

Review: Huszar et al., Modeling the present and future impact of aviation on climate: an AOGCM approach with online coupled chemistry

Overall comment

In general the question the authors state is interesting, but the way the results are presented is not sufficient for publication in ACP. The lack of comparison with earlier studies, discussion and explanation of differences to other studies is one main point of criticism. Furthermore, the level of significance to test the results is insufficient, significance of results on the at least 95% confidence level should be shown. The results are not explained properly, many features in the results figures remain unexplained, many questions the reader will have remain unanswered. It is hard to draw a conclusion from this paper.

In its actual status the manuscript is not suitable for publication, therefore I would recommend to reject the manuscript for publication in its actual status.

If all the points mentioned and all comments are considered, the paper could possibly be resubmitted and undergo a new review process.

General comments

1. Does the paper address relevant scientific questions within the scope of ACP?

The question the authors of this paper raise is interesting: Whether it makes a difference, if an AOGCM with online coupled chemistry is used to calculate aviation induced chemical perturbations and the respective temperature response within a transient simulation or if chemical effects of aviation emissions are determined by means of a CTM beforehand and the climate response is calculated offline, using either simplified response models or an AOGCM without online-chemistry? Unfortunately, the authors do not provide a convincing answer to the question and make no comparison to other studies.

2. Does the paper present novel concepts, ideas, tools, or data? Are the results sufficient to support the interpretations and conclusions?

The authors use a strongly simplified chemistry in the lower troposphere, however, they do not compare their results (e.g. chemical perturbations) to other studies using CTMs with more sophisticated chemistry schemes nor do they prove that the model even produces reasonable results with respect to e.g. NO_x effects.

It is highly unusual to drive a climate model with aviation emissions, which are not scaled. The scaling of forcings in earlier studies was necessary in order to obtain statistically significant results and to make sure to interpret more than just noise (Ponater et al., 2005, Rap et al, 2010 (GRL), Olivie et al., 2012). I highly doubt that the authors would receive any significant results in the present study, if they tested for significance on the 95% or 99% confidence level. At least 95% confidence level should be shown, the 90% confidence level is not sufficient. I also doubt if the degrees of freedom are reduced (Zwiers and von Storch, 1995) in case of serial correlation in the time development (as evident in Fig. 6). Maybe other statistical methods should be considered (e.g. pattern recognition, multivariate statistics, ...), e.g. von Storch & Navarra, (Chapter 8).

The problem of small climate forcings and questionable significance of results even gets worse, as the authors use transient simulations with a deep ocean to derive the temperature response. It was shown by Ponater et al., 2005 (GRL), that e.g. for aviation CO₂ and contrail forcings, the temperature response in a transient simulation is only about 25-30% of the response of equilibrium simulations. Also Boer and Yu (2003) and Carson (1999) have studied ratios of transient to equilibrium surface temperature response.

3. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Are the number and quality of references appropriate?

In general, if the authors give references, usually it is just some arbitrary recent reference, but in most cases not the original or most appropriate reference (e.g. page 3819, line 18; page 3820, line 3).

Furthermore, the authors don't give proper credit to related work (e.g. page 3839, line 15 ff, "Our experiments indicate, that the temperature response.....different geographical pattern than ... radiative forcing". This is not a new finding, the authors should refer to the respective literature, e.g. Rind et al. (2000) found that the temperature response is dominated by the feedbacks and shows little geographic relationship to e.g. contrail coverage.... or other related studies, e.g. Hansen et al., 1997, Joshi et al., 2003, Hansen et al., 2005, Ponater et al., 2005, etc.).

Generally there is almost no comparison of their results to earlier studies or any explanation why results look as they do or any discussion. This is extremely unsatisfactory, as results are in conflict with previous studies and call for explanation (e.g. the CO₂ related temperature response pattern for 2031.2050). So there is a general lack of consideration of related work, of comparison, discussion and referencing to other studies.

4. Are the scientific methods and assumptions valid and clearly outlined? Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

In general, the model description and description of methodologies is not very comprehensive and should be more detailed, e.g. what chemistry processes are included.

It is not clear, why the chemistry in the lower troposphere is relaxed towards climatological values, and why the chemistry in the lower troposphere is not calculated explicitly? Aviation emissions and their products are transported downwards to the lower troposphere and result in perturbations of surface concentrations, e.g. O₃ (see e.g. IPCC, 1999, Grewe et al., 2002, Gauss et al., 2006, Köhler et al., 2008...) Why are only OH and CO₂ concentrations relaxed towards climatological values but not O₃ concentrations? How are seasonal variations of surface OH concentrations treated? How can the authors make sure, that they don't include effects twice, e.g. when emissions and related OH is transported downwards into the lower troposphere or when prescribed OH is transported to higher levels...

The way, the authors implement contrail induced clouds is very rudimentary. The methodology is not explained in detail, and it is not proven, that the results are reliable and comparable to other studies. E.g. it is not explained, whether the contrail conditions wrt temperature and humidity are calculated from monthly mean values or from each timestep, etc. As the radiative forcing of contrails and contrail cirrus depends on the contrail coverage and the optical depth of contrails, which itself depends on ice water content, it is important, how the ice water mixing ratio is distributed with respect to latitude and altitude. It is not clear from the explanation, whether the ice mixing ratio which is added to the natural cloud ice mixing ratio is the same everywhere or whether there are any latitudinal, altitudinal or seasonal variations. This should be shown and compared to other studies. Furthermore, there might be differences in contrail ice water and its distribution in future climate?

The authors state, they want to ensure, that the chosen model configuration is able to give reasonable results without significant biases. However, no proof for this is given, particularly not with respect to the simulation of aviation effects, and that their simplifications wrt chemistry or contrail induced clouds give reasonable results. I would expect one section, where the aviation effect is compared with other available studies for the year 2000 with respect to the distribution and magnitude of aviation perturbations (e.g. CO₂ concentrations, the NO_x and O₃ concentrations, CH₄ lifetime change, the contrail ice water mixing ratios, ozone and methane radiative forcing). Differences to other studies should be quantified and explained and discussed in detail. Only then, such a model configuration with such strong simplifications can be considered to be suitable for further studies. Until such a comparison is made and differences are quantified and discussed, the interpretation of any further results is not worthwhile.

5. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

The evolution of the surface temperature for default experiments from 1860 to 2100 must be compared to similar studies in the literature. The performance of the model with respect to transient simulations and surface temperature response should be evaluated. Differences to other studies should be quantified, explained and discussed. The bias in surface temperatures because of the sea-ice extent must be quantified and discussed. Why is there a difference in surface temperature by the end of the century in the DEFchem and the

DEFnochem? Why is the Arctic Sea-ice only overestimated in the DEFnochem simulation? Could the parameterisation of the sea-ice flux be adjusted to the resolution of the model? This paragraph (3.1) is confusing and relations between DEFchem. and DEFnochem and the resolution of the model should be clarified.

The results section generally lacks any discussion and explanation. Significance testing on the results shown is not done with proper tools and is not interpreted with the necessary care. And some of the trends they interpret are hardly to be seen, e.g. trends in 100 hPa (Fig. 6). Is it possible, that a part of the 100 hPa results is in the troposphere in the tropics and in the stratosphere in the extratropics, so that stratospheric trends and tropospheric trends are mixed together and therefore give no proper trend?

So many features of Figure 6 are not explained at all. E.g. why is the CO₂ response mostly negative from 2020 until 2060...? And this is only one example...

In Figure 7 very patchy patterns are shown, in most cases the results are significant only in very small regions. The pattern and the distribution of temperature response is not explained. E.g. why is there a cooling from CO₂ over the North Pole? No explanation is given. There is no comparison to other studies. The resulting patterns of responses are not typical, e.g. if you look at temperature responses in Hansen et al., (2005) for a CO₂ doubling, the largest responses appear very clearly over the north and south pole and over the continents and they are positive. In the present study large responses are also shown over the South Atlantic and over the Pacific.

As the NO_x-effect is so small (probably underestimated?), the “non-CO₂-effect” almost only consists of the CIC-effect. It is not worth showing the “non-CO₂” effect separately? Furthermore, under “non-CO₂-effect” I would understand all effects which are not CO₂, namely O₃, CH₄, H₂O, CIC, Aerosols, ...

6. Are substantial conclusions reached?

Overall, I find it very hard to draw any meaningful conclusion from the presented results or to find a take-away-message in this paper.

7. Does the title clearly reflect the contents of the paper?

The title should reflect that the chemistry in the model is strongly simplified.

8. Does the abstract provide a concise and complete summary?

The abstract should give information of advantages and disadvantages to earlier approaches. And summarize the outcome of differences to earlier approaches.

9. Is the overall presentation well structured and clear?

Ok.

10. Is the language fluent and precise?

Ok.

11. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Ok.

12. Is the amount and quality of supplementary material appropriate?

Ok.

Selected references:

Hansen et al., 1997: Radiative forcing and climate response, JGR.

Hansen et al., 2005: Efficacy of climate forcings, JGR.

Joshi et al., 2003: A comparison of climate response to different radiative forcings in three general circulation models: towards an improved metric of climate change. Clim Dyn.

Ponater et al., 2005: On contrail climate sensitivity, GRL.

Von Storch & Navarra: Analysis of Climate Variability - Applications of Statistical Techniques, 1999, 342pp.