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Interactive comment on "Coupled Chemistry-Climate Effects from 2050 Projected Aviation Emissions" *by* Andrew Gettelman et al.

Anonymous Referee #2

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The paper presents results from two climate-chemistry-ocean models on the climate impact of future aviation. In particular the paper discusses the climate impact of aviation emissions on climate temperature changes.

Certainly, the topics are interesting and important and deserve careful investigations. An ocean-atmosphere coupled model with a high quality model simulating the fate of the aircraft emissions is essential for computing aviation climate change.

Unfortunately, the paper is insufficient in many respects. The material presented, though not irrelevant, does not add enough new insight and results to the existing literature. This topic deserves a far deeper investigation and a technically better paper.

The paper reports quantitative values of the radiative forcing (RF) values for various aviation emissions and effects. The majority of these results are taken from earlier publications and given here with little or no discussion on the ranges of validity and discussions on the differences to results from other studies. Additions concern specific scenarios.

Then the study reports various “nonlinear effects” from BC, including strong solar radiation absorption by BC from engine exhaust in cirrus, sulfur brightening of low level clouds, regional disturbances, advection effects, and surface temperature response.

The simulations were performed with two different models. Here too, a large part of the results was published earlier (in the various cited papers by Jacobson, Chen and Gettelman). In fact, the paper has strong overlap with Brasseur et al. (2016), Jacobson et al. (2013), and Chen and Gettelman (2016), even repeating some of the numerical values in the tables and one of the already published figures. The two models disagree in many respects, and the discussion mentions possible reasons, but the discussion remains qualitative and does not present new convincing evidence explaining the differences clearly.

Not surprisingly, the authors did not find statistically reliable results in this respect. This was to be expected, as discussed in other studies, because the disturbances are small compared to the inherent climate noise. For this reason, other authors either use enhanced disturbances or follow the idea of climate response models, which are quasi linear in the disturbances, with model parameters (inertia or heat capacities and climate sensitivities) fitted to full climate model studies with enhanced disturbances. The possible inaccuracies of such approaches because of inherent nonlinearities are unavoidable. It would have been interesting to see how linear or nonlinear the model responses are (see Rind et al., 2000).

As the authors mention themselves, none of the climate change results on regional temperature and ozone changes and surface temperature changes is strictly statistically significant. Even the global mean surface temperature changes remain in the statistical noise. (This seems to revise some earlier conclusions form the same data;

e.g., Jacobson et a. 2013).

The patterns of the simulation results do not look convincing. There is little systematic pattern in the mean responses. Many of the results just look random. Fig. 2b and 4 are examples.

I am not sure, whether the significance tests are reliable because based on local tests; see Wilks, D. S. (2016), "The stippling shows statistically significant grid points": How research results are routinely overstated and overinterpreted, and what to do about it, Bull. Amer. Meteorol. Soc., 97, 2263-2273, doi: 10.1175/BAMS-D-15-00267.1. This should be discussed.

The suitability of the models for this study is not sufficiently justified. Since a global model with very coarse horizontal resolution (4 $\times$ 5 degrees) cannot resolve plume dispersion of NOx and neither line-shaped contrail formation nor their merging into contrail cirrus, the results rely highly on the validity of the subgrid scale (SGS) models used. The present method gives no details on these SGS models but refers to previous publications. When looking to some of the previous publications one finds a lot of ad-hoc simplifications. I agree that simplifications are unavoidable, but I see a lack of principle justification (e.g., on plume cross-sections), reflection of recent insight and data, and lack of validation of the SGS models with the observations that are now available from various studies. The paper does not discuss the degree of agreement or disagreement with related contrail studies, e.g., in respect to contrail ice water content, optical depth, life times, cross-sections etc.. Hence it is unclear of how good the SGS models are and how much the results change when the SGS model is changed.

The assumption that the radiative properties of contrail cirrus are the same as those of normal cirrus clouds is highly questionable and has been overcome in other studies. We know since Minnis et al. (1998) that contrails remain observable at ages larger 10 h. Many further measurement results have been presented on this since then. Many findings indicate that aged contrails differ significantly from other cirrus. In particular they often contain higher concentrations of small ice particles, with impact on optical properties, sedimentation and life times.

A prerequisite for contrail cirrus simulations is the suitability of the simulations of ice supersaturation (and temperature). See Irvine, E. A., and K. P. Shine (2015), Ice supersaturation and the potential for contrail formation in a changing climate, Earth Syst. Dynam., 7, 555–568, doi: 10.5194/esd-6-555-2015. It would be important to show how good the present models resolve temperature and ice supersaturation along aircraft flight tracks, e.g., compared to qualified reanalyzes or to in-situ measurements.

With respect to NOX and O3, I miss a discussion of the dispersion of NOx etc. from aircraft engines to grid scale which is known to cause nonlinear O3 changes since early studies in the 1990's, depending on the dilution model assumed. See, e.g., Paoli, R., D. Cariolle, and R. Sausen (2011), Review of effective emissions modeling and computation, Geosci. Model Dev., 4, 643-667, doi: 10.5194/gmd-4-643-2011.

The question whether aviation NOx emissions cause a positive or negative or zero RF is still under debate. See Pitari et al. (2016), Radiative forcing from aircraft emissions of NOx: model calculations with CH4 surface flux boundary condition, Meteorol. Z., 23, doi: 10.1127/metz/2016/0776. The CH4 surface boundary condition seems to matter. The present study uses prescribed CH4 at the surface which likely has consequences for the results.

Work from other teams is hardly mentioned. Differences in the results between this study and other studies (e.g. for NOx induced O3 and CH4 changes or contrail RF) are neither mentioned nor discussed.

As mentioned in a comment by M. Ponater, the studies by Olivié et al. (2012) and Huszar et al. (2013) are related to this work, and should have been discussed.

The results and conclusions are not always clearly presented. One example: The authors relate (in the abstract and the conclusions) the non-local surface temperature signal from local radiative forcing to advection. The assumption that advection is the reason for nonlocal behavior is reasonable and not fully new (Ponater et al, Ann Geophys., 1996; Shindell and Faluvegi: Climate response to regional radiative forcing during the twentieth century, Nature Geosci., 2, 294-300, doi: 10.1038/NGEO473, 2009; see also Rind et al., 2000). However, this paper does not bring any new argument to support this conclusion in this paper, except that the (noisy) temperature patterns exhibits downstream shifts. Is that worth mentioning as a new finding in the abstract? I think, this needs more analysis.

The authors claim that heat absorption by BC from aviation is large enough to cause strong warming in contrails. They refer to Liou et al. (2013) in this respect. There is no doubt that BC does change absorption when present in sufficient amount. The quantitative results depend on the assumed BC mass (and effective sizes) of the soot and the mass of ice particles (and their sizes) and the fraction of ice particles containing soot particles. It would be important to check the mass budget of the BC in ice particles and see if this mass budget is consistent with the aviation BC emissions, the lifetime of cirrus and aerosol sinks. For example one could analyze from the model results the total ice mass in the cirrus clouds regionally or globally, convert that to cross-sections, and could compare this with the total BC mass from aviation in the same clouds. I would not be surprised if the mass or area fraction of BC turns out to be by far smaller than that for cirrus ice.

In spite of parallel studies by Righi et al. and Gettelman et al., I am not convinced that aviation sulfur emissions change low level clouds in any significant manner. I miss a careful and critical discussion of the amount and concentrations of cloud condensation (CCN) particles of reasonable sizes which could be contributed by aviation compared to the many other sources for CCN. Again, one could compute from the model results related statistics. To my understanding, most of the CCN in stratus clouds come from non-sulfur sources. I do not know of any single measurement showing that aircraft could indeed change water clouds by sulfur emissions. So, to me, this effect appears

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Discussion paper

to be purely hypothetical.

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