

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2022-222

Anonymous Referee #1

Referee comment on "Impact of present and future aircraft NO_x and aerosol emissions on atmospheric composition and associated direct radiative forcing of climate" by Etienne Terrenoire et al., Atmos. Chem. Phys. Discuss.,
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Review of "Impacts of present and future aircraft NO_x and aerosol emissions on atmospheric concentrations and associated radiative forcing of climate" by Terrenoire et al.

This paper studies the impact of present-day and a range of projected aviation NO_x and aerosol emissions on atmospheric composition and climate. Aviation NO_x has been the focus of numerous studies over the past decades, but here the authors include less well studied contributions from nitrate and sulfate and applies an updated and more refined, including the addition of interactive stratospheric chemistry, version of the LMDZ-INCA global model than previously used.

The results reconfirm previous findings and are in overall agreement with older comparable studies when it comes to features of the NO_x-induced perturbations to ozone and methane concentrations. A key new finding is that accounting for indirect effects on nitrate and sulfate aerosols, results in a switch from net positive to net negative aviation NO_x forcing. The sign of the net NO_x forcing is also found to be highly sensitive to background conditions, with implications for future scenarios. Given that the most recent comprehensive assessment of aviation climate impact placed NO_x as the third largest warming contribution, these are important findings. The sensitivity of the net NO_x climate effect to these factors, combined with the large uncertainties surrounding atmospheric aerosol in general, shows that further efforts are required from the scientific community.

The paper is very well written and organized, albeit a bit long and detailed at times. The future scenarios used are old and may not capture more recent formulations of the ICAO goals and the Paris Agreement, which is an issue for the relevance of the results in the broader context – however, this is also understandable given the lack of up-to-date aviation emission scenarios and fine within the more process-focused scope of this paper.

Overall, I consider the paper to be an important contribution to the literature and have mostly minor comments for clarity and readability that should be considered before publication.

General comment:

Both the abstract and Summary and conclusion sections are quite long and detailed, which makes it a bit challenging to extract the key findings and implications. I would encourage the authors to look for possibilities to reduce the level of detail in the abstract, which reads almost like a full summary on its own, as well as eliminate some of repetition that seems to be between the summary and previous sections (or alternatively remove from results section for a more condensed summary of course).

The study includes a large number of experiments; while described in the methods section, I think it could be helpful for the reader to have a table that summarizes them.

Minor comments:

Line 130: Lund et al. 2017 <https://doi.org/10.5194/esd-8-547-2017> also included nitrate aerosols

Line 147: missing an "us"?

Line 215: Because the paper repeats the importance of the emission altitude and to be able to better study the zonal concentration changes, it would be useful with the figure (in the SI) showing the zonal mean, vertical emission distribution.

Lines 228-235: could be worth a quick mention of the evolution of the sector in the past 10-12 years (as assessed by Lee et al. 2021) since these emission inventories are older – for context. Also how, do the CO₂ emissions for 2006 compare with those in Lee et al.? I seem to recall that CEDS emissions are a bit lower than that study in 2018, also the case for 2006?

Lines 294: the study by Ocko et al. <https://acp.copernicus.org/articles/19/14949/2019/>

presented some scenarios that were aligned with ICOA goals and policies – worth mentioning comparing here, or in the discussion?

Line 380: given that one of the main novel contributions in this work is the inclusion of aerosols, I suggest the authors also add a brief summary of what has been found regarding model performance for sulfate/nitrate, rather than just referring to other studies.

Line 390: typo

Line 390: can you please specify the latitude bands, unclear what high latitudes are

Line 405: mix of southern and northern hemisphere stations as I understand it – are these three stations the only ones located in the southern hemisphere high lats?

Line 470: maybe mention findings about non-linearities in the scaled small perturbation vs 100% removal approach?

Line 487: what about the approach to derive methane forcing? Describe here?

Line 603: to get a better feel for the magnitude of the changes, it would be useful to express some of this in terms of percentage of the baseline concentration...

Line 620-629: while the sulfate change is pronounced towards the high latitudes, the nitrate change is focused much further south – can the authors provide a brief explanation? Meteorological factors?

Lin 764-782: what would be the potential implications of allowing climate to change, instead of fixing meteorological conditions? Of significance?

Line 897-898: I feel that there is a bit of a mismatch between one of the novelties of this paper and the only two-line mention of this here. For instance, in light of the large uncertainties surrounding aerosols RF, how confident are you in the net response?

Line 910: is table S4 the correct reference here or am I misunderstanding the sentences?

Line 1013: typo

Line 1055: given the uncertainties surrounding the ERF/RF conversion factors, why not include a comparison with the RF numbers provided by Lee et al. (as well).