez de Leon et al.

This paper forms a sensitivity study of contrail radiative forcing (RF) with respect to contrail optical properties (ice water content, geometrical thickness, effective ice crystal size, ambient temperature). Of all contrail RF studies using radiative transfer models with prescribed input it is the first that does not rely on globally uniform input parameters (according to my knowledge). Variability ranges specified for the input parameters are – in some cases – interdependent. A central assumption is that the microphysical properties of (aged) contrails agree, under equivalent ambient conditions, with those of natural cirrus clouds, for which much more measurement data are available. Hence, use can be made of the Schiller et al. (2008) data, far more extensive than the contrail observations compiled by Schumann (2002) or even those most recently reported by Voigt et al. (2011). The main result of the present paper is a considerably enhanced range of possible global radiative forcing values for line-shaped contrails, compared with recent assessments (IPCC, 2007; Lee et al., 2009), with the upper limit about twice as large as given by Lee et al. (2009).

I have no doubt about the correctness of the radiative transfer calculations that have been performed for this study. However, considering the input specified for these calculations, serious problems arise conceptionally as well as with respect to the interpretation of the results. I feel that the paper contains many open and hidden inconsistencies only some of which are addressed by the authors. This implies a high danger of misleading the reader. I fear that the paper, as it stands, is confusing the issue rather than advancing the scientific knowledge on the subject.

I think the paper should be made acceptable due to the methodical progress it forms beyond existing radiative transfer studies. It can be made acceptable by critically reflecting the bold assumptions leading to the enhanced uncertainty range for global contrail RF, which I think are not justified. This holds mostly for the upper limit of the range, but to a less extent for the lower limit, too. Or, alternatively, the respective assumptions may be explained in a more convincing way. I would also feel more comfortable, if calculations for April and October could be added to form a sounder estimate for annual mean. Generally I recommend revision of the whole text for a precise presentation along the line of my minor comments.

A) Main concerns

- While the description on pages 19930-19932 is not completely clear to me, my impression is that no attempt is made to remove total water path (ISCCP) or ice water path values that are contaminated by convective events. I say contaminated because contrails may have a similar IWP as *thin* natural cirrus, but certainly not similar to cirrus related to convection. I fail to gather from the text, whether or how the considerable effort made in Schiller et al. (2008) to separate convective from non-convective cirrus (removing part of the measurement data; relying on a fit for the median rather than the mean IWPs for the fit) has found its way into the present paper. Consequently, I suspect that the IWP values assumed for a given temperature in the present paper are high biased. Or do I misunderstand all this and are the ISCCP data only used for background clouds rather than for the contrails?

- Even more crucial for the basic assumption of the paper is the way in which the authors argue to create their maximum estimate of IWC and contrails optical depth (p. 19938, l. 12). I see no reason at all for assuming a maximum optical depth of 2.02 to create a maximum estimate of contrail radiative forcing, if the probability of this value according to existing contrail analyses from satellite observations is negligible (Palikonda et al., 2005).
- As for the IWC sensitivity, I appreciate the choice of the variability limitation by removing the highest and lowest 5% quantiles from the assessment of upper and lower limits. Even so, as the large contrail IWC variability for a given temperature is created by fluctuation of independent variables (e.g., initial ice supersaturation, vertical motion, wind shear) it is questionable to assume, for the assessment of a global maximum/minimum range, the same maximum or minimum IWC everywhere. As this extreme view is combined with equally uniform assumptions on maximum and minimum geometrical thickness, I feel that the resulting upper and lower boundary thresholds for contrail RF form substantial exaggerations.
- The present paper uses January and July calculations to create an annual mean by arithmetic averaging (p. 19936, l. 9). This almost certainly introduces a low bias and forms an awkward simplification when comparing with annual means from other studies, as contrail coverage tends to be larger during intermediate seasons than during solstice seasons (e.g., Ponater et al., 2002; Palikonda et al., 2005; Stuber and Forster, 2007; Rap et al., 2010).
- While Figures 5 and 6 are formally correct in itself, neither provides a reasonable impression on how contrail RF is expected to change by flight altitude shifting: Not Figure 5, because the dependence of cloud coverage on flight altitude is neglected, nor Figure 6 as giving a latitude dependence of RF weighted by contrail coverage masks the effect of the strong latitudinal dependence of coverage. The authors may reconsider their presentation by reflecting what additional information they like to offer beyond the much more lucid illustration by Fichter et al. (2005) of the same topic.

B) Minor remarks

1. p. 19928, l. 4: "... based on a correlation with ambient temperature derived from in situ observations ..."
2. p. 19928, l. 15: I don't think that the 0.08 and 0.32 optical depth values should be referred to as "measured in situ", they are rather parameterized from a correlation based on in-situ measurements of IWC and temperature. I have already expressed my doubts for the 0.51 and 2.02 OD values being referred to as "satellite retrieved".
3. p. 19929, l. 14: I think you may stress that the equivalence of contrail and natural cirrus IWC is holding for cirrus in non-convective areas (see above).
4. p. 19930, l. 15: Is this shape configuration similar to any of those recently tested by Markowicz and Witek (2011)? Does the choice imply the possibility of a bias?
5. p. 19931, l. 5: "... Krämer et al. (2009), who provide ..."
6. p. 19932, l. 1: I do not understand the following paragraph.
7. p. 19932, l. 23: I do not understand why the model vertical resolution should have any implication for the specification of a horizontal cloud and contrail area.

8. p. 19933, l. 7: Don't forget about the changes of thermal radiative efficiency due to seasonal changes in the temperature difference between contrail and Earth's surface!
9. p. 19933, l. 21: Rap et al. (2010) find a similar difference between all-sky and clear-sky contrail forcing as your study, in contrast to the references cited. They have used the same radiation scheme as you do, haven't they? Can an explanation be offered?
10. p. 19935, l. 20, l. 24: The choices of 200 m and 1000 m as minimum and maximum geometrical contrail thickness sounds somewhat arbitrarily when reading the text; however, in view of the statistical analysis of supersaturated layers (Spichtinger et al., 2003). resulting in a mean depth of about 500 m, the choice seems to be acceptable.
11. p. 19937, l. 2: Considering that you are using a contrail distribution produced by a model including many very thin contrails (Ponater et al., 2002; Fichter et al., 2005), it might be sensible to include into your discussion the recent work of Kärcher et al. (2010), who go to some length in determining a representative value of mean and median contrail optical depth including contrails not detectable by satellites.
12. p. 19937, l. 15: I am amazed how the authors interpret the Palikonda et al. (2005) results, whose Figure 8 indicates a large variability of contrail optical depth at least between 0.05 and 0.6 (forgetting for the moment for the even wider probability distributions in Marquart et al., 2003 and Kärcher et al., 2010). To support their 0.29 to 0.34 variability range the authors evidently use the *seasonal* variation of Palikonda et al's median optical thickness. To me this is comparing apples and oranges.
13. p. 19937, l. 22: Again I suspect that the authors are confusing optical depth variability arising von fluctuating temperatures and IWCs with its seasonal variability.
14. p. 19937, l. 23: typo "broader"
15. p. 19938, l. 12. Apart from the fact that I would like to have explained how this new fit is constructed, the general intention seems dubious to me. Why should an optical depth range of between 0.51 and 2.02 be "more consistent with satellite measurements", if these apparently indicate a variability range between 0.05 and 0.6 (see above)?
16. p. 19938, l. 21: In Ponater et al. (2002) this is the most extreme value taken from a 10 year simulation, while also the Atlas and Wang retrieval is most probably an extreme case (as the authors of the present paper themselves acknowledge by making 1 km geometrical depth their upper limit). As I have stated it is questionable to use these values for creating an upper limit for a global radiative forcing estimate.
17. p. 19939, l. 10: Here, and at some corresponding locations in the text before, I am wondering whether or where the present paper is discussion global optical depth means and global radiative forcing or Northern Hemisphere averages. Table 1 is indicating that these are global averages.
18. p. 19939, l. 13: typo "between"
19. p. 19939, l. 15: So what? Why should IPCC's best estimate fall into a range of maximum estimates? I think you ought to compare your own mean (or best estimate) with the IPCC best estimate.
20. p. 19940, l. 14ff.: The meaning of the sentence is not quite clear. If it implies that satellite and in-situ measurements of the same contrail scene are (unfortunately) missing, I do agree.

21. p. 19940, l. 20,21: I do not think that the discussed effect has be adequately separated from the temperature dependence of thermal emission in this paper to draw such a conclusion.

C) References (only if absent from the paper manuscript)

- Kärcher, B., et al., 2010, Importance of representing optical depth variability for estimates of global line-shaped contrails. PNAS, 107, 19181-19184.
- Markowicz, K. and Witek, M.,2011: Simulations of contrail optical properties and radiative forcing for various crystal shapes. J. Appl. Met. Clim., 50, 1740-1755.
- Rap, A., et al., 2010: Parameterisation of contrails in the UK Met Office climate model. J. Geophys. Res., 115, D10205.
- Schumann, U., 2002: Contrail Cirrus. In D. Lynch et al. (eds.), Cirrus, Oxford University Press, New York, U.S., 231-255.
- Spichtinger, P., et al., 2003, Ice supersaturation in the tropopause region over Lindenberg, Germany, Meteorol. Z., 143-156.
- Voigt, C. et al., 2011: Extinction and optical depth of contrails. Geophys. Res. Lett., 38, L11806.