

Interactive comment on “Impact of aircraft NO_x emissions on the atmosphere – tradeoffs to reduce the impact” by M. Gauss et al.

M. Gauss et al.

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Once again we would like to thank reviewers for their valuable comments. A revised manuscript has been submitted to ACP taking into account all reviewer comments and suggestions.

Below are our answers to the comments of reviewer #1. We first quote the reviewer. Our answers are marked by >> <<

Anonymous Referee #1 Received and published: 17 January 2006

The newly introduced aircraft inventories, which include feedbacks of flight altitude shift to fuel consumption, allow a fresh look at the net impact of such shifting. I even think that the authors have underplayed this considerable progress in their paper, as it is

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not mentioned in the abstract and casually noted in the concluding section. It might have been expressed that considering this feedback is a necessary pre-requisite if the consequences of short-term flight altitude shifting is to be assessed. (For long-term shifting with adjusting aircraft design to the new flight routes, the normalized approach may be acceptable for a sensible assessment.)

>> We fully agree. This feedback is now mentioned in the abstract (following reviewer suggestion B1 below). <<

Nevertheless, I think that major revision of the paper is required to improve the quality of the presentation. I comprise my criticism in three major points and add a number of suggestions that may help to improved some details of the presentation. A0) Major points: I have three main critical points where I think that a major revision is worthwhile, all of them are related to the way the results are presented: A1) The separation of presenting the ozone radiative forcing (RF) calculations from the description of the corresponding ozone change perturbations is difficult to comprehend. In both abstract and conclusions this leads to an unsatisfactory comparison of column ozone (in DU) with methane forcing (in RF units). Such a comparison of different parameters does not allow reasonable conclusions with respect to the net effect. Furthermore, it is possible that for ozone change patterns showing a dipole structure (such as Fig. 15, right) even the sign of the column ozone change may not be a reliable predictor for the sign of the related RF change. If the authors cannot provide a convincing reason why the ozone RF results are not to be included in the present paper, I recommend that the methane RF results should be left out as well and be postponed to the anticipated later publication (p.12277, l.21), leaving the present paper to describe NO_x, ozone, and OH concentration changes. The authors may consider to declare the two corresponding papers as "Part I" and "Part II".

>> As the Stordal et al. publication has progressed quite a lot recently, it was considered most reasonable to proceed corresponding to the reviewer's second suggestion, i.e. the methane results are now removed from the present publication and will be in-

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cluded in Stordal et al. However, since this paper is a one-model study while Stordal et al. is a multi-model study we refrain from titling the two papers as “Part I” and “Part II”. The connection between the papers will be made clear in Stordal et al. In addition this paper refers to Stordal et al. and its anticipated RF results (‘conclusions’ section). <<

A2) The designation of the simulations (“1”, “2”, “3”, ...) has quite a technical character, apparently due to historical reasons (as No. “4” is missing). This non-suggestiveness makes the understanding of text and figures unnecessarily difficult. I therefore recommend to provide the simulations with more suggestive names, e.g.: yy = reference, 1 = base, 2a = polar-NORM, 3b = down-ADJ, etc., or something similar.

>> We have chosen new acronyms, very similar to the ones suggested by the reviewer: ‘ref’, ‘base’, ‘pol_norm’, ‘low_adj’, ‘high_adj’, Ë We fully agree that this has made the paper and figures much easier to read. <<

A3) There is a (too) large number of figures in the paper, and some of them are not discussed very deeply (e.g., Fig. 3, Fig. 10, Fig. 14). Likewise, the 2b scenario is only briefly mentioned one time in the text without drawing any conclusions. The authors may consider to focus their paper on those aspects that are relevant for their main conclusions.

>> The number of figures was indeed too large and has been reduced by about 1/3 (from 21 to 15). In particular, the figures showing total ozone change were removed following a suggestion of reviewer 2, and the figure showing magnitudes and locations of the NOy and NOx perturbations was removed as it is not relevant for our main conclusions. However, figures 3, 10 and 14 have been kept (after the renumbering they are now called Figures 2, 7, and 10). We consider Figure 2 to be important for a better understanding of the vertical shift in aircraft emissions, which plays a central role in this paper. Inspired by the reviewer’s suggestion we have also added a few sentences to the discussions of Figures 2, 7, and 10. The reason why the 2b scenario is not discussed in detail is that we don’t consider it to be a realistic scenario. Although

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the use of polar routes may increase total air traffic the increase won't be as significant as in scenario 2b because polar routes to a large extent will be implemented at the expense of mid-latitude routes. <<

B0) Minor suggestions: B1) p. 12256, l. 9: A hint may be included to the "b" scenarios, e.g., "... are investigated, including feedbacks of flight route shifting to fuel consumption and emissions."

>> Done. (added " are investigated, taking into account effects of flight route changes on fuel consumption and emissions.") <<

B2) p. 12256, l. 19: Replace "...lower altitudes." by "...altitudes below."

>> Done. <<

B3) p. 12256, l. 21: Replace "...the stratospheric decrease dominates..." by "...the contribution from higher altitudes dominate..."

>> Done. <<

B4) p. 12258, l. 2,3: Removing "which is lost primarily through the reaction with OH" may make the sentence more readable without loss of important information.

>> Done. <<

B5) p. 12258, l. 7: "...act on spatially and temporally different scales...", Stevenson et al. (JGR 2004, doi:10.1029/2004JD004759) may be mentioned here.

>> Done. <<

B6) p. 12259, l. 3: "...results contributed to TRADEOFF by the Oslo CTM-2..." to relate this paragraph to the preceding one.

>> Done. <<

B7) p. 12260, l. 28: Specify if the daily, monthly mean, or annual mean tropopause is used because this information is needed to understand later arguments concerning

differences of the scenarios in certain seasons.

>> The tropopause is updated every 6 hours using tropopause pressures given by the NCEP reanalysis. This is now clarified in the text. <<

B8) p. 12262, l. 7: "... of fuel burnt, NO_x emissions, and flown distances..."

>> Done. <<

B9) p. 12263, l. 7: Explaining the parameters controlling E.I.(NO_x) in the various inventories may help the reader to understand these numbers.

>> The variation of EINO_x is rather complicated, but is a 'real' effect in the inventories. For example, in case 2b, the frequency of a small number of routes that goes near the pole (a particular subset of aircraft types) is magnified. By doing this, the overall inventory EINO_x changes as a result of changing the 'population' of aircraft in the inventory. Also, for the case 5b, this is an example of the EINO_x being actually higher as a result of the recalculated emissions. An according statement is now added to the manuscript. (section 3, end of 4th paragraph) <<

B10) p. 12263, l. 14: "tropopause" should read "troposphere"

>> Done. <<

B11) p. 12263, l. 20: Is the absence of military aircraft in "1" the only difference with respect to "yy" ?

>> Yes. According to one of the co-authors (David Lee) there was no difference in NO_x emission technology as was previously thought by the first author. Also, there was an error in the total emission number of the reference case which is now corrected. It is 0.656 Tg(N)/yr, compared to 0.594 Tg(N)/yr in the 'base' case that does not include military aircraft. <<

B12) p. 12264, l. 15: See introducing remark! "This approach reflects the impact change resulting from alternative flight routing in a more realistic way than the normal-

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ized scenarios. Moreover, it allows ..."

>> Done. <<

B13) p. 12265, l. 3: Why have forecast data rather than real data for 2000 been used? Provide a reasoning or a reference.

>> Forecast data is not necessarily less 'real' than analyzed data. We generate our meteorological data using the Integrated Forecast System of ECMWF and use forecasts only on a short term (12 to 36 hours). The forecast data provide fields that are in better agreement with the primitive equations of dynamic meteorology than the analyses (which still include some degree of measurement error). For example in the EU FP5 CANDIDOZ project we have found that using the vertical velocity derived from forecast horizontal winds gave more stable results than the analyzed vertical velocity. In the paper we have clarified that we are using short term forecasts and that these have been found to give better results than the reanalyzed data. <<

B14) p. 12265, l. 5,6: Is there a spinup period for the chemistry, and how long is it ? Can year to year variability be neglected (reference) ?

>> The model was spun up for 5 years. This is now mentioned in the text. The year-to-year variability is hard to judge based on the available model results, which is why the planned use of other years than 2000 was mentioned already in the first version of the manuscript ('Conclusions' section). Very recently we have retrieved meteorological data for the years 2001-2005 and plan to study interannual variability. This point is now made clearer by a parenthesis in the conclusions section. ("It is planned to run the CTM with meteorology from other years than 2000 in order to assess the inter-annual variation of the impact") <<

B15) p. 12265, l. 13: I think that Table 3 should be introduced before Table 1. Alternatively, both tables may be merged into one.

>> This table was meant to make life easier when dealing with acronyms such as

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'2a', '3b', '5b', etc. The new acronyms, introduced following reviewer comment A2, are easier to deal with and make the aid of Table 3 unnecessary. We have thus decided to remove Table 3. <<

B16) p. 12266, l. 17: "... during these months."

>> Done. <<

B17) p. 12266, l. 24, 27: "certain altitude/ height" may be replaced by a more quantitative phrase.

>> Replaced by "about 20 km". <<

B18) p. 12268, l. 10 etc.: Does this imply that a global mean CH₄ decrease has been calculated from a global mean OH increase, and that this global mean CH₄ change has been converted to a radiative forcing. Please, state so or otherwise. Which radiation code has been used to calculate the CH₄ RF ? (see A1, however.)

>> CH₄ results and discussion is removed, these will be presented by Stordal et al. (2006) in the near future" <<

B19) p. 12269, l. 7: Replace "sensitivity" by "relative change".

>> Done. <<

B20) p. 12272, l. 12: Replace "chemical net production" by "ozone net production".

>> Done. <<

B21) p. 12273, l. 12: To relate this sentence to the preceding one, I suggest the reformulation: "In contrast, an increase in the tropospheric ozone column is modeled at mid- to high Northern latitudes for the lower-altitude scenario (3b).

>> Done. <<

B22) p. 12274, l. 20: "...which is again most pronounced during summer, as it was in the lower-altitude case (Fig. 14)."

>> Done. <<

B23) p. 12274, l. 23: "...NOx emissions from fuel burn enhancement is positive"

>> Done. <<

B24) p. 12275, l. 8: "... are opposite in sign to what ..."

>> Done. <<

B25) p. 12276, l. 5: "In spite of ..." , this sentence needs to be reformulated

>> This sentence dealt with methane RF, which is now removed from the paper. <<

B26) p. 12277, l. 5: "... use of high latitude routes..."

>> Done. <<

B27) p. 12277, l. 6: Please, reformulate the begin of this sentence.

>> Changed into "This study has also investigated the impact of changes in flight altitude" <<

B28) p. 12278, l. 5-7: In view of flight altitude sensitivity considerations, the recent paper of Fichter et al. (Metorol. Z., 2005, p 563ff.) may be cited here.

>> Done. <<

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 12255, 2005.

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