

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

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Dear Dr. Schumann,

We have provided more description as noted. However, we also note that we did not provide all the details initially since all the information is available in numerous previous work, and we did not want to duplicate. We agree that some additional explanation is warranted and have provided it. We did not intend to hide anything about the model, but strike a balance between referencing previous work and providing important information in this study. We have enhanced the description along the lines that you have suggested, and we appreciate your careful review to help improve this manuscript.

General comment:

Much of the criticism concerns the approximation in the method. We understand that
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you disagree with the approach we have taken to approximate contrails in a climate model, with an approximate treatment of contrails but an explicit treatment of climate. This paper is not about the methodology used in the model. We have made an effort to better describe the uncertainties and approximations were appropriate, but we are not going to restate the model methodology. This is contained in several earlier papers. We have tried to address these points and reflect the uncertainty in the methods and how that translates into uncertainty in the results. We have added to the discussion of uncertainty. We also added a paragraph at the end of the model description section that clearly indicates this philosophy and the significant uncertainties that arise.

Here is our response to your review on our manuscript:

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 highly approximate method to represent contrail cirrus.

This method is approximate, but has been shown to be relevant and useful for climate studies.

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.

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We have tried to make sure we carefully formulate the abstract and conclusions, and think these have been improved by these comments, and those of other reviews.

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 -36 mWm⁻² are larger than the warming effect due to contrail cirrus and aviation induced cloudiness (16 mWm⁻²). So, the new study differs only by using scenarios for future traffic.

Yes, but this is important to show the impact, and the sensitivity to different emissions trajectories.

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.

Studies added.

The paper does not explain why contrail RF increase 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 η [Schumann, 1999], but η seem to be kept constant here (not clear). Higher efficiency of aviation requires more efficient propulsion. Hence η should increase [Sausen et al., 1998]. SO, what causes the factor 7?

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We have added explanation on why contrail radiative forcing in 2050 is increased by a factor of 7 while the fuelburn is only increased by a factor of 5. Two factors should be considered for this. First, the fuelburn in East Asia is projected to increase by a factor of 7.5 under the baseline scenario. The radiative forcing in this region reflects this. Due to the region being lower in latitude than Central Europe and East US, the pronounced increase of contrail cirrus in East Asia can carry more weighting in the global average. Second, an important portion of contrail ice mass is from the uptake of the ambient water vapor. Under our parameterization, the volume of fresh contrails is a function of flight distance. As the flight distance increases, as projected in the future, the water uptake is also going to increase which will add to the total ice mass of the contrails.

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 contrails [Meerkötter et al., 1999].

As noted above in the reply we have investigated this in detail. It is not due to cold bias.

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 weakness partly hidden. Parts of the study are not new, and related references not sufficiently acknowledged.

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The parts that are not new we have tried to reference (based on previous work). We have not tried to write a review of all previous work, but have added the suggested references.

The abstract reports the simulation result 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 title of the paper is misleading, since the paper discusses only a fraction of the important effects (CO₂ is missing, for example). The abstract and the paper does not reflect all the uncertainties which exist in this model study.

The title has been changed to clarify the radiative forcing we are assessing. The abstract and conclusions have been modified to better highlight the uncertainties. We do not mean to imply the model is truth.

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.

We do think the model can reproduce important aspects of the distribution of contrails as noted in previous work. All models are different than observations. But it also reproduces important aspects of observations. So the reader can assess their level of comfort. We are focusing on the self-consistent climate representation of contrails.

Part of the problem comes from the highly simplified contrail model used. The method assumes that emission from aviation gets 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

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al., 2013; Minnis et al., 2013; Vázquez-Navarro et al., 2015] and LES [Lewellen, 2014; Unterstrasser, 2014].

The reviewer is mistaken: contrails are not spread over the grid box in half an hour, as the contrail is given a small cloud fraction, representative of a hour hour of linear contrail with spreading to a few hundred meters, and added for all flights. This is detailed in Chen and Gettelman 2013. We believe this is physically correct, and the method has been published previously. The real issue with the method is spreading contrails in the vertical as the model layers are nearly 1 km thick in the UTLS. These uncertainties and sensitivity tests are detailed in earlier work as well.

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

We are not making any assumptions that are based on a optical thickness. Our work has been checked against recent observations by Minnis et al., 2013.

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

Aircraft size effects are taken care of in fuel burn. Yes, we are approximating details using this method, but this is necessary for climate purposes.

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

This is a valid point, however there is large uncertainty in the nucleation properties of soot, and the number concentration.

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

We never mention ice water content in this paper, so we are not sure the origin of the comment. It may be about earlier work. There may be confusion about the method. As detailed in Chen et al. 2012, we are putting water vapor emissions from the aircraft into a contrail, and then also moving ambient humidity above ice supersaturation into the contrail. This yields a grid-box IWC. We use an expression from Schumann (2002) as a function of temperature based on observations of contrail IWC to adjust the cloud fraction so that the contrail 'in cloud' IWC matches observations.

The diurnal cycle of cirrus properties in the North Atlantic, discussed shortly in Chen and Gettelman (2013) and their response to a reviewer remark, is more than an order of magnitude smaller than observed [Graf et al., 2012].

This is not about this paper. This is a criticism of earlier work. We are not using the diurnal cycle in this work. The Graf et al. 2012 method also has uncertainties, but this is not the place to debate them.

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

The references have been included in the manuscript.

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

We have noted the caveats to the model treatment of BC in response to this and another reviewer. As noted above, there are large uncertainties here, and disagreements

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between assumptions and laboratory experiments with ice nucleation. We have done sensitivity tests with enhanced soot activation in earlier work.

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).

The reviewer should consult the references for CAM5 contained in the model description in Section 2.1 in the paper and earlier work by Chen and Gettelman 2013. We are not going to evaluate the climate model here. However, there have been numerous studies on the development of the model cloud optics and radiation code. Gettelman et al. 2010 describes in detail the ice particle optics used. This is cited in our previous work.

Some of the uncertainties were discussed in the preceding papers of the author team but are reflected properly in this paper.

We have tried to bring appropriate uncertainties forward, but will not bring all of them forward. We cannot restate all of the uncertainties in the method, and we clearly reference the sources. Readers are free, like the reviewer, to examine those uncertainties in previous work. We understand if the reviewer does not agree with the earlier assessment, but that is not the point of 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.

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Added 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.

Again, this relates to previous work and not to this manuscript. This is a critique of earlier papers.

The authors tend to show comparisons and say they show good agreement when the agreement is 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.

The conclusions are in previous papers. We respectfully disagree with the reviewer on this point, but it does not have relevance for this manuscript.

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

The reviewer is correct. In earlier work we identified that the upper tropospheric climate of the GCM in this version is sensitive to the vertical resolution. Thus it is necessary to

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use a GCM resolution that provides a different background state. It is not the contrail parameterization that is the real problem here. We have noted this now in the text.

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.

The uncertainty is reflected in the new stated error bars.

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

The increase of sulfate aerosols in the lower troposphere due to aviation has also been reported in Barrett et al. (2010b) which is now cited in the manuscript. This is a critical uncertainty and we have more carefully identified it in the discussion in detail: there is uncertainty due to two factors that need to be better quantified. First, the model has a large perturbation to sulfate in flight corridors. Second, the cloud response to aerosols needs to be carefully quantified. The latter is a subject of considerable ongoing work. We note these uncertainties specifically now, and the magnitudes of the effects do seem large, so if anything this is a high estimate of the effects.

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

We note that this finding is highly uncertain. However, taking a state of the art climate model, and adding these aerosol distributions in the upper troposphere is justified and appropriate. The reviewer should refer to Gettelman and Chen, 2013 (GRL) for a more complete discussion. Furthermore, there is other work indicating this. We have noted that the aerosol portion needs to be addressed and think we have treated this as a model finding that needs to be considered. Abstract and conclusions have been modified.

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.

The same model is used. So these uncertainties are taken into account. Aviation sulfate will work like any other sulfate aerosols in clouds. Again, we are not pulling all uncertainties forward and redoing the work of Gettelman and Chen, 2013.

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

Agreed, but beyond the scope of this work.

Gettelman and Chen (GRL, 2013) write: "The -46 mWm^{-2} represents about 3% of the -1600 mWm^{-2} total anthropogenic SW liquid cloud indirect effects in CAM5 [Gettelman

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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.

This is referencing earlier work, and is not relevant here. In any event, the point is to say it is 3% of the indirect effects, and if these are overstated in the model, the 3% should stand.

Then, why do you insist on just 0.1% BC activation. The evidence of 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?

This is referenced in Gettelman and Chen, 2013: the value comes from laboratory measurements of ice nucleation properties of BC. We have added caveats to the discussion of aviation BC to note that the effects are highly uncertain.

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

See above. We have conducted earlier sensitivity tests of this work. The criticism is again criticism of earlier work.

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.

This effect is included in the model: low efficiency of ice nucleation for soot, but they will have a long lifetime in the upper troposphere due to a lack of scavenging.

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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.

We have tried to note the caveats, but we are not going to restate the earlier work that addresses all these uncertainties. That work is clearly referenced and described. We have tried to add more caveats and a representation of uncertainties to the abstract and the conclusions.

Regards,

Chih-Chieh Chen and Andrew Gettelman

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