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Comment on acp-2021-329

Andrew Gettelman (Referee)

Referee comment on "Exploring the uncertainties in the aviation soot–cirrus effect" by Mattia Righi et al., Atmos. Chem. Phys. Discuss.,
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Review of "Exploring the uncertainties in the aviation soot-cirrus effect" by Mattia Righi et al.
Review by Andrew Gettelman

This manuscript performs an in-depth investigation of the effect of aviation soot on aviation radiative forcing. The topic is an important uncertainty in aviation climate effects and is well suited for ACP. The manuscript is well written and should be publishable in ACP subject to some significant revisions and clarifications.

General comments:

1. The manuscript could do a bit better at highlighting the importance of the model base state of clouds for the soot effect, and the balance of homogeneous v. heterogeneous ice nucleation. This seems to be a critical factor. Maybe the EMAC-MADE3 values could be compared to other previous work (e.g. similar to figures 4 and 5 in Gettelman et al 2012)

Gettelman, A., X. Liu, D. Barahona, U. Lohmann, and C. C. Chen. 2012. "Climate Impacts of Ice Nucleation." Journal of Geophysical Research 117 (D20201). https://doi.org/10.1029/2012JD017950.

2. It would be nice to dive into the microphysics a bit. Could you perhaps show sensitivity plots for Figure 3 (especially zonal mean mass and number concentrations) and maybe ice crystal number and mass? This would be very helpful at understanding radiative forcing.

For vertical velocity perturbations this is all buried in Figure S7, but would probably be more useful to try to put in the main text for this as well as for the different Script and f values. Maybe the relevant properties from figure S7 for each case could be reduced to a zonal mean perturbation, and then the line plot would enable all sensitivity studies to be put on one plot. Clearsky ant total water are probably not that relevant. That would leave a 6 panel figure with all the relevant information. One for W perturbations and one for the Scrit and F perturbations.

3. In addition, can you state what the contrail RF is for just H2O without aerosol perturbations? This would be a useful baseline to compare to other work (e.g., estimates in Lee et al 2021). Maybe this is in Righi et al 2020, but please state it here and note how it is similar (or not) to other work.

Specific Comments:

Page 1, L24: 'milden' is not an English word. Maybe 'reduce'

Also note the pandemic did temporarily reduce the increase. See Gettelman et al 2021.

Gettelman, Andrew, Chieh-Chieh Chen, and Charles G. Bardeen. 2021. "The Climate Impact of COVID-19-Induced Contrail Changes." Atmospheric Chemistry and Physics 21 (12): 9405–16. https://doi.org/10.5194/acp-21-9405-2021.

Page 2, L59: no statistically significant effects were reported by Gettelman…

Page 4, L119: I don't think you mentioned this important point (or at least not sufficiently) in the introduction: that the model cloud state (humidity and water content and homo or heterogeneous nucleation dominance) may determine the response to soot as well. This has been discussed in the literature before, and should be highlighted in the introduction. See general comment above

Page 7, L178: what do these gamma values mean? Please state what autoconversion scheme is being used. Is the minimum CDNC for the autoconversion scheme or independent?

Page 7, L195: aviation soot = BCtag?

Page 9, L230: is there a timescale for the nudging or is it replacement of the model dynamical fields?

Page 14, L289: What is the overall contrail RF with and without soot (I.e. just aviation H2O)? That helps put this study in context of other work. See general comment.

Page 14, L315: I'm not quite following this logic. How does enhanced cloud lifetime mesh with reduced cloud fraction?

Page 16, L352: how does this compare to the total contrail RF?

Page 17, L363: order of tens of mWm-2

Page 18, L377: seems like the activation efficiency is key for disparity across studies. Can you intuit anything about model dynamics and cloud environment?

Page 19, L407: pair of assumptions for the ice nucleation….

Page 20, L416: so the effect jumps around with sub grid W? That seems a bit worrisome. You explanation is plausible however that the effect changes sign of sensitivity with W.

Page 23, L510: maybe you could plot homogenous IN faction? This may be a good model metric for comparison?

Can you comment any more here about the impact of the model base state on response to aircraft soot? Seems like you can draw some significant conclusions, basing the homo v. Hetero balance on your variation of w and Scrit?