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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 #1

Received and published: 2 October 2007

General The paper presents a potentially useful model framework for analysis of the overall climate effects of aviation. The authors have made an impressive effort to integrate knowledge from detailed and complex studies into a tool that can be used for an overall assessment. This framework can be used for a systematic analysis of different choices of technology, regulations and scenarios. Thus, this type of tool can be very useful and important. However, the work is rather complex and the presentation should be improved, as it is often difficult to follow what the authors have done. I have several questions and suggestions for clarifications that I think will improve the paper and make it more accessible.

The paper provides comparisons of various effects, scenarios and types of airplanes (supersonic vs subsonic). The comparisons are useful in this context. But the authors should make an effort to show that these comparisons are valid for the assumed emission histories and scenarios since the responses and differences depends on the emission paths for the long-lived components (e.g. the factor 5 for supersonic vs subsonic for 2100 would be different for a different scenario and evaluation year). If the task is to compare activity levels, then a forward-looking perspective based on emissions in a chosen year may be of more relevance. See Forster et al. (2006; 2007) for a discussion . An approach based on pulses and future deltaT or integrated RF may then be useful. Please, include a brief discussion on the application and limitations of the obtained results.

(Forster, P. M. D., K. P. Shine, and N. Stuber (2006), It is premature to include non-CO2 effects of aviation in emission trading schemes, Atmospheric Environment, 40, 1117-1121.

Forster, P. M. D., K. P. Shine, and N. Stuber (2007), Corrigendum to: “It is premature to include non- CO2 effects of aviation in emission trading schemes” (vol 40, pg 1117, 2006), Atmospheric Environment, 41, 3941-3941.)

The title is long and somewhat ambiguous. Consider shortening. Something like this? “AirClim: An efficient tool for climate evaluation of aircraft technology”

Specific comments

Page 12186 Line 3: Add global before “climate”.

Lines 17-19: The sentence “The estimate of….” is unclear. Perturbation times for what components?

Line 22: The sentence starting “For subsonic aircraft….” seems incomplete. Is something missing about NOx at the end of the abstract?

Line 28: Explain the unit “nm”.

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The abstract focuses on CO2 and NOx. Why not mention other warming effects of aviation?

Page 12187 Lines 5-8: The NOx-O3 issue is mentioned. This is just one element, and I suggest also mentioning just very briefly other mechanisms; e.g. contrails.

Line 10: The wording “provide some metrics” is ambiguous. Something like this could work better: “Provide estimate of responses in chosen indicators or metrics of climate change.”

Line 21: Regarding “….and a time integrated radiative forcing including its efficacy….”. Unclear where and by whom this is chosen to be appropriate. Here in this work or in other studies?

Page 12190 Lines 13-16: Which scenario is assumed?

Page 12191 Line 29: A brief explanation of the methodology by Stuber et al. would be useful.

Page 12192 Line 12: How do the efficacies chosen here compare to other studies? And how critical is this for the results? Please discuss briefly.

Line 20: Here you mention a UV-change metric. But this is not something that is picked up later, as far as I can see. Please clarify.

Page 12193 Lines 26-27: Regarding the wording “to obtain a metric for climate change”. The point is not to obtain a metric – this can be established in generic terms – but to obtain a response in the chosen impact parameter which is, in this work, surface mean temperature.

Page 12194 Lines 7-8: Please reconsider the model used to calculate the relation between CO2 emissions and concentrations. This is important since CO2 is the gas with the largest RF. Applying a simple exponential decay with a constant lifetime (i.e. one time constant) for this gas with its complex behavior is, in my opinion, not sufficient.

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There are several simplified models in the literature which use several time constants, and as far as I can see this should be a significant and required improvement. If the authors refrain from this, they should give convincing arguments with numbers for deviation from a more complex modelling of this gas.

Line 10: The notation Tconst + гspecies is confusing. If Tconst is 2050, do you, in the case of methane, just calculate for 2050 + one lifetime of methane? Please explain.

Page 12195 Lines 12-14: What is the background level of methane; i.e. what scenario is assumed? The sentence “A temporal… scaling with normalized CO2 emissions, ….” is unclear.

Page 12196 Line 15: Please discuss very briefly what the effect of only one time constant for the ocean may have on the results? How good is this simplification?

Consider changing the notations for temperature and time (T, deltaT, t…). May also use ‘yr’ instead of ‘a’.

Regarding equations 3-5: Please explain why you use one time constant for the climate response, and the effect that this choice of constant has. (36.8 yr vs other choices in the literature; e.g. Shine et al. 2005 use a much smaller value). The effect of including additional time constants could be briefly mentioned.

Shine et al., 2005. Alternatives to the Global Warming Potential for comparing climate impacts of emissions of greenhouse gases. Climatic Change, 68 (3): pp. 281-302.

Page 12197 Line 1: What is the magnitude of the effect of omitting saturation effects? Line 11: figure 7 is very useful for communicating the features and the behavior of the system. See previous comment on time constants for CO2 and the climate system.

Page 12199 Lines 5-9: A comparison of these results with other results in the literature would be useful. Please, consider the option of presenting the sensitivities in a table for the various regions and levels; this can be useful for other groups doing research in

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this field.

Line 13: Please explain better why the strength is secondary. A reference to Stevenson et al. would be relevant.

Is the primary mode for ozone taken into account? See papers by Prather and Stevenson et al., 2004 (JGR) and Berntsen et al. 2005 (Tellus).

Page 12200 Line 25: The last sentence is somewhat unclear.

Page 12201 Lines 23-24: Please discuss why methane shows this deviation; not only that there is a long chain, but also the factors that affect the results along the chain. This doesn’t have to be a long discussion.

Lines 23-24: Please discuss the implications of these deviations for the application and reliability of the results.

Line 26: the conclusion “sufficiently well” needs more basis. A more solid basis would strengthen the paper; see two previous comments. Some more attention to the O3 results would also be good.

Page 12202 Line 22: Please explain this better (exponential interpolation for fuel use).

Lines 24-26: Please discuss why there is such a difference between the two studies with respect to methane and ozone. I don’t think it is sufficient to refer to the figures.

Line 26: And what is the uncertainty range that is referred to?

Table 6: I don’t think you should use the sum alone to do comparisons. Please give more attention to the deviations for the various agents. For CO2 (the agent with largest RF) the deviation is notable. The compensating effects of O3 and CH4 for NOx emissions can also be discussed.

Page 12203 Lines 5-8: Please explain better why you multiply by 0.25 and the justifi-

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cation for doing this. Is this just a scaling (tuning) to adjust the results?

Lines 13-14: Please note that CO2 will not reach steady state if you use a more realistic CO2 model. See comment above with recommendation for a somewhat less simple treatment of CO2.

Line 18-19: Unclear what you mean by “no other meaningful interpretation….”.

Line 24: I guess this is critically dependent on the value of r? And what values are used for the other agents?

Fig 10: The figure is difficult to read. Consider the option of splitting it into two separate figures. Explain the CH4 + O3 bars vs the O3 bar. Is the primary mode response of O3 included here? Please also discuss the impacts of unrealized effects of CO2 for 2100 vs 2250, as this may explain some of the behavior here.

Line 28: See previous comment regarding steady state and CO2.

Page 12206 Line 21: the use of the word ‘metric’ is somewhat misleading. Could say ‘chosen impact parameter’ instead.

Page 12207 Lines 11-13: How would other values for efficacy affect the results obtained?

Page 12208 Line 9: The word ‘lifetime’ reflects the simple treatment of CO2 here. The lifetime of CO2 is just a few years, but what you have in mind, and try to model, is the lifetime of the perturbation or the adjustment time. Please, improve wording.

Fig 12: Try to improve this figure. Consider splitting the figure and removing the “factor” bars. This factor may be misunderstood by some readers (see second general comment); i.e. it may be taken out of context and used for considerations of future effects. Also consider the option of introducing a secondary axis instead

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of dividing by 3 for the total.

A relevant reference that I would suggest including:

“Assessing the Impact of Aviation on Climate,” K. Marais, S. P. Lukachko, M. Jun, A. Mahashabde, and I. A. Waitz, to appear in Meteorologische Zeitschrift, 2007.

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