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Atmospheric Chemistry and Physics Discussions

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 #2. We first quote the reviewer. Our answers are marked by >> <<

Anonymous Referee #2 Received and published: 18 January 2006

2 Specific comments Regarding the length and focus of the paper, the authors elaborate on the standard impact of aviation at great length. As this part of the study is not really the focus and has been done by many others, my suggestion is to present it in

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a more condensed fashion, just to show that the results are consistent with previous studies. As for the more interesting part dealing with the alternative routings, it seems that some Figures (e.g. 8, 12, 16, and 20) could be removed without losing much of the necessary information. Also the text is rather descriptive of what qis to be seen in the Figures.

>> Figure 5, which was part of the discussion on the reference scenario, has been removed from the paper. Figures 8, 12, 16, and 20 have been removed from the paper. <<

However, the authors identify distinct mechanisms that explain the effects of the alternative routes, i.e. the amount of emissions deposited in the troposphere in combination with wash-out andor convection. It would make the paper much clearer that per alternative routing the key mechanisms are presented in a more systematic way, followed by the most informative figures that reveal the expected effects.

>> After the removal of (the old) Figures 5, 8, 12, 16, and 20, as well additions to the discussions of the remaining figures the discussion is now more focused. Also, in section 4.2 and 4.3 the expected key mechanisms are now stated immediately before the respective results are presented. In section 4.2 we have added "The polar environment differs from the mid-latitude regions because of the relatively low tropopause height, implying a larger fraction of the flight operations occurring in the stratosphere. This, in turn, implies a less efficient removal of NOx pollutants through wash-out at cruise altitudes. Also, the strong dependence of chemistry on sunlight combined with the strong seasonality of insolation in high latitudes lead to a large seasonal variability in ozone impact due to aircraft in high latitudes, which is of particular importance when considering increased high latitude routing." In the beginning section 4.3 we have briefly summarized the significance of cruise altitude changes for NOx and ozone perturbations. Finally we hope that through the renaming of the scenarios the identification of the key mechanisms has become more transparent. <<

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I mentioned the application of a trop-strat chemistry model as a novelty of this study. However, the consequences of this approach remains untouched. It is clearly beyond the scope of this paper to discuss this extensively, but some remarks on this would be interesting. I find it worriesome that the split between tropospheric chemistry has been put exactly on the tropopause and they do not overlap in the UTLS. The UTLS is a mixing zone, so this division is artificial. Especially on impact of aviation studies this is an important issue. The authors mention plans to address this issue in future simulations.

>> The Oslo CTM-2 model applies two different chemical schemes, one for the troposphere and one for the stratosphere. The tropospheric scheme does not include Cl/Br, and the stratospheric scheme does not include non-methane hydrocarbons (NMHC). It has to be stressed, however, that all components, including halogens and hydrocarbons, are present (and transported) throughout the entire model domain. Cl/Br mixing ratios in the troposphere are taken from the Oslo 2D model, while for NMHC in the stratosphere finite lifetimes are applied. The ozone production cycle from methane is included in both the stratospheric and the tropospheric scheme. It is true that the tropospheric scheme does not include Cl/Br, but the catalytic destruction cycles involving halogen are found not to be important in the upper troposphere. There is thus no pronounced discontinuity in the transition between the two schemes. In order to investigate this in a more quantitative manner, numerous test runs have been made since the TRADEOFF project where the height of the tropopause was varied by up to 3 km both up and down. It was found that the two chemical schemes agree very well in terms of NOx and ozone (within 5%). Two sentences addressing this have been added to the text: “In addition to the reactions that are relevant for the stratosphere the [stratospheric chemistry] scheme includes the ozone production mechanism involving methane.” and “It has to be stressed, however, that each transported species is present and advected throughout the entire model domain, and non-methane hydrocarbons are calculated above the tropopause according to their globally averaged stratospheric chemical lifetimes, so that no notable discontinuity exists at the transition zone between the two

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schemes.” <<

The section on the construction of the inventories is somewhat confusing. ’The novelty for the TRADEOFF work’ (page 12262) seems to be out of place, because it is followed by a technical description of the construction. Should this sentence be place more below where the alternative routings are described?

>> Done. The sentence was indeed out of place and has been moved to the beginning of the paragraph where the alternative routings are described. <<

The difference between the reference run and the base run is not clear at all. The TRADEOFF base run assumes a somewhat higher standard of technology in NOx reductions. So, what is the more reasonable assumption? If the base run uses the best estimates, why is the reference run used at all? The authors should clarify this. Also, are the emissions of the military aircraft held constant in all simulations (I guess so)?

>> The reference case (now labeled ‘ref’) includes military aircraft, while the ‘base’ case does not. This is now explicitly stated several times in the paper (first two sentences of section 4.1, first two sentences of section 4.2, and Table 1). This is the only difference. (The statement that the base case assumes a higher NOx reduction technology was due to a misunderstanding between the first author and D.Lee, and has been removed.) The reference case was included in order to compare with previous studies, which also included all (i.e. both military and civil) aircraft. However, the focus on this case is now reduced in the new manuscript, e.g. by removing Figure 5 showing the location and magnitude of the maximum nitrogen perturbation. With the exception of the reference case, military aircraft are not included in any simulations. The main reason for not including military aircraft was that there are big uncertainties in their movements and they would not be involved in changes in flight routing anyway, i.e. they would not contribute to the perturbations with respect to the base case apart from changing the ‘background’ impact of aircraft in the ‘base’ case, which, however is

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assumed to be a small effect. <<

3 Minor comments I still find the MOZAIC comparison quite uninformative. It does not add much confidence to the quality of the model and the two Brunner papers already evaluated the models performance at flight altidudes extensively. The paper is quite lengthy as it is, therefore I would like to suggest to remove that part. Instead it would be appropriate to state the conclusions of the Brunner papers that are relevant for this paper.

>> The MOZAIC comparison has been removed. The reference to the Brunner et al. papers is now more detailed. Conclusions from the Brunner et al. papers are added. (see end of section 2) <<

The model is based on ECMWF meteorological fields. Then why does the model use the NCEP analysis for the tropopause heights?

>> The reason for this is merely historical: The NCEP reanalysis tropopause pressures were simply easier to retrieve. However, the importance of the NCEP tropopause height in CTM2 should not be overestimated. It serves ONLY as a decision height for which chemical scheme to call, the tropospheric or the stratospheric one, and has no other purpose in CTM2. As the two chemical schemes agree very well in the transition zone (sensitivity studies with varying tropopause heights have been made) and all chemical species are present and advected throughout the entire model domain, the importance of the tropopause height is rather small. In the tables stating tropospheric and stratospheric emissions it was used because it is well documented and easily available to everybody on the web (see note in the acknowledgements). <<

The tables 4 and 5 state numbers with 4 decimals. Two decimals would be more appropriate, in line with the precision of the results.

>> This is true. However, some of the small perturbations vanish (–> '0.00') when neglecting the 3rd and 4th decimals. Anyway, we have complied with the reviewer's

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suggestion and modified table 4 (now Table 3). Estimates for very small perturbations are mentioned in the text only. Table 5 has been removed following the suggestion of reviewer 1. <<

Figures 5, 7, and 11 lack legends (but have the lines decribed in the captions). Please add legends, especially Figure 11 would be much clearer.

>> Legends have been added to Figures 7 and 11. Figure 5 has been removed in an effort to comply with reviewer 1 to put less focus on the reference case. <<

On several parts the author use the phrase 'significant' where they seem to intend substantial or something alike. I would prefere to avoid 'significant' in those contexts as it has a distinct statistical meaning.

>> We have replaced 'significant' with the words 'substantial', 'large', or 'notable' at 12 places in the document. Confusion with the statistical meaning of the word should now be avoided. <<

p.12256. smaller ozone increase .. larger ozone increase use plural or add 'a'

>> Done. <<

p.12266. In July and October the increases are somewhat smaller but still significant. It is more that they are almost twice as small but still substantial.

>> Agreed. The sentence has been changed accordingly. <<

p.12277 Another investigation in this study has investigated... rephrase.

>> Done. <<

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