Atmos. Chem. Phys. Discuss., 7, S3853–S3856, 2007
www.atmos-chem-phys-discuss.net/7/S3853/2007/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Climate impact of supersonic air traffic: an approach to optimize a potential future supersonic fleet – results from the EU-project SCENIC" *by* V. Grewe et al.

V. Grewe et al.

Received and published: 8 August 2007

Reply to Referee 2

The referee was quite positive about the manuscript, however had some concerns, which we would like to comment. We have included most of them in the manuscript and hope that it is now more focused. - thanks to the referee.

1st comment: Lost focus From the beginning, we were totally aware that this paper has a dilemma. On the one side, the apporach from emissions to temperature change and ozone layer change is quite clear. And one could present the results straight forward focusing on temperature change. However, then we run into a 'show and

tell paper', which we wanted to avoid. Hence the more explanations you give the more out of focus the paper will be. Referee 1 even recommended more explanations, which makes focusing even more complicated.

We tried to avoid to loose the main theme, by introducing Figure 1, which shows the main logic of the methodology and paper.

At the beginning of each section, we introduced a paragraph referring to this Figure. We hope that this makes sure not to loose the thread. Additionally, we moved a more general part, which discusses metrics, to the introduction to have the methodology more focused.

2nd comment: paper on emissions in preparation We totally agree with the referee. It has been planned to submit a paper on the emissions to this Special Issue. Unfortunatly, it is not ready yet. We could refer to the SCENIC final report. However that is not openly available. We will clarify, whether a copy of that part of the SCENIC report, which deals with the emissions can be added to the paper as electronical supplement in the case the scientific paper is not yet submitted to ACPD.

3rd comment: Statistical significance Agreed. We didn't mention the statistical significance of the changes. They are routinely calculated, but not shown in our Figures. Except for some transition areas (changes from plus to minus) the results are all statistical significant at least at a 95% level. This has several reasons. First the calculated differences are quite large and often in the order of 10%. Second, the differences are based on a multi-annual mean after a long spin-up, which makes the signal more robust. Third, the meteorology between base case and perturbation scenarios is identical in the numerical simulations, since the feedback from the perturbations is omitted, which reduces noise significantly. Some of the models are chemical-transport models, which are based on meteorological data from weather forcast models. A short comment is given in section 4.2, 4.3

ACPD

7, S3853–S3856, 2007

and 4.5

4th Use of RF Totally agreed. Still, people are thinking in units of RF. That's the reason, why we first mention dT in the abstract and then RF, so that readers who are not so familiar with dT may have a chance to put the numbers into context. We added a discussion on dT induced by subsonic aircraft and expected total anthropogenic climate change: The total subsonic impact is in the order of 10%, whereas the supersonic replacement is in the order of 1% with respect to total expected anthropogenic climate change. (Inlcuded now in the conclusions)

5th comment on the impact of uncertainties on the results The impact of these uncertainties is taken into account and leads to an uncertainty range e.g. for the total RF of 9 to 29 mW/m2. We also included the model uncertainties in the analysis of the minimum impact scenario (see Fig. 10). We also investigated whether one of the main results, i.e. that water vapour is the most important climate agent is valid within the uncertainty range (min-max) (Fig. 9 and respective text passage). We added a line in the abstract to clarify this. A new section on "Discussion of uncertainties" is included.

A ranking of the models would require a detailed analysis of the model's differences with respect to observational data. Those are in many cases not available. E.g. turn-over point for ozone production to ozone loss; Residence time of NH perturbation. Other intercomparisons and model evaluations were performed. The general picture is that some models perform better in some aspects and worse in others. Therefore a ranking is always somehow arbitrary and we consider each model as equally important.

Specific comments: done. Thanks. We still call it normalized RF to be consistent with Sausen and Schumann (2000). The losses indicated in Tab. 4 weren't meant to be included (commented out) and probably wrong anyway. Figure 7 updated.

ACPD

7, S3853–S3856, 2007

Comment to conclusions: We have to correct one statement in the conclusions, which was wrong. The intercomparison between super and subsonic aircraft, which was based on the intercomparison of TRADEOFF and SCENIC results is incorrect for a couple of reasons:

- Considered time horizon is different, which leads to different CO_2 accumulation effects.
- The considered aircraft fleets are too different to be intercomparable. We replace it by a passage, which refers to an upcoming paper (to be submitted in August 2007), in which SCENIC supersonic aircraft are directly intercompared with respective subsonic aircraft.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 6143, 2007.