Second round review response to 'Impact of future aircraft NOx emissions on atmospheric composition and climate: dependence on background conditions'

Reviewer 1

Colours: 1st round review - 1st round response - 2nd round review comments - 2nd round response

This paper provides a worthwhile addition to the existing literature concerning the modelled effects of aviation NOx pollution, which has high practical and policy relevance given the likely evolution of air transport and foreseeable attempts to mitigate climate impacts by altering flight profiles. This paper provides a useful cautionary lesson that strong variability exists between different atmospheric models, limiting the usefulness of single-model studies, so encouraging care in the use of their results. I recommend publication subject to some clarifications and corrections as listed below.

Thank you for your detailed reading and thoughtful comments that have enabled us to improve and clarify the manuscript following your suggestions.

Great, thanks. I'm re-reviewing changes against my comments only, I have not re-read the entire manuscript again from scratch but trust that reviewer 2 will check their changes similarly. I've highlighted my responses in bold where I think some action, or at least checking, is still required.

Thanks - have pasted below only those comments in bold where action was needed (rest of comments were resolved in first round).

L140 "change in methane lifetime from each simulation ($\Delta\tau$ CH4)" and L150 (equation 1): having looked at this myself recently, to make it clear I think it needs to be the _relative_ change in CH4 lifetime, a unitless quantity. Otherwise the units don't balance in equation 1. It goes back to Holmes (2011) who says "_relative_ change in CH4 lifetime" (my emphasis). If the calculations were done with an absolute change (say in units of years), it wouldn't work.

Yes indeed dTCH4 is the relative change in methane lifetime (%), have clarified in the text 'based on the modelled relative change in methane lifetime (%)'

Sorry to be super-picky, but I wouldn't specify percent, because we don't want anyone to be confused with a possible factor of 100; it's just a unitless proportion

Deleted (%)

L185 "The models show two grouped responses: a stronger ozone response at a higher altitude (LMDZ-INCA, MOZART3, ~200hPa), or a weaker, lower peak ozone response (EMAC and OsloCTM3, ~300hPa, see Fig 2b)." Actually zooming in on Fig 2b I'd say all three of

LMDZ-INCA, MOZART3 and EMAC peak at 220-250 hPa; it is only OsloCTM3 that peaks at around 340 hPa, with a notably broader altitude distribution.

Have updated to correct this: ‘The models show varied responses: a stronger ozone response in LMDZ-INCA and MOZART3, and a weaker response in EMAC and OsloCTM3 (Fig 2b). OsloCTM3 also has a lower and broader peak (~340 hPa) than the other three models (which peak around 220-250hPa.’

OK thanks except new typo thing: should have space before units in "250hPa"

Added space

L253 "The models all show lower ozone concentrations in the northern mid latitude upper troposphere... under an SSP1 background..." I think it's more complicated than that because we have two deltas going on. Firstly we have deltaO3 = O3 (normal NOx emissions) - O3 (20% reduced emissions), which is positive. Then (say for LMDZ-INCA in the UT) we have an increased deltaO3 (more red) for SSP1 background minus the same thing for SSP3 background. So that's like a deltadeltaO3! So when you finally say "show lower ozone concentrations" that sounds like a simple change in concentration (e.g. from 200 to 150 ppbv), but actually it's a change in sensitivity (how much O3 concentration changes with NOx). So maybe Fig 4a actually tells me something more like "The models all show enhanced ozone production from aviation NOx under an SSP1 (vs SSP3) background in the UT, and reduced production in the LS under SSP1 (vs SSP3) background".

Agreed, some more care needed describing the double difference - have updated as suggested ‘The models all show reduced ozone production from aviation NOx under an SSP1 (vs SSP3) background in the northern mid latitude upper troposphere, and enhanced ozone in the LS under SSP1 (vs SSP3) background (Fig 4a).The relative magnitude of these changes varies between models, giving a different net response.’

Nearly there but one of us has got the polarity wrong! I reckon (clearest in LMDZ-INCA) that the ozone production is increased (enhanced) in the UT and reduced (diminished) in the LS.

Double checked this, plotted as SSP1 - SSP3 so red (LS) = enhanced ozone in SSP1 vs SSP3 and blue (UT more pronounced in MOZART and OsloCTM) = reduced ozone in SSP1 vs SSP3.

L343 "Unger et al. 2013)" spurious parenthesis

Fixed, added bracket

Obviously not a biggie, but this one (missing parenthesis I should have said) not actually fixed in tracked changes/revised version

Fixed

L352 the "m-2" is currently broken across a line at the end, maybe meaning it is the wrong sort of dash/minus sign?

Fixed, changed to correct minus sign

OK but my apologies, missed this last time, earlier in the same sentence you have the units "mWm-2" without a space or dot after the "mW", inconsistent with elsewhere. Hardly worth mentioning!

Fixed this - found a couple of occurences

Reviewer 2

Colours: 2nd round review - 2nd round response

General comments

Thanks for the revised manuscript and I apologize for taking so long to look at it. The revisions have improved the paper, but I do still have some suggestions for further clarification (I think these are all fairly straightforward) – see specific comments below. If these can be responded to, then I am happy to recommend the paper is accepted.

Thanks for your comments and for suggestions for further clarification - these have been addressed below.

Specific comments

Table 2/L148: I suggest you include the prescribed CH_4 lower boundary condition mixing ratios for each simulation in the table. The values for the two SSP scenarios in 2050 are in the text, but I didn't find the value used for the present-day simulations. Also – is this a single global mean value (I assume so), or do you apply some latitude dependence (e.g., as observed)?

In this paper, we only show results for the RF calculations for the future scenarios, and not for the present day, which are presented in Cohen et al. (2026), and which we point the reader towards (also explained in comment below along the same line). We use the present day simulations only to show the relative differences in NOx emissions and short-term ozone radiative forcing, so the method described here only relates/is applied to the the future scenarios. However, for completeness, have added the methane LBCs used (which are not perturbed in the aviation experiments, only depend on the background PD, SSP1 or SSP3 used) in the table caption: "Prescribed global mean methane mixing ratios for present day, SSP1 and SSP3 are 1834, 1519, and 2472 ppb respectively."

And yes, this is a global mean value as standard for these models, to clarify have specified *"a fixed global mean concentration"*

L131 Table 2 caption: change to "…perturbed (reduced) by 20%)…"

Added (reduced)

L144: Say "global annual mean" (change in methane mixing ratio).

Added "global annual mean"

L149: I understand your pragmatic approach of using a model mean value for feedback. But it is probably worth noting (here and/or in the later discussion), that this factor is likely to be different in each model – and is another contributor to model differences. You perhaps have not done the experiments to know how f varies between your models, but my suspicion is that this may well introduce significant inter-model differences to your results. Perhaps note as another aspect of uncertainties and further work.

True - these model-specific factors are available for some but not all models and do indeed add to the model spread/uncertainty. Have added a sentence: *"In the methane concentration calculations we also use a multi-model average for the methane feedback factor, which likely masks some inter-model differences had model-specific feedback factors been used, and adds to the uncertainty."*

L148/L159 Equation (1): [CH4]SSP. You define this as the 'methane reference mixing ratios in 2050', but Equation (1) must also be used for calculating the present-day impacts on methane of the 20% reductions in aviation (simulation 2 vs simulation 1)? (Actually, maybe this methane change is never calculated – PD short-term O3 RFs are in Figure 2, but Figure 5 only calculates the net aviation NOx RF for the future scenarios). My recommendation would be to additionally include the PD CH4 RF values somewhere, and rename [CH4]SSP to [CH4]REF, to make it generally applicable.

Have updated SSP to REF in Equation (1). Here we only look at the present day results in section 3 to provide some context for the emissions and ozone radiative forcing magnitudes, while the section 4 focuses on the sensitivity to background (future only) and includes the RF calculations. The present day RF results can be found in the Cohen et al (2026) paper, have added some text to point towards this (L340): "*(for equivalent results for present day simulations, see Cohen et al., 2026)"*

L143-174 Overall, although the description of how methane is handled is improved, I still found it quite confusing, and I needed to very carefully read this section several times to work out exactly what you had done. In addition to the previous comments, I have a few other recommendations: I suggest include a new table (Table 3) with details of the inferred CH4 perturbations, something like: Experiment [CH4]REF (ppbv) $\Delta\tau$CH4 (%) ΔCH4 (ppbv)

[CH4]perturbed (ppbv) RFCH4 (mW m-2) REF_PD 1800 - - - - PD_Air-20 1800 +1 +100 1900 -20 Etc.

Thanks for this feedback, have added this suggested table in the supplement to provide more details for the method and clarify this (also pasted below). Added a sentence in the methods: *"These inferred methane perturbations for the future scenarios, and Δτ_CH4 (for results in section 4.1) are given in Table S4."*

Table S4: Details for the inferred methane perturbations and RF calculated from the modelled methane lifetime change ($\Delta\tau_{CH4}$) from the 20% perturbation (reduction) in aviation NOx. Calculated based on the method described in the main text based on Equations 1 and 2.

	$[CH_4]_{REF}$ (ppbv)	$\Delta\tau_{CH4}$ (%)	ΔCH_4 (ppbv)	$[CH_4]_{PERT}$ (ppbv)	RF_{CH4} (mW m^{-2})
SSP3BG_SSP3Air					
LMDZ-INCA	2472	-2.354	-67	2405	-25.01
MOZART3	2472	-1.767	-51	2421	-18.77
OsloCTM3	2472	-1.812	-53	2419	-19.50
SSP3BG_SSP1Air					
LMDZ-INCA	2472	-1.239	-42	2430	-15.51
MOZART3	2472	-0.980	-33	2439	-12.25
OsloCTM3	2472	-0.969	-33	2429	-12.26
SSP1BG_SSP3Air					
LMDZ-INCA	1519	-3.627	-64	1455	-31.53
MOZART3	1519	-2.536	-45	1474	-21.96
OsloCTM3	1519	-2.490	-45	1474	-21.88
SSP1BG_SSP1Air					
LMDZ-INCA	1519	-2.060	-43	1476	-21.01
MOZART3	1519	-1.426	-30	1489	-14.50
OsloCTM3	1519	-1.351	-29	1490	-13.94

L164-166/Equation (2): As I suggest above, I would be explicit about the methane values, and rename/clearly define: [CH4] -> [CH4]perturbed [CH4]SSP -> [CH4]REF [CH4] (bar) = ([CH4]perturbed + [CH4]REF) / 2

Renamed as suggested

L172: I wasn't quite clear on the efficacy used for the long-term ozone forcing (associated with the methane forcing) – is this also 1.370, or does this come under "indirect forcings" and so is 1.180? My question really relates to what the efficacy (or ERF/RF ratio) depends upon – is it the gas, or is it a combination of the gas and the spatial/temporal structure of its change in distribution? I suggest briefly discuss and better justify your choices.

1.180 is used for the long-term ozone forcing conversion from RF to ERF. Have clarified this: "1.180 for the methane direct and indirect *(stratospheric water vapour and long-term ozone)*". These ratios are not well constrained and come from a single study, as already outlined in the next sentence highlighting this limitation. We do not discuss ERFs in the main text, rather focusing on RF results, exactly due to this limitation, but we do point to and provide the ERFs in the supplement (with a mention of this caveat, L358) in case it's useful for other studies to compare results, since ERF is widely used.

L189 Clarify if 'background emissions' refers to total anthropogenic NOx emissions, or all NOx emissions (including natural sources).

Background - > total anthropogenic

L197 Clarify the vertical profiles in Figure 2b are global annual mean changes in distribution.

Added *"(global annual mean)"*

L204 "baseline": You mix baseline and background and these have subtle differences in meaning. I suggest you try and consistently use background.

Here it refers to the baseline concentration simulated by the models (i.e. without emissions perturbations), rather than a background concentration. Have edited a bit to clarify *"model baselines (e.g. baseline ozone concentration in present day or 2050)"* Have checked all instances of use of 'baseline' to only refer to baseline scenario.

Figure 2b: Suggest add a vertical zero line for clarity. Figure 2b caption: Add global annual mean

Added 'Global annual mean' and a vertical line at zero.

L214 gradient between the stratosphere and the troposphere?

Added *"(due to smaller ozone gradient between stratosphere and troposphere)"*

L268 negative sensitivity to SSP1 -> lower sensitivity for the SSP1?

Changed negative -> lower

L293 are scenario-specific

Added ‘are’

Discussion of Figure 5: My reading of Figure 5 is that the net RF from NOx is approximately zero (I think if you had error bars on the net values they would all cross zero). I think this could be more clearly stated in the conclusions (I do think that someone could usefully say: after decades of work with multiple models, we can probably all go home and just worry about the aircraft CO2 emissions, and forget about the NOx, but I appreciate that is a bit sweeping and controversial!)

Agreed - this is in the discussion but not included in the conclusions. Have added the following in L428:
“The magnitude of net NOx forcing from aviation remains associated with a high uncertainty (spanning positive and negative values, driven by uncertainty in the parameters used for the calculation, and multi-model differences. This contrasts with the well-understood and robust climate forcing effects of CO2 emissions from aviation (Lee et al., 2021).”