Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2015-922-RC1, 2016

Atmospheric Chemistry and Physics
Discussions
Open Access
EGU

Interactive comment on "Simulated 2050 aviation radiative forcing" *by* C.-C. Chen and A. Gettelman

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Received and published: 30 January 2016

The paper investigates the radiation forcing (RF) from increased air traffic in the year 2050 compared to 2006 for given scenarios using a global climate/aerosol general circulation model, in a nudged mode, with a highly approximate method to represent contrail cirrus.

The study finds an over-proportional increase of positive RF from contrails. The absolute value in 2050 stays small because the model predicts a small contrail RF also for 2006. The model finds a larger negative RF from aviation sulfate aerosols on liquid clouds (assuming that fuels still contain sulfur in 2050). They state: "As a result, the net 2050 aviation radiative forcing has a cooling effect on the planet."

The potential climate impact of aviation may be important for future climate change and any new result on this attracts attention in the aviation community and related science and policy discussions. This requires a carefully formulated abstract and conclusions.

The results presented are straightforward extrapolations from Gettelman and Chen (GRL, 2013) who concluded: "Direct and (mostly) indirect effects on liquid clouds from SO4 of –46 mW m-2 are larger than the warming effect due to contrail cirrus and aviation induced cloudiness (16 mW m-2)." So, the new study differs only by using scenarios for future traffic.

The impact of traffic scenarios until about 2050 has been investigated before [Gierens et al., 1999; Marquart et al., 2003]. See also the discussions in [IPCC, 1999] in the chapters on aerosols, climate change, and technology. These studies are not cited here.

The paper does not explain why contrail RF increases by a factor of 7; see Table 3, mentioned on page 9 and the summary, without explaining the reason. The traffic increases by a factor of 4 on average and by a factor of 6 in Asia. The meteorological conditions show a warming with less contrails forming in the future. A contrail cover increase could be understood from an increase in the overall-propulsion efficiency eta [Schumann, 1996], but eta seems to be kept constant here (not clear). Higher efficiency of aviation requires more efficient propulsion. Hence eta should increase [Sausen et al., 1998]. So, what causes the factor 7?

A possible reason may be the low temperature (and possibly a cold bias) at the extratropical tropopause, possibly enhanced for the future climate. For higher and increased traffic at the tropopause more cirrus gets very cold (and the surface gets warmer) causing stronger LW contrail forcing. How do the temperatures in CAM5 compare with ERA-reanalysis results? Other possible reasons: does the atmosphere brightness temperature increase? Does the effective albedo increase? Both would increase the RF from contrails [Meerkötter et al., 1999].

The comparisons of the model results with observations and other model studies for present climate (here 2006), presented so far, are not stringent enough to allow for extrapolation into the far future without careful discussion of consequences of model uncertainties for the results. The model strengths are overemphasized and the model weaknesses partly hidden. Parts of the study are not new, and related references not sufficiently acknowledged.

The abstract reports the simulation results as if one could trust them in quantity and sign. A newcomer would read from this paper that aircraft cause a negative RF at present and in the future. The tittle of the paper is misleading, since the paper discusses only a fraction of the important aviation effects (CO2 is missing, for example). The abstract and the paper does not reflect all the uncertainties which exist in this model study.

The contrail cirrus model used does not compare well with observations. The tests shown in Chen et al (2013) all show large differences to observations.

Part of the problem comes from the highly simplified contrail model used. The method assumes that emission from aviation get spread over a grid cell (about 200 km * 200 km * 1 km) within half an hour. Thereafter they are part of normal cirrus and have the same optical and sedimentation properties. That may be "self-consistent" but is not physically correct. See the many recent contrail and contrail cirrus observations [Voigt et al., 2011; Iwabuchi et al., 2012; Bedka et al., 2013; Duda et al., 2013; Jeßberger et al., 2013; Minnis et al., 2013; Vázquez-Navarro et al., 2015] and LES [Lewellen, 2014; Unterstrasser, 2014].

Contrail cirrus is optically thicker than assumed some years ago [Marquart et al., 2003] and observations are coming back to estimates as of the 1999 IPCC [Iwabuchi et al., 2012; Kärcher and Burkhardt, 2013; Vázquez-Navarro et al., 2015].

Aircraft size or speed effects are ignored but are important [Voigt et al., 2011].

The ice particle concentration is computed independently of the soot number emissions. This is inconsistent with several observations and models [Kärcher and Yu, 2009].

The model underestimates the ice water content in 30-min old contrails [Schumann et al., 2015], possibly by 1 to 2 orders of magnitude.

The diurnal cycle of cirrus properties in the North Atlantic, discussed shortly in Chen and Gettelman (2913) and their response to a reviewer remark, is more than an order of magnitude smaller than observed [Graf et al., 2012].

There are further studies on line-shaped contrails not cited here, partially giving far larger RF [Kärcher et al., 2010; De Leon et al., 2012].

The model approach does not include heterogeneous ice nucleation effects from soot, possibly being preprocessed in contrails [Zhou and Penner, 2014].

Are there test results from CAM5 which can be used to assess the radiation transfer model and the background atmosphere properties for contrail cirrus in the modelled background atmosphere as shown in [Myhre et al., 2009] (and later studies based on this).

Some of the uncertainties were discussed in the preceding papers of the author team but are not reflected properly in this paper.

For example, the present paper cites the 2006 results, for present traffic, with 12 mW/m2. In the previous paper (ACP, 2013), it was stated as 13+-10 mW/m2. I now miss an assessment of the huge uncertainty range.

The paper mentions other contrail RF results, which are about 4 times larger (see also [Schumann et al., 2015]), but does not reflect these differences in the conclusions and the abstract.

The authors tend to show comparisons and say they show good agreement when the agreement is in in fact not good or at best marginal. For example, in their 2013 ACP paper they wrote: "CAM5 can simulate the mean relative humidity and reproduce the distribution of the frequency of ice supersaturation in the upper troposphere and lower stratosphere (UTLS) (Chen et al., 2012) as observed...” If one looks to Chen et al. (2012), one notes huge differences in the panels a) and b) of Fig. 1. The text comments the figure: “Relative humidity in CAM5-SD is about 50% higher than AIRS throughout much of the UTLS.” Later: “The frequency of ice supersaturation in CAM5-SD is also higher than in AIRS”. Nevertheless they state: “CAM5-SD does a reasonable job...” To my opinion, this conclusion is not justified.

Chen et al. (2012) find that the model results depend strongly on vertical resolution. In the present paper this irritating fact is simply ignored.

They state in Chen et al. (2012): “CAM5-L82 is found to produce cloud fraction distributions and gradients similar to MODIS but with lower magnitude (by a factor of 3).” Chen and Gettelman (2013) give an uncertainty of factor 2.5. This uncertainty is not reflected in the new paper.

With respect to sulfate aerosols: The authors say that "Aviation aerosols emitted at cruise altitude can be transported down to near Earth’s surface and thus the aerosol concentration in the lower troposphere can be substantially increased in remote regions.“

I wonder whether any increases of sulfate aerosol from aviation has been observed or is observable at all. How does this increase compare with changes in aerosol concentrations from other sources (natural and shipping etc.)? There is no observational constraint to test the model results in this respect.

Hence, the aerosol part is highly speculative and this should be admitted.

The amount of aerosol arriving in low-level clouds depends strongly on the modelling of wet scavenging and precipitation reaching the ground. This is clearly discussed in the paper by Liu et al. (GMD, 2012), on which this study is based. But the many uncertainties which were discussed by Liu et al. are not taken into account here.

It would be interesting to see a parameter study on wet scavenging parameters and show how they impact the aviation effects. The scavenging of aviation aerosol is special because of the high emission altitudes, often far above liquid or mixed-phased clouds.

Gettelman and Chen (GRL, 2013) write: "The –46 mW m-2 represents about 3% of the –1600 mW m-2 total anthropogenic SW liquid cloud indirect effects in CAM5 [Gettelman et al., 2012]". In view of recent integral climate change arguments [Stevens, 2015], the total may be a bit high and this may apply to the computed aviation effects as well.

Then, why do you insist on just 0.1% BC activation. The evidence for this specific value from airborne observations of aviation soot is zero. Why should aviation soot have any similarity to biomass burning soot? How can you exclude a few percent?

There is little observational evidence on which you can base this quantitative assumption, from which far reaching conclusions are derived.

Another parameter of importance is the lifetime of aviation soot emissions in the atmosphere at cruise levels. They get emitted at high altitudes and get scavenged slowly just because their ice nucleation efficiency is low. The long lifetime may cause small but long-lasting effects and hence balance the low nucleation effects on cirrus partly. This may increase their importance.

In conclusion, the paper needs to be revised considerably before getting acceptable: The paper should identify not only the strengths but also the major weaknesses of the model, in comparison to existing studies, acknowledge previous work, explain results physically, and formulate abstract and conclusions such that the reader is aware that the results are of qualitative nature and not quantitatively reliable.

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