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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A strategy for climate evaluation of aircraft technology: an efficient climate impact assessment tool – AirClim" *by* V. Grewe and A. Stenke

Anonymous Referee #2

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This paper is potentially worthwhile and with changes can make an important contribution to assessing aviation impacts. However, I have some strong misgivings about the current draft and methodology.

I have four major concerns 1. The paper presents the airclim model allowing people to explore differing aviation growth scenarios, without the need for a sophisticated modelling effort. I strongly feel without properly addressing uncertainty in the methodology, the paper is of little value and will only serve to confuse potential users of the system. Worse, if people use the AIRCLIM model to make policy choices they could be misled into making the wrong ones. A proper and thorough uncertainty assessment is

really needed. In particular, they make a bold statement about NOx vs. CO2 in the abstract. This seems to be the result of having i) an incorrectly small value of the negative methane forcing and ii) a very high efficacy for ozone, much higher than other models. Therefore, I don’t think this statement is at all robust as is potentially very misleading. There are several (at least 3) aviation NOx GWP estimates and they are all very different (e.g. Forster et al., 2007: IPCC AR4, Chapter 2, Table 2.15). Likewise, the ozone efficacy could also be smaller than one (Figure 2.19 of Forster et al. 2007). The AIRCLIM model may correctly simulate the ECHAM model (but still seems to underestimate methane cooling), but without examination of uncertainties it will be of little use to its intended readership. The bizarre thing is that the authors acknowledge the need for an uncertainty analysis in the introduction – line 11, p. 12187 and imply they do one. Further on page 12913, line 11 – they say “see uncertainty analysis below”, but then there never seems to be one! On page 12205 they acknowledge that AIRCLIM doesn’t address uncertainty. I think they need to practise what they preach and do a proper uncertainty analysis and accompany this with clear discussion of other uncertainties, comparing results with available literature wherever possible

2. Related to 1), the paper does little to compare and contrast its methodology to other approaches in the literature: GWP, GTP, Wit et al. etc. – I endorse the references and points made by referee one here that a forward looking metric may be more useful for assessing future emissions.

Wit R. C. N., Boon B. H., van Velzen A., Cames M., Deuber O. and Lee D. S. 2005: Giving wings to emission trading. Inclusion of aviation under the European emission trading system (ETS): design and impacts. CE-Delft, No. ENC.C.2/ETU/2004/0074r, the Netherlands.

3. The paper and figures seem quite disorganized. I’m not sure if figures are referenced in order (for example). I found it very hard going to interpret the figures and find out what was going on. I suggest careful rewriting, restructuring and careful

ACPD
7, S5695–S5697, 2007

consideration of the figures

4. I question whether enough has been done to verify the model, the model gives time dependent changes. But uses time-slice data from 2050 to determine parameters, surely to model change with time at least two time-slices at either end of the time period would be needed? This is particularly relevant as I endorse referee 1’s comment s about use of a single time constant for both CO2 and the ocean temperature change seems wrong and unnecessarily simplistic? Also, should a longer “perturbation lifetime” be used for methane of around 12 years – Again see chapter 2 of the ipcc report, Table 2.14 discusses methane lifetime and the CO2 response function.

Other comments

1. Can you explain what goes wrong with the models methane prediction?

2. Last three bar-chat figures were difficult to interpret. I think RF and temperature changes should not be shown on same plots

3. I think that more effort is needed to compare with previous studies – e.g. the 6.3 Wm-2 forcing for 100% contrail coverage on page 12196

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 12185, 2007.