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Atmospheric Chemistry and Physics Discussions

Interactive comment on "TRADEOFFs in climate effects through aircraft routing: forcing due to radiatively active gases" *by* F. Stordal et al.

Anonymous Referee #2

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General Comments

This paper presents calculations of the radiative forcing impacts of aircraft CO2 and NOx emissions, and the effects of different routing scenarios. The authors conclude that the different scenarios lead to only small changes in the radiative forcings from the gases, and that these are likely to be smaller that the changes due to contrails. However that should not detract from the value of the study as it is still important to publish negative conclusions.

Four models were used in this study. However only three of them were used in the radiative forcing calculations. There needs to be more justification for including TOMCAT in this study as it does not currently contribute to the conclusions. It would therefore be

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very valuable to run the TOMCAT results through the Oslo radiation code.

The paper does not provide convincing evidence that the coarse-resolution ULAQ model can be used to simulate something on as small a scale as aircraft. This is particularly true for the changes in flight altitude which are far smaller than the ULAQ grid spacing.

Generally the advantages of multi-model comparisons are not exploited fully. I would recommend looking at the ozone budget terms and the OH distributions to understand model differences even if the plots are not presented in the paper.

Plots of the vertical distributions are needed, particularly for sections 4.3 and 4.4.

I found the concepts of "backward looking" and "forward looking" approaches very confusing. The concepts of radiative forcing and GWP are well used in the literature, and if explanations are needed then the IPCC reports are a good starting point. For CO2 a steady state approach is not applicable, a full history of the emissions are needed.

This paper is a useful study and should be published after revision. I would particularly recommend calculating radiative forcings from the TOMCAT model.

Specific Comments

1) Abstract, lines 8-11: I found this sentence difficult to understand (also see general comment about "backward-looking"). It should be re-written, and split into several sentences.

2) Abstract, line 17: "The differences" - differences with respect to what?

3) page 10737, section 2.1: Could do with a very brief summary of what hydrocarbons are included.

4) Page 10738, section 2.2: Could do with some comment about whether lack of NMHCs will affect the simulation of ozone from aircraft.

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5) Page 10739, section 2.3: This section needs to describe the tropospheric chemistry a lot more since this is where most of the aircraft fly. Is the tropospheric chemistry sufficient to model ozone from aircraft. Why is the sulphur chemistry described? Are sulphur emissions from aircraft treated? What effect will mixing aircraft emissions over a 2.84km layer have on the ozone production. Given the non-linearity of ozone production can a model with this vertical and horizontal resolution be used?

6) page 10743, line 2. Please give more explanation here on how to go from CO2 emission increases to a CO2 forcing. This paper might be read by non-experts. How do you apply the decay function? The correct solution would seem to be to integrate the decay curve convolved with the aircraft emissions profile from 1903 to 2000 to get the 2000 change in CO2 concentration due to aircraft. Is this what is done?

7) page 10743, lines 10-15: I find this "backward-looking" terminology very confusing. I'm sure there must be a simpler way to describe the concept of RF.

8) Page 10743, lines 24-29: For the forward looking case, the evolution of the long-lived species (CO2 and methane) may depend on their past histories. Do the authors consider a no-aircraft atmosphere in 2000 and then suddenly switch on CO2 emissions and methane loss for 20 & 100 years? Or do they start with a 2000 aircraft atmosphere where methane is in equilibrium (but CO2 still not) and consider a continuation of the emissions for 20 & 100 years compared to a switching off? I assume the time evolution of the primary ozone mode is taken into account for the 20 year RF. The primary mode certainly won't be in steady state over that short period.

9) page 10744, line 15. The Kraabol parameterisation may account for the 20% difference between Oslo and LMDz, but not for the 50% difference between TOMCAT and Oslo. Why might TOMCAT be so much higher? ULAQ get even lower ozone than Oslo. Yet not only do they not account for sub-gridscale effects, because of their resolution they spread the NOx emissions over the largest volume. This should give the highest ozone response.

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10) page 10744, lines 18-20: The authors also need to mention the effect that two of the models not including stratospheric chemistry might have.

11) page 10744, lines 21-23: This link between NOx emissions and methane lifetime needs a lot more explanation. It is not obvious that NOx emissions in the flight corridors lead to OH changes on the global scale. Changes in OH at aircraft altitudes and in the mid-high latitudes don't affect methane lifetime, there needs to be some mechanism transporting the aircraft impacts to the lower troposphere at lower latitudes such as ozone, PAN or CO. A latitude-height plot of OH would help. Which models include PAN chemistry? Does the effect on methane lifetime correlate with the amount of ozone produced? There are four models, not three, for this entry in table 3.

12) Page 10745, line 2: Why are there only three estimates here not four? The TOMCAT results need to be put through the Oslo radiation code.

13) Page 10745, line 12: "new steady state" - there are variety of timescales involved in this study and the authors need to be clearer about them. CO2 will never be in steady state (timescales ˜1000 years), the short-lived ozone mode will equilibrate within a few months, but the long-lived methane and ozone modes will take a few decades (e-folding time ˜12 years) to equilibrate. How do the authors decide which species to regard as time varying and which as in equilibrium?

14) page 10745, top of section 4.2: The authors should explain why only two models were used. I assume it is due to lack of stratospheric chemistry in the others.

15) page 10746, lines 1-4: These sentences seem to imply that the ULAQ model is missing a real effect (increased ozone at high latitudes) due to its resolution. If that were the case it would seem sensible not to give too much credence to the ULAQ change.

16) page 10746, lines 6-8: Why does the ULAQ model predict ozone losses in winter, whereas the Oslo doesn't. Is it because ULAQ models stratospheric aerosol?

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17) page 10747, section 4.3: This section needs latitude-height contour plots to understand the effect of flight altitude changes. A lot of the results will hopelfully become a lot clearer then. Examination of changes in ozone production and loss might help explain what is happening in the different seasons. With a resolution of 2.84km, how is a 1.8km flight change implemented in ULAQ?

18) page 10747, lines 18-19: What are the reasons for this methane change, more ozone (not true for ULAQ) or NOx emissions closer to the methane oxidation in the lower trop?

19) page 10747, line 25: How is a 2000ft (˜600m) flight change implemented in ULAQ?

20) page 10747, line 28: I wouldn't expect the ozone increase to be linear with height change anyway.

21) page 10748, line 6-10: Latitude-height plots of OH would help identify the reasons behind the methane lifetime change. The models with methane reductions don't include stratospheric chemistry, could this be a factor?

Technical corrections:

Abstract, line 1: "radiative forcing." -> "radiative forcing from aircraft."

Abstract, line 18: "moderate adopting 100" -> "moderate when adopting a 100"

Abstract, line 20: "Also, we have found" -> start new paragraph "We have found"

Page 10736, line 2: "e.g. the altitude" -> "the altitude"

Page 10736, line 3: ", whereas in the case" -> ". In the case"

Page 10736, line 7: "can" -> "could"

page 10736, line 8: "will" -> "may"

page 10736, lines 9-10: "to include .. trading schemes" -> "that aviation

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emissions should be included in trading schemes."

page 10737, line 4: "included also" -> "also included"

page 10737, line 8: "data and" -> "data,"

page 10737, line 10: "Advection applies" -> "For advection it uses"

page 10737, line 14: spell "Mueller" with umlaut, and in refs.

page 10738, line 15: "and including" -> ", including"

page 10739, line 25: use letter O, not numeral 0

page 10742, line 26: "Appendix 2"->"Appendix B"

page 10743, line 21: "We are assuming" -> "We assume"

page 10746, line 1: "only in" -> "in only"

page 10746, line 25: "ozone loss" -> "ozone production"

page 10747, lines 4-5: "Like in ... as well as ozone" -> "The ozone change is negative in LMDz/INCA, Oslo CTM2 and TOMCAT. This is expected due to a shorter lifetime for NOx and ozone at lower altitudes."

page 10755, line 17: "Mueller, J" -> "Muller, J.-F." with an umlaut on the "u".

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 10733, 2006.