Atmos. Chem. Phys. Discuss., 14, C1397–C1401, 2014
www.atmos-chem-phys-discuss.net/14/C1397/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aviation 2006 NO_x-induced effects on atmospheric ozone and HO_x in Community Earth System Model (CESM)" *by* A. Khodayari et al.

Anonymous Referee #1

Received and published: 14 April 2014

The manuscript by Khodayari et al. examines changes in ozone, HOx, methane lifetime, and ozone radiative forcing due to aviation NOx emissions, based on simulations using the CAM4 and CAM5 models. The findings, though not surprising, are clear and interesting, and will be a useful addition to the discussion on aviation effects on atmospheric composition and climate. The manuscript is well written and the topic is certainly suitable for Atmospheric Chemistry and Physics. Therefore, I recommend publication following some revisions described below.

GENERAL COMMENT:

The manuscript is based on results from two versions of the CAM model. This certainly adds value to this work, as it explores (to some extent) the robustness of the results. However, the manuscript does not provide substantial insight as to why the two models respond in significantly different ways when it comes to oxidant responses to aviation NOx emissions. The fact that the authors stress from an early stage (Abstract) that the main difference between the models is that they handle aerosols differently, and the fact that in Section 2 there is a fairly thorough description of the aerosol component of the models, makes the reader expect that the results will be discussed in the context of those differences in the model set-up between the two sets of experiments. More specifically, one expects the answer to the question: why does a different handling of aerosols in the models lead to a) differences in oxidants, and b) different responses to aviation NOx?

I would suggest that the authors at least add some discussion to address such questions.

SPECIFIC COMMENTS:

Page 6164, Lines 25-27: It is important to clarify here whether the radiative forcing estimate includes effects of NOx on ozone through methane. Clearly the study does not address such an effect, but it should be clear in the abstract.

Page 6165, Lines 23-25: I am not sure whether this statement is correct: aerosols would cause more cirrus nucleation, leading to more longwave radiation being trapped, and thus leading to heating.

Page 6166, Lines 8-11: Please add a few sentences to summarise the findings of these "several studies".

Page 6166, Lines 27-28: But the paper does not really discuss how different aerosol handling can modulate such effects. Can you suggest some possible mechanisms?

Page 6167, Lines 20-23: So, it is not just the aerosols that differ, it seems. If so, you

ACPD
14, C1397–C1401, 2014

would need to amend previous parts of the text (e.g. lines 27-28 on page 6166 and lines 9-11 on page 6164).

Page 6168, Line 16: What is a "triangular distribution" in this context? Please explain.

Section 2: As stated earlier, this section is dominated by the description of the differing aerosol handlings, whereas there is not much discussion on how these matter later on. On the other hand, not much is mentioned for other crucial aspects of the models, e.g. how is photolysis handled?

Section 2, last paragraph: Please briefly state why the radiative forcing calculations were not performed online, to avoid inconsistencies with model (e.g. having to use ISCCP etc).

Page 6169, Line 26: Is it certain (e.g. from previous studies, or from the authors' tests) that the atmosphere would need 6 years to reach steady-state, even with fixed methane? It might have been worth discarding less years and using more years to assess statistical significance post spin-up. Please discuss.

Fig. 1: Please state in caption that this is for the control runs.

Pages 6170-6171: It would be interesting to discuss in this section whether adding aviation NOx emissions improves model performance in general (in CAM4 and in CAM5).

Pages 6174, Lines 2-3: Do not see ozone decreases below 450hPa in Fig. 5. Ozone seems to be increasing everywhere. The authors possibly refer to the net ozone production.

Pages 6175, Lines 6-7: Differences are arguably not "small", compared to intermodal variability, but perhaps "smaller".

Pages 6176, Line 11: Any ideas why it is more distributed towards the surface in CAM5.

Pages 6176, Lines 26-28: Which feature? The previous sentences just state that the high-altitude perturbation extends into the surface for both seasons/models.

ACPD
14, C1397–C1401, 2014

Pages 6177, Line 27: But why is photolysis increasing in the mid-troposphere?

Pages 6178, Lines 21-22: Please change “concentrations” to “perturbations” or “concentration changes”.

Pages 6178, Line 24: I would rephrase to “thus increasing OH, and subsequently HO2”.

Pages 6179, Line 6: Please rephrase to “In both CAM5 and CAM4, the changes in methane loss are mostly confined. . .”

Table 3: It is stated in the caption that the models underestimate observation-based methane lifetime. I would suggest mentioning this in the text too. It is also worth mentioning that most current models feature such methane lifetime underestimates (e.g. see ACCMIP studies for most recent multi-model estimates: Voulgarakis et al., 2013 and Naik et al., 2013).

Pages 6179, Lines 18-19: It would be useful to give reference for the methane lifetime feedback concept, as well as for the feedback factor mentioned further down (line 23).

Section 4.5: It is worth highlighting that the overall highest RF changes are over Southern Europe and the Middle East.

Pages 6180, Lines 14-15: How do these figures compare with RF from other sectors? It would be useful to put these estimates into context.

Pages 6180, Lines 16-17: Please explain more clearly what is meant by “short-term ozone”.

Pages 6181, Line 10: I believe the authors intended to write “longer” rather than “shorter” when referring to the winter photochemical lifetime of ozone.

Pages 6181, Line 25: I do not particularly like the phrase “various complicated aerosols processes”. Please either remove this or state which processes are implied.

Pages 6181, Lines 25-28: I understand that the authors meant to state that the dif-

ACPD
14, C1397–C1401, 2014

ference between CAM3 and CAM5 ozone responses is smaller that the present-day model uncertainty regarding these effects. If so, please rephrase.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 6163, 2014.