Atmos. Chem. Phys. Discuss., 12, C11609–C11615, 2013
www.atmos-chem-phys-discuss.net/12/C11609/2013/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global sensitivity of aviation NO_x effects to the HNO_3-forming channel of the $HO_2 + NO$ reaction" *by* K. Gottschaldt et al.

K. Gottschaldt et al.

klausgottschaldt@web.de

Received and published: 15 January 2013

We thank the referee for the constructive and helpful comments, which focus on the quality of the figures and the degree of detail in the presentation. A revised manuscript will contain improved figures, as detailed in our reply to Anonymous Referee 1. We considerably reduced the paper, as detailed below. However, this paper essentially combines two studies and thus the revised manuscript might still be longer than average. The first part addresses the effects of the reaction channel in question on atmospheric chemistry in general, which is a prerequisite for the discussion of aviation effects against the three different background settings in the second part. The abstract has been modified to clarify this aspect. Also, we tried not to sacrifice reproducibility for readability.

The following parts were shortened, moved into appendices or the Supplementary material:

- section 2.1: some model configuration details, including Table 1 (now Table B1)

- section 2.4: QCTM

- section 3.1: parts of the discussion on reaction rates and Fig. 2 (now Fig. S5)

- section 3.3: discussion on HO2 and the corresponding parts of Fig. 1 (now Fig. S6)

- sections 3.5, 3.6: removed discussion of some details

- Fig. 4 into Supplementary material (now Fig. S7)

- section 5: RF according to Holmes et al. (2011)

- section 5.3: shortened discussion of Unger (2011)

The following replies to detailed comments are tagged by page and line numbers of www.atmos-chem-phys-discuss.net/12/24287/2012/

p. 24288, l. 10: *Make clear that without this reaction the effect would be positive. In my understanding relatively small changes in chemical mechanisms or other parametrization can turn the climatic effect from positive into negative*

Most other studies attribute a warming effect to aviation NOx (e.g. Sausen et al., 2012), but any value for aviation NOx related radiative forcing depends on various methodological assumptions, and thus is on its own ambiguous. Our methodological choices and other uncertainties are discussed throughout the paper. They include above all the emission history (compared to increasing emissions, sustained emissions give a higher weighting to the cooling long-term effects), the emission inventory, the inclusion of secondary effects from methane perturbations, different methods for gauging chemical perturbations and different methods for estimating radiative forcing for given chemical perturbations, the model (particularly the NOx background and related pro-

ACPD

12, C11609–C11615, 2013

cesses). Table 5 shows that for our methodology aviation NOx related RF would be positive (close to zero) without the HNO3-forming channel and negative for both implementations of the channel. The near-zero effect without the reaction is in accord with another recent study (see section 5), which required only small adjustments to our methodology. We changed the wording of the abstract to avoid an absolute statement about the sign of the RF without methodological context.

however the reader needs to understand that these effects are anyhow not so large compared to those of other emissions.

We added a sentence to the introduction about the importance of aircraft NOx emissions, but refer to the literature for a further discussion of the relative importance of aviation NOx effects with respect to other aviation effects and other transport emissions (e.g. Sausen et al., 2012).

p. 24288, l. 15: *Mention already in abstract what evidence from measurements there is corroborating these results. In the text I find that HNO3, NOx, CO profiles match equally well the observations- with or without these reactions, which means that the importance of this reaction remains somewhat hypothetical.*

Besides the limitations of our evaluation itself, model uncertainties (e.g. emissions, parameterisations, other chemical aspects) prevent definitive conclusions about the HNO3-forming channel. The focus of the study is on aviation NOx effects, and comparisons to observations are only meant to ensure that the atmosphere is reasonably well represented in all our simulations. Thus most evaluation parts were placed in the supplement already. Mentioning them in the abstract might be misleading.

p. 24290: *Would it be possible to give the reactions R1b; R2b already here?*

As recommended, we moved reactions R1b, R2b and R3 from section 2.3 to section 1.

p. 24290, l. 21: *What did Hoor mention? This sentence doesn't give information, so either delete or tell why we need to know this.*

ACPD

12, C11609–C11615, 2013

As recommended, we removed the sentence.

p. 24290, l. 25: *affected much ? little?*

We removed the half-sentence, because it is not relevant for our study.

p. 24291, l. 25: *Explain more extensively why above 200 hPa no nudging was used. Briefly describe the characteristic of S2 setup*

We modified the sentence to make clear that we simply took the nudging settings from that simulation of Jöckel et al. (2006), which resulted in the most realistic dynamics. Parameterisations (e.g. boundary layer) of the ECMWF model and the EMAC model are not always compatible. The nudging coefficients are chosen such that the EMAC internal physics is disturbed as little as possible, but the prescribed synoptic scale dynamics is still enforced.

p. 24292, l. 13: *was evaluated by : One sentence on what is the result of such evaluation.*

As recommended, we added a half-sentence.

p. 24292, l. 22: *'high convective liquid and ice water contents' reader has no idea of what is going on, and what other consequences could be present. Is this detail needed?*

This detail is only relevant for EMAC users. It is needed for reproducibility, because that update in the scavenging parameterization was introduced for this study (EMAC 1.10), but has been officially included only from EMAC release 2.42 onwards. We added a corresponding footnote in Appendix B.

p. 24292, l. 27: *what is meant with transient? I guess you use multi-annual simulations and the biomass burning emissons where valid for that year Biomass burning emissions are valid for the years simulated and vary on a monthly basis.*

We modified the sentence before line p. 24292, line 27 to clarify what it refers to.

ACPD

12, C11609–C11615, 2013

p. 24293, l. 3: *scaling factors? up or down? How much?*

We refer to the cited paper of Eyring et al. (2010) for a discussion of the scaling factors used to extend the time range of the original shipping emissions data.

p. 24293, l. 25: *ENSO is a tropical index and can not characterize the deviation from the climatology all over the globe. Maybe you want to focus on the tropics?*

We removed the sentence about ENSO.

p. 24296, l. 16: *Here I am wondering wondering if your sensitivity studies are gearing up towards a study on additivity /linearity? I am wondering why you haven't chosen for a milder perturbation (i.e. 10 or 20 %) instead of switching off?*

We removed the statement about the small perturbation. The 100% perturbation was chosen for comparability with other studies.

p. 24297: *QCTM Here I am confused; The model is nudged (upto 200 hPa); are you really to capture independently the effects on dynamics. At most partly. In line 20 I read that the feedback to dynamics was switched off, but then it is not clear why not to nudge directly all meteo data? I think the reasoning behind this procedure should be described clearer and earlier.*

We could indeed have used a CTM for this study. It makes no difference to the chemical analysis if the dynamical fields are prescribed offline from some external model, or if they are recalculated for each simulation ... as long as the dynamics is identical in all simulations. The dynamics generated by EMAC is per se not better or worse than dynamics from any other model that could be used to drive a CTM. Jöckel et al. (2006) showed that EMAC in nudged mode yields realistic dynamics. Calculating dynamics once and then use it for all simulations was no option, because there is no pure CTM mode available for EMAC. However, the computational cost for calculating dynamics is small compared to the chemical calculations anyway. As suggested, we added a sentence about the QCTM mode to an earlier section (2.1). The discussion of the

ACPD

12, C11609–C11615, 2013

QCTM mode was extended, and moved from section 2.4 into Appendix A.

p. 24303, l. 10-15: *To my opinion there are many processes not or not accurately included in this and other models; a good correspondance with OH should therefore not be the goal of this study. Why do we need to know the 'Tamborini' study?*

Global OH concentration is one of the most important parameters in atmospheric chemistry. In particular, the reaction of OH with CH4 is the biggest sink of the greenhouse gas methane. Methane lifetime is an important parameter for estimating the anthropogenic greenhouse effect. Considering the HNO3-forming channel of HO2 + NO or not may change estimates of methane lifetime by about 50%, imposing a considerable additional uncertainty on methane budget calculations (if not solely based on observations). We highlighted this aspect in the abstract and the conclusions of the revised manuscript. The effects of the reaction on global OH concentrations are also fundamental for turning the radiative effects related to aviation NOx from warming into cooling. On the other hand, global OH concentration is according to experience relatively insensitive to reasonable parameter variations in the model. Thus we put some effort into showing that all our simulations are covered by observational constraints. We agree that there are more uncertainties in the model impacting on OH, and modified the presentation of the Taraborrelli et al. (2012) study accordingly.

References:

Jöckel, P., Tost, H., Pozzer, A., Brühl, C., Buchholz, J., Ganzeveld, L., Hoor, P., Kerkweg, A., Lawrence, M. G., Sander, R., Steil, B., Stiller, G., Tanarhte, M., Taraborrelli, D., van Aardenne, J., and Lelieveld, J.: The atmospheric chemistry general circulation model ECHAM5/MESSy1: consistent simulation of ozone from the surface to the mesosphere, Atmos. Chem. Phys., 6, 5067-5104, doi:10.5194/acp-6-5067-2006, 2006.

Sausen, R., Gierens, K., Eyring, V., Hendricks, J., Righi, M.: Climate impact of transport, in: Atmospheric Physics – Background – Methods – Trends, Schumann, U. (ed.),

ACPD

12, C11609–C11615, 2013

Springer, doi: 10.1007/978-3-642-30183-4, 711-725, 2012.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 24287, 2012.

ACPD

12, C11609–C11615, 2013

Interactive Comment