

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

Anonymous Referee #2

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1. General comments

This paper addresses the impact of subsonic aviation on atmospheric ozone and methane. Although many extensive studies regarding this top already exist, this paper addresses new interesting issues. The novelty of this study is the assessment of the impact of possible alternative flight routes and the application of a combined tropospheric and stratospheric CTM to address the impact of aviation. The methodology of the study is sound and the conclusions are justified by the results. However, the presentation is somewhat long-winded and could be more to the point. In connection to that, also the focus could be better. Some issues need further clarification. Therefore,

the paper deserves publication in ACP after revisions as recommended below.

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 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. 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 and/or 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.

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 worrisome 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 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 placed more

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below 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 NO_x 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)?

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

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

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

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

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

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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.

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

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

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