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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #3

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General comments:

The article reports on global model studies of the radiative effects of aviation emissions in the time period between 2006 and 2050. The effects of contrails and aviation-induced aerosol particles are quantified. The authors discuss the individual effects of contrails and aviation-induced sulfate and black carbon aerosol in different future aviation scenarios. Since aviation shows very high growth rates it is of particularly importance to study its possible future climate effects. In this context it is quite valuable to consider the possible effects of aerosols from aviation on low-level clouds since this process has been neglected in most previous studies on the aviation impact. For these reasons, the article is of considerably high relevance for climate research, aviation industry, and environmental policy.

Since the focus of the article is the analysis of atmospheric perturbations due to specific anthropogenic emissions, including an analysis of the underlying physical and chemi-

cal process, the paper is well suited for publication in ACP. In most parts, the paper is very well written and of good technical quality. The modelling concept and the results obtained are clearly presented. Relevant literature is referenced thoroughly. Unfortunately, in-depth analysis and discussions of uncertainties are missing in some parts of the article. In addition, the number of figures appears to be somewhat unbalanced with the length of the text. I recommend publication after the following comments have been addressed by the authors.

Major comment:

As mentioned above the results of the article might be of particularly high relevance for climate research, environmental policy, and possibly also aviation industry. Decisions on future regulations of aviation emissions might be influenced by the results presented. Hence the major conclusions need to be drawn very carefully and, as a basis for robust decisions, uncertainties need to be discussed. It is of high value that the authors thoroughly consider uncertainties due to future projections of aviation emissions and background meteorology. Unfortunately, uncertainties of the major processes driving the analysed aviation effects are discussed only sparsely. More in-depth discussions would clearly improve the quality of the article. In more detail:

i) The authors mention that contrails are treated as background clouds after being initialized but refrain from discussing the possible consequences of this strong simplification. This issue could be addressed more carefully.

ii) The authors assume a comparatively low freezing fraction of aviation-induced BC typical for biomass burning smoke. A discussion on the sensitivity of the results to changes in this assumption is unfortunately missing and uncertainties due to assumptions on the freezing efficiencies of background aerosols are not discussed. More in-depth information would be helpful here. Credit should be given to the study by Zhou and Penner (JGR, 2014) who suggest large effects of aviation-induced BC on cirrus when pre-activation of these particles in contrails is assumed. It should be stressed that the uncertainty of BC effects on cirrus is still high and further research is required to increase our understanding of this effect. The current version of the manuscript suggests that the effect is definitely negligible and no further investigations are required. Much more research would be necessary to draw such a strong conclusion.

iii) The simulations reveal that cloud modifications by sulfate aerosols from aviation have a large impact on climate. Gettelman and Chen (2013) showed that this effect strongly depends on the assumed size of the emitted particles. This uncertainty needs to be discussed in the present article as well, given the large indirect effect of the particles.

Minor comments:

1. The manuscript includes many figures. This appears to be somewhat unbalanced with the length of text. Some figures could be skipped or shifted into a supplement. For example, Fig. 6 is only briefly discussed. As an alternative, the conclusions drawn could also be discussed without showing the plots and Fig. 6 could be skipped. The plots probably show numerical noise in areas where effects are small. Hence if the figure is kept in the paper, a statistical analysis could help to exclude non-significant signals (this also holds for other plots shown in the paper).

2. 80-year simulations were performed for each time slice, driven by the respective 4 years of meteorological data extracted from the CESM experiment. Due to autocorrelations, some of these (comparatively short) 4-year periods might not be representative for the climate of the respective time slice. It should be discussed in more detail why this setup has been chosen, rather than, for instance, a transient simulation. The consequences of possible biases should be analysed.

3. The authors mention the finding of Penner et al. (ACP, 2009) that aviation BC could induce an indirect forcing of -161 mW/m2 by assuming aviation BC particles to be highly efficient heterogeneous ice nuclei. However, the authors do not mention that different sensitivity simulations were performed in that study leading to very different

results. This should be discussed in the article. Also the studies by Liu et al. (2009, JGR) and Zhou and Penner (2014, JGR) should be mentioned which suggest an indirect forcing of similar magnitude, with high uncertainty.

4. It should be mentioned in the manuscript that the downward transport of particles from aviation to low altitudes has also been found in the simulations by Barret et al (2010, Environmental Science and Technology).

5. The authors focus on contrails and aerosol-induced modification of natural clouds, due to the comparatively high uncertainties of these effects. However, facing the large growth rates in aviation also other effects might experience considerable increases. To put the conclusions of the paper in the right context, the manuscript could be improved by including some rough estimates at least of the expected CO_2 effect.

6. With regard to the assumed future scenarios, the underlying assumptions on regulations or technological developments could be discussed in some more detail. This would help the reader to evaluate the reachability of specific aims of climate impact mitigation, as considered in the scenarios. If these aspects are discussed somewhere else in the literature, corresponding citations would be sufficient.

7. Page 4, lines 22-23, 'BC and sulfate aerosols are internally mixed in the modes ...': It should be specified which modes are meant here, the aged modes only or the primary carbon mode as well.

8. The simulations are based on RCPs 4.5 and 8.5. It should be explained in the manuscript why these scenarios have been chosen and why RCPs 2.6 and 6.0 are not considered.

9. Fig. 3a: The authors should specify in the manuscript whether the 'different background meteorology' (present-day, RCPs 4.5 and 8.5) also implies corresponding differences in background emissions.

10. Page 14, 'inclusion of aviation aerosols is found to further increase IWP (Fig. 9c

and d)': The authors should discuss the plausibility of this effect. Is this an effect of BC or sulfate? Effects of sulfate aerosols on cirrus via homogeneous freezing are not very probable since this process is usually not limited by aerosol number. Other mechanisms could be diabatic effects due to aerosol-radiation interactions or effects on mixed phase clouds. Are the simulations capable to identify possible reasons?

11. Section 3.5, Aviation BC: As discussed, for instance, by Zhou and Penner (2014) the effects of aviation BC probably depend on the assumptions on ice nucleation efficiency of background ice nuclei as mineral dust or BC from non-aviation sources (see also major comment). A possible mechanism causing the positive radiative forcing of aviation BC simulated in the present study could be that the number of ice nuclei increases, resulting in more but smaller heterogeneously forms ice crystals. Sedimentation and corresponding thinning of cirrus would be reduced resulting in a net warming. The strength of this effect would, however, depend on the amount of ice nuclei in the background. This should be admitted in the paper.

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