

Review of “Simulated radiative forcing from contrails and contrail cirrus” by C.-C. Chen and A. Gettelman

Contrail cirrus is potentially the largest climate impact component from aircraft and results on the issue are yet sparse. This paper contributes another estimate and, thus, forms a valuable contribution to aviation climate impact research. While I perceive some conceptual caveats (see below), the paper should nevertheless be published in ACP after revision. The paper contains a commendable treatment of the statistical issue of identifying small radiative signals from substantial background variability. However, I find the current presentation rather descriptive, even somewhat superficial. I expect that the authors discuss their results more comprehensively in the revised version and, in particular, give more heed to a thorough and sound discussion of previous work related to their subject.

A) Main concerns

- The model is sufficiently equipped (as described in section 2) to be fit for a reasonable estimation of contrail radiative forcing. However, the focus of the paper is on effects of the diurnal cycle of aviation on contrail forcing and I feel that the authors have not fully exploited the potential of their model configuration in a diagnostic sense, as (apparently) the simulation results have been stored only every 6 hour. This excludes any discussion of lag effects between aviation density and contrail development as recently studied by Newinger and Burkhardt (2012), Graf et al. (2012), and Schumann and Graf (2013). I do not think that the simulations can be (or have to be) recalculated with a shorter storage interval, but it is regrettable. As for the effect of diurnal effects on long-term mean radiative forcing, this *can* be studied from the simulations given that the forcings for all time steps have been included in the mean be accumulation over the storage interval. I assume that this has been done, and I request that this information is added to the model description. Otherwise, in view of the distinct structure the diurnal contrail radiative forcing cycle has due to solar zenith angle effects, I fear that the capturing of aviation daily cycle effects would remain incomplete.
- The paper is rather careless (too careless, in my opinion) when citing and recalling the findings of previous papers. This item has several facets: There are citations that are simply wrong, e.g. Stordal et al., 2006 (p. 10941, l. 6), whose work does not refer to the contrail diurnal cycle at all. Nor do Stordal et al., 2005, hence it can't be explained by confusing the publication years. Other papers are cited in the wrong context, e.g. Meerkötter et al., 1999, who discuss the effect of the solar zenith angle diurnal cycle on contrail radiative forcing but not the effect of the aviation diurnal cycle. Aviation diurnal cycle effects on global mean forcing have rather been discussed by Myhre and Stordal (2001, Table 2), by Frömming et al. (2011, Table 2), and (as correctly cited) by Stuber and Forster (2007, Table 5). Whereas regional impacts of the aviation diurnal

cycle on radiative forcing have been studied by Stuber et al. (2006), Newinger and Burkhardt (2012), and Schumann and Graf (2013). Considering the dominance of the longwave contrail forcing over the shortwave one, one might also refer to the work of Dietmüller et al. (2008, Fig. 1), Myhre et al. (2009), Rap et al. (2010, Fig. 7), and Markowicz and Witek (2010).

Not to be misunderstood, there is no need to cite all these papers. However, the authors should be strict to mention the *proper* references according to the points they would like to make.

B) Minor remarks

1. p. 10940, l. 10: It cannot be understood from this abstract what is meant by “instantaneous” and “integrated”, because these terms are not common in the intended context. However (see below), my recommendation is to drop these unusual terms anyway. The authors might consider to mention in the abstract that their radiative forcing values (contrails and contrail cirrus alike) are at the low end of previous estimates. It would be very welcome to add some hint to the reason, if the authors have an idea in this respect (see below).
2. p. 10941, l. 2: “... is thought to dominate ...”, this is true for the long-term mean, but not necessarily for individual cases, see major comments.
3. p. 10941, l. 13: I think the factor 1.8 is from Minnis et al. (2004) while more recent work (Burkhardt and Kärcher, 2011, as correctly cited) tends to yield larger spreading factors.
4. p. 10942, l. 23ff: “... to be spherical ...”, “... initial diameter of 10 μm ...”, “... no overlap with background clouds ...”; all these assumptions are critical (see Markowicz and Witek, 2010; Marquart et al., 2003; Rap et al., 2010, respectively), so some discussion of the related impact on radiative forcing is necessary (also p. 10944, l. 25). I recommend to hint at the later discussion in section 3.2 already here, and to expand that discussion a little bit.
5. p. 10942, l. 25: “... fraction associated with persistent contrails is determined by assuming an empirical value for the in-cloud ice water content ...”. While this statement is similar to that given in Chen et al. (2012), I nevertheless find it confusing as no connection between contrail fraction and contrail ice content is obvious to me. Both thicker and thinner contrails may have large or small coverage. The relationship given by Schumann (2002) links ice water content and temperature, not referring to humidity or coverage. Please, give some thoughts of the idea behind this statement.
6. p. 10943, l. 21: Schumann and Graf (2013) give an even larger mean estimate and a larger range, so their results ought to be mentioned here if there is no convincing reason to the contrary.
7. p. 10943, l. 22ff: Such statistical considerations have been neglected, unfortunately, by some recent papers on the subject, so I’d like to express my appreciation of the respective concept adopted in the present paper.
8. p. 10945, l. 8: “... no uncertainty ...”, this term suggests purely deterministic results, yet we find a “level of significance” indicated in Fig. 1, hinting

- at a certain degree of background variability. I feel that some explanation is needed to help the reader distinguish “variability” from “uncertainty”.
9. p. 10946, l. 9: Because the term “instantaneous ... radiative forcing” is often used as an alternative to the more commonly used “stratospheric adjusted radiative forcing” (e.g., Hansen et al., 1997), I recommend to use another terminology here, e.g. “line-shaped contrails” vs. “contrail cirrus” like in Table 1. The terms “instantaneous” and “integrated” are not even very suggestive for the modelling concepts described in section 2.3 (in my opinion).
 10. p. 10946, l. 15: Figure 2. It is important to know (model description!) whether the displayed quantities are instantaneous values at the times given, or averages over the six hour interval preceding that time. See also major comments.
 11. p. 10947, l. 11: “... little variation”. Yet there is some variation, despite of the constant aviation density throughout the day. A similar phenomenon is obvious in, e.g., Dietmüller et al. (2008). Do you have any idea with respect to the origin of this diurnal variation of the longwave forcing component?
 12. p. 10947, l. 12: I find it interesting that the difference is similar between the daily mean and monthly mean case and the daily mean and daily varying case. You might wish to come up with a suggestion on the origin, even of speculating nature?
 13. p. 10947, l. 22: “When aircraft emissions ...”; I agree that this statement holds for those regions investigated here, which also dominate the global mean. Yet, there may be geographical regions where the number of night flights exceeds the number of day flights. That may explain that the excess of daily SWCF over monthly SWCF is stronger for the regions than for the globe (Table 1).
 14. p. 10947, l. 26: “... positive longwave forcing takes place at 18.00 UTC ...”; you mean “... positive longwave forcing *peaks* at 18.00 UTC ...”, don’t you?
 15. Table 1: I wonder why you use different terms for longwave and short-wave contrail radiative forcing in Table 1 (Δ LWRF and Δ SWRF, respectively) and Fig. 2 (FLNT and FSNT, respectively). These are the same quantities, or do I miss something here? I would also recommend (if sustained) that LWRF/SWRF is used rather than Δ SWRF/ Δ LWRF, as radiative forcing is in itself a flux *difference*. Likewise, RESTOM is an unusual name for the contrail net radiative forcing, so you might at least explain what it is indicating!?
 16. p. 10947, l. 4: Section 3.2. Although it is difficult in view of the 6-hour interval of the daily cycle display, I think that some discussion (or at least some mentioning) of the respective results of Newinger and Burkhardt (2012) and Schumann and Graf (2013) should be added. I would also encourage the authors to add some words why the contrail cirrus daily cycle is *not* displayed in the same way as in Fig. 2. Are there problems with the statistical significance? Are there any implications your results might have to contrail cirrus lifetime?
 17. p. 10949, l. 1: See above. The sensitivity of RF with the effective diameter can indeed be expected to be very strong in the range between 5 μ m and 10 μ m (Zhang et al., 1999; Marquart et al., 2003). However, I think that

your range is valid rather for young contrails than for the aged contrails that matter on the climatologic time scale, particularly for contrail cirrus (Bedka et al., 2013, Table 1). On the other hand, particle shape may induce a parametric uncertainty even larger than for particle size (Zhang et al., 1999; Markowicz and Witek, 2011).

18. p. 10949, l. 12: Several previous papers have inter-compared model results of global radiative forcing of line-shaped contrails. Your results are rather on the lower side of the range. Can anything be said about the reason? Low optical depth (Kärcher et al., 2010)? In view of your discussion of particle size effects, is your compensation of shortwave and longwave component particularly large to serve as a possible explanation?
19. p. 10949, l. 25: I would have expected some comparison with the results of Burkhardt and Kärcher (2011), Newinger and Burkhardt (2012), and Schumann and Graf (2013) here.
20. p. 10950, l. 9: Other authors have mentioned contrail optical depth as a key factor controlling the uncertainty of contrail radiative forcing (Minnis et al. 1999; Stuber and Forster, 2007; Kärcher et al., 2010; Frömming et al., 2011). Have you left out that aspect, deliberately? Do you think that ice water content and optical depth of contrails are sufficiently known at this stage?
21. p. 10950, l. 23: As I have mentioned, there are important results for contrail radiative forcing that came up after Lee et al. (2010). They should not be ignored here.
22. Reference No. 3: Kächer should be Kärcher,
23. Please, add to the caption of Figure 2 that the figure is for line-shaped contrails.
24. Some technical problems with plotting the coastlines around Greenwich in Figures 4c and 5d show up and should be removed in the final version.

C) References (if not included on the paper manuscript)

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- Stuber et al., 2006: The importance of the diurnal and annual cycle of air traffic for contrail radiative forcing, *Nature*, 441, 864-867.
- Zhang et al., 1999: Effect of crystal size spectrum and crystal shape on stratiform cirrus radiative forcing, *Atmos. Res.*, 52, 59-75.