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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modeling the present and future impact of aviation on climate: an AOGCM approach with online coupled chemistry" *by* P. Huszar et al.

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Response to Referee 2 comments

Referee's comments are highlighted in italics.

- *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.

Thank you for your detailed review of our manuscript and for all your suggestions to improve our paper. We have addressed all your comments and below are our responses to each of them. We have also modified our text in order to address the various issues raised. More specifically, we have first included in our text further and more detailed comparisons with earlier studies. In doing so, we have underlined more the particularities of our study that distinguish it from previous ones. In addition, we have added further analyses of the differences or similarities we find with other results. Furthermore, we now show results with a 95% confidence level of confidence.Please note that the Figure numbers below refer to the original ACPD version of the manuscript.

- *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.

  The purpose of this paper is not per se to make a comparison study between results obtained with an AOGCM with online chemistry and results obtained with

a CTM beforehand and the climate response to aviation emissions calculated offline. Here the intention is to evaluate the transient response of climate (temperature) to the aviation emissions using the most advanced tool available for this purpose, i.e. using an AOGCM where the chemistry is online and interactively (i.e. with radiative feedbacks) coupled to the atmospheric model. This approach has not yet been presented – according to our knowledge – in any other study regarding the aviation emissions, and we consider this as an important step forward. The assumption here is that a climate model coupled with chemistry is by essence closer to the 'real world' by the fact that the chemical evolution of a number of constituents is one of the physical processes described by the model. We are aware that such a tool is quite sensitive to use in the study of the impact of aviation upon climate where effects are small and therefore harder to identify.

We have developed this tool in the context of various programs, among which the CCMVal international project (WCRP Word Climate Research Programme SPARC), and the objective here is to highlight the results obtained with our sensitivity studies to the aviation emissions. To our knowledge, this is one of the first studies about the impact of aviation emissions performed with a full climatic model that considers both the atmosphere, up to 70 km including a chemical scheme fully coupled to the radiative scheme, and the entire ocean. One of the other novelties here is that transient simulations have been performed, over 160 years (1940-2100), as ensemble simulations. Spatial resolutions of the atmosphere in our simulations, both in the horizontal where we chose a T42 truncation (the corresponding Gaussian grid has 128 longitudes and 64 latitudes i.e. 2.8spaced grid points), and in the vertical, where 60 levels cover the atmosphere from the surface up to mesosphere (0.07 hPa, 24 levels above 100 hPa and 8 levels in the upper troposphere and lower stratosphere (UTLS)), correspond to those of state of the art chemistry-climate models. One of the other novelties here is that transient simulations have been performed, over 160 years (1940-2100), as ensemble simulations. These model and experimental setups improve

on previous studies (see Lee et al. (2010) for a recent and thorough review of the assessment of aviation on climate).

- *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. NOx effects.

  It is true that the chemistry included so far in our General Circulation Model (GCM) does not include the detailed non-methane hydrocarbon (NMHC) chemistry specific of the lower troposphere. However, for the rest of the atmosphere, including the upper free troposphere from 560 hPa up to the lower mesosphere (about 70 km), the chemical reactions of our scheme correspond to that of state-of-the-art chemistry-climate models (see Morgenstern et al. (2010); SPARC (2010)). This scheme calculates the evolution of 55 species using 160 gas-phase reactions, with the JPL chemical kinetics of Sander et al. (2006).

  The chemistry is computed down to the 560 hPa level while for higher pressures the mixing ratios of a number of species (namely N2O, CH4, CO, CO2, CFC11, CFC12, CFC113, CCl4, CH3CCl3, CH3Cl, HCFC22, CH3Br, H1211, H1301, Ox, O3, Cly, Bry, NOy) are relaxed towards evolving global mean surface abundances (see SPARC (2010) for the ozone depleting substances and greenhouse gases, and the CNRM-CCM technical documentation for the other compounds). Explicit wash-out of chemical species is not considered in this version of the model, and neither are convective and turbulent transport. In addition, three types of particles are considered in the heterogeneous reactions, liquid stratospheric aerosols and Polar Stratospheric Clouds (PSCs) that include water ice and NAT (Nitric Acid

Trihydrate) (see Teyssèdre et al. (2007); Michou et al. (2011); Morgenstern et al. (2010) for further details).

One has to note that state-of-the art CCMs rarely consider tropospheric chemistry because of computer resources (among the 18 models of CCMVal-2 only 3 represented tropospheric chemical reactions, see Morgenstern et al. (2010)). The chemical scheme we use is fully convenient for the study of all the processes within the stratosphere, the UTLS and down to the middle troposphere, the latter two regions being of the most importance to studying the aviation impact. This scheme has been evaluated in a large number of publications as the CCMVal-2 effort was aimed at assessing CCMs performances, both individually and collectively, among 17 other CCMs models. The evaluated processes cover radiation, stratospheric dynamics, transport in the stratosphere, stratospheric chemistry, UTLS, natural variability of stratospheric ozone, long-term projections of stratospheric ozone, and the effects of the stratosphere on the troposphere. A number of CCMVal-2 related publications appear in Michou et al. (2011).

Lee et al. (2010) report on the impact of considering the NMHC chemistry on aircraft-induced ozone changes, quantifying the difference in O_3 production in the lower and free troposphere. A couple of studies have looked at this issue; the most recent one, Kentarchos and Roelofs (2002) indicate that their chemistry-climate model simulates up to a 12% increase in O_3 when including the NMHC chemistry in their model. We have underlined in our paper the possible impact of not considering the NMHC specific reactions in our model.

As logically suggested by the other reviewer, we have shifted the paragraph on the perturbations to the chemistry due to aviation emissions of CO_2, ozone and NO_x before the sections that present the temperature response, and we have included additional comparisons to previous studies. In general, the chemical response in our simulations is less pronounced than in previous studies. One has to note however that, in each of our simulations the chemistry is driven by the

meteorology specific to that simulation. In contrast, in most previous studies the meteorology was fixed while aviation emissions were perturbed. Nevertheless, our chemical response for the 1991-2010 period shows that it goes in line with previous studies, however with a smaller magnitude and significance. This is explained and discussed in more details in the revised manuscript.

- *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.*

  We agree that by scaling the emissions, it is easier to get the significance of the results. However, scaling the forcing is also a questionable method (even Olivie et al., (2012) who use scaled emissions, warn the reader that the actual impact might be different from the estimate based on scaled emissions). In our study, we have chosen not to make any hypothesis on the linearity of the response and to

use the model in a more realistic way. The drawbacks are that the results are less evident at first glance and that the significance of the results really needs to be shown.ÂăTo reduce the noise, we have chosen to run simulations in ensemble-mode. Using several members allows to increase the sample size. From figure 6, if one looks at ensemble members (not ensemble mean), the autocorrelation is weak. Moreover, on figure 7, it should be acknowledged that the test is made on a grid point basis and that even the ensemble mean on a grid point basis would be be less auto-correlated than the global mean shown on figure 6.ÂăWe agree that the 90% significance level was a too low criteria and we present now the results using the 95% significance level. The variance for the significant testing has been recalculated from all members of the ensemble and not the ensemble mean only. All this has decreased the significance of the results but many of the important features remain.

We also agree that a transient response has to be distinguished from an equilibrium response. Both are of interest. In our study, the aim is to assess the impact of aviation emissions on the XXIst century, thus we are interested in the transient response. We believe that it is what will happen in the near future and is therefore of high interest. The equilibrium climate response is more a theoretical response and is not our objective here. This makes the comparison with studies analysing equilibirum simulations not fair and we have raised this difference in the text.

Our simulations contribute to qualifying and quantifying how an AOGCM with online chemistry responds to presence and changes of aviation emissions. We have done that by the means of ensemble simulations. There is no 'a priori' knowledge of how many members are required in order to get the signal information from the noise (A. Ribes personal communication), so we decided, on the basis of our computer and time resources constraints, to run ensembles with 3 members each. Our results and analyses show that if this number of members does not seem to be sufficient for the surface temperature signal, however it appears more

or less appropriate for the temperature signal at higher altitudes.

It should also be acknowledged that the ensemble generation method is also probably not fully adequate in this study. The different members of each ensemble have been taken from the same corresponding 20^{th} century simulation by only shifting the ocean state by one year. However, a one year shift does not provide a very different oceanic state in term of climate variability and thus the different members are started from close initial states (considering the multidecadal variability of the ocean). This may explain why the CO_2 signal on sea-ice may be close in two members. The method of ensemble generation is thus an important point to take care in future studies.

- *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.).

  We admit that our referencing of related work was sometimes insufficient. We improved this in the revised text with more relevant and more specific references. We thank to the reviewer for suggestions for references that we had neglected in a first version of the manuscript.

  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.

Following your review, we have put substancial efforts in the revised manuscript to analyse, as far as possible, the results obtained, in particular in the light of previous studies. We hope that our revised manuscript will be found to be adequate. However, we have to be clear that there is a lack of very comparable model and simulation set-ups in the literature so far, thus limiting comparisons. This should change as modelling tools evolve.

- *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, Koehler 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

  We have included above, in our response to your question 2, further details on

our chemistry scheme, incorporating the entire list of species for which we relax mixing ratios towards yearly climatological values between the surface and 560 hPa.

In answer to your question here, the choice of relaxing mixing ratios is mandatory in place of the tropospheric processes that we do not consider, i.e. dry deposition, wet deposition, diffusion and convection. All this is scientifically coherent with not considering the NMHC detailed chemistry of the lower troposphere. We exclude these chemical/physical processes from our simulations in the sake of computing time, vital in climate modelling where transient simulations are performed.

So, we do relax CO_2 and O_3 mixing ratios. Then the OH mixing ratios, not relaxed considering OH very short lifetime, adjust themselves. This way of taking into account the lower troposphere is common among the CCMVal-2 CCMs (see SPARC (2010)).

- *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 NOx 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.

Implementation of the parameterization of contrails follows that described in Olivie et al., (2012), parameterization validated in the same paper. As you request, in the revised manuscript we have detailed more the particularities of this contrail treatment, and we have included some comparisons with previous studies (e.g., contrail and induced cirrus cloudiness RF distribution).

The purpose of our paper is not to provide additional validation of our model configuration, validation that appears in other articles, but to build on these articles and present the results obtained through sensitivity to the aviation-related emissions simulations. Our simulations may include deficiencies intrinsic to our model, but we believe these deficiencies are scientifically acceptable and do not prevent us from showing and analyzing results from our sensitivity simulations.

- *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 tem-*

peratures 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.

We agree that the paragraph 3.1 was confusing, and we have rewritten it. We present the 1860–2100 global mean surface temperature evolution as an illustration of the mean present and future climate. We also show the Arctic sea-ice extent evolution for September (which is the most sensitive to the climate change) and compare both results (temperature and sea-ice) with those of Olivie et al., (2012). We found that the most important global mean temperature differences are caused by differences in the sea-ice amount.

One of the results of our simulations is that the sea-ice extent is very sensitive to details in the model, and is not always responding according to scientific expectations. However, unrealistic large variations in the sea-ice coverage of the Northern latitudes have been seen in other AOGCM simulations that include on-line chemistry (O. Morgenstern personnal communication). One has to note that such model configurations are so far quite unique. The CCMI project (Chemistry-Climate Initiative), with a launching workshop in Davos in May 2012, is putting together a coordinated effort as a follow up of the CCMVal-2 and ACCMIP projects, and should analyze outputs from a number of CCMs performing atmospheric-ocean coupled simulations (see Eyring et al., (2013) and http://www.pa.op.dlr.de/CCMI/). This will allow improvements in the understanding of unexpected behaviors of such models.

- *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 NOx-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' effect separately? Furthermore, under 'non-CO2-effect' I would understand all effects which are not CO2, namely O3, CH4, H2O, CIC, Aerosols, ...

We now present results with a significance level of 95%, and this reduces a little the zones statistically significant. However, the overall picture does not change much, and provides interesting and rather new information on the modelling perturbation approach we used for studying the impact of aviation upon climate.

Figure 6. is analysed in more detail, focusing on the negative CO_2 response and leaving out some of the hardly noticeable features (e.g., trends at the 100 hPa level). In the Discussion, we attribute some of the surface temperature changes to changes in the Arctic sea-ice. Furthermore, we impute the North Pole cooling "due to" aviation CO_2 largely to the sea-ice extent. We warn the reader that the

surface temperature changes over the Arctics have to be viewed with caution as the sea-ice variation extent that occurred in our simulations might have greatly influenced the surface temperatures in parallell to the impacts of the aviation emissions.

It is true that due to a negligible NOx effect, the non-CO_2 impact resembles the CIC impact. However, we would like to keep presenting them separately to show this finding explicitely. Regarding other potential aviation forcing agents, we did not account for aerosols because of their low expected impact (as shown by e.g. Olivie et al., (2012)), and in the case of water vapor, we accounted for it only in the form of contrail ice particles and not as gaseous water vapor, assuming again a negligible radiative impact of the latter, as confirmed recently by Wilcox et al. (2012).

- *6. Are substantial conclusions reached? Overall, I find it very hard to draw any meaningful conclusion from the presented results or to find a takeaway- message in this paper.*

  The revised manuscript defines the goal of our study more precisely, i.e., the idea to study the impact of aviation upon climate performing transient simulations with a state-of-the-art AOGCM including on-line chemistry. The revised manuscript describes the setup for these simulations, i.e. no scaling of aviation emissions, and ensemble simulations. The discussion brings into attention the various aviation temperature impacts. The characteristics and significance of the impacts are presented, and appear as valuable information for further studies that will use similar model approaches.

- *7. Does the title clearly reflect the contents of the paper? The title should reflect that the chemistry in the model is strongly simplified.*

  We suggest to leave the title as is. Indeed, if our chemistry scheme ignores the NMHC chemistry, it includes the processes required to represent the chemical

evolution of the atmosphere down to the mid-troposphere. We can also add that a very similar scheme is used by the CMAM Canadian CCM which is one of the models of the ACCMIP project, aimed at analysing the chemical evolution of the troposphere down to the surface. The CMAM results do compare quite favorably to the results from the other models of ACCMIP (CCM or CTMs) in the various papers published so far, eventhough these other models include more complex chemistry schemes (see http://www.giss.nasa.gov/projects/accmip/). One has to note also that the complexity of the various tropospheric chemistry schemes span quite a large range. The details we have added in our paper indicate now very clearly, we think, which kind of chemistry scheme we have been using in our simulations.

- *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.*

  The abstract has been modified in accordance with the revised text.

- *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.*

Please find below a list of the most important changes made in the revised manuscript:

- we rearranged the list of authors to reflect the additional work done on this paper during the review procedure.

- the Introduction is more precise in defining the goals of the study.
- the model description is more detailed, with a focus on the chemistry scheme. The contrail parameterization is also more lengthly described, and the contrail and induced cirrus RF is compared to previous studies.
- the section on the evolution of the mean climate is now analyzed in more detail, and presents, besides the temperature, the Arctic sea-ice extent which influences greatly the surface temperature.
- the section on the CO_2, NO_x and ozone perturbations has been shifted before the ones on the impact of aviation upon climate. It is compared (there or in the discussion) to previous studies and discussed in more detail.
- in the sections on the impacts upon temperature (globally, zonally, monthly), the significance has been recalculated and colors indicate now the 95% level of significance. Analyses have been modified accordingly.
- the discussion has been extended with more comparisons and analyses. Figures have been added among which one shows the mean impact on the Arctic sea-ice extent of different aviation emissions, which we found related very closely to features of the impact on the global mean temperature. The aviation NO_x induced chemical perturbation is also presented and analyzed in terms of its variability.

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