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Atmospheric Chemistry and Physics Discussions

Interactive comment on "

Modeling the climate impact of road transport, maritime shipping and aviation over the period 1860–2100 with an AOGCM" *by* D. J. L. Olivié et al.

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ACPD
11, C12232–C12247, 2011

Answer to reviewer #2

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We thank the reviewer for the constructive comments on the manuscript. Please find below our responses to the comments.

1. *The approach is off-line (prescribe concentrations of forcing agents) and therefore what has been assumed throughout is that the resultant climate changes do not feed back and affect the forcing mechanisms. Is this valid? For example, in a region where there is a large change in predicted precipitation, that would have large effects on the aerosol forcing.*

The approach for the climate forcers is indeed off-line. In reality, climate change will impact the climate forcers, and so possibly create negative or positive feedbacks. E.g., the long-lived GHG CO_2 will cause a change in temperature which will change the uptake of CO_2 by vegetation and by the ocean, causing possible feedbacks (Friedlingstein et al., 2003, 2006; Gregory et al., 2009). But also short-lived climate forcers can be modified by the impact of changes in the climate, e.g., humidity changes will impact the OH concentrations, which in turn can impact on CH_4 or on the formation of secondary aerosols like sulfate. In addition, as wet deposition is an important removal mechanism for atmospheric aerosols, modifications in precipitation can also

modify the distribution of aerosols. Horowitz (2006) showed that simulated aerosol concentrations are highly sensitive to the rate of aerosol wet removal, which is poorly known.

We see our approach as a first order approach, where impacts from modifications in emissions on concentration distributions are taken into account, but where impacts of climate change on these distributions are not taken into account. Horowitz (2006) report that emissions changes over the 2000–2050 period are very large and are expected to dominate the change in O_3 and aerosol concentrations, but other concurrent changes also affect these concentrations: meteorological variability and trends, which affect water vapor concentrations, circulation and precipitation patterns, and production of NO_x from lightning.

There have been several studies which studied the impact of a changing climate on the tropospheric chemistry. Recently, Hedegaard et al. (2008) and Koffi et al. (2010) both performed simulations for present and future climate using constant emissions. Koffi et al. (2010) mainly concentrated on tropospheric O_3, and performed simulations for year 2000 and 2050 climate conditions (SRES scenarios A1B and B1) with emissions from the year 2000 using the LMDz AGCM with prescribed SSTs and linked to the INCA CTM. Hedegaard et al. (2008) did a similar study focussing on tropospheric chemistry and aerosols. They investigated the impacts of climate change on air pollution levels in the Northern Hemisphere (with special focus on Europe and the Arctic). They used the ECHAM4-OPYC3 AOGCM and compared the decades 1990, 2040 and 2090 (SRES scenario A2). They separated out the impact from climate change by keeping the anthropogenic emissions on a constant 1990 emission level. They found that the dominant impact from climate change on a large number of chemical species are related to the predicted temperature increase. They mention that in some areas of the NH the wet deposition is increasing and in other areas the wet deposition is decreasing in their model simulation. However, none of these changes were significant. Finally, Wu et al. (2008) performed simulations for the year 2000 and 2050 (SRES

ACPD
11, C12232–C12247, 2011

scenario A1B) with the GISS GCM 3 with q-flux linked to GEOS-Chem. They also separately investigated the impact from emission changes and climate changes on the tropospheric chemistry. They conclude that over this period changes in O_3 are dominated by emission changes, while changes in OH are dominated by changes in climate.

We added at the end of Sect. 2.3:

"Furthermore, in our approach resultant climate impacts do not feed back or affect the forcing mechanisms. E.g., OH, NO_x, and O_3 distributions for the year 2050 have been calculated using 2050 emissions but using year 2000 (or 2003) meteorology, such that impacts of possibly warmer and wetter conditions on the presence of these species are not taken into account. Expected impacts of changes in precipitation on aerosol distributions are not taken into account either. Several studies (Brasseur et al., 2006; Wu et al., 2008; Hedegaard et al., 2008; Koffi et al., 2010) investigated the impact of climate change on tropospheric chemistry and aerosols. Over the period 2000–2050, Wu et al. (2008) found that OH changes from climate change prevail on changes from emissions, while O_3 changes are mainly driven by emission changes (Brasseur et al., 2006; Wu et al., 2008; Koffi et al., 2010). Hedegaard et al. (2008) found both regions with increasing and decreasing wet deposition of aerosols, but none of these changes were significant in their simulations."

2. *NO_x emissions influence aerosol formation (for, example, Shindell et al., Science, 2009) and the carbon cycle (for example, Sitch et al., 2007 and Collins et al., JGR, 2010). These indirect forcings are not accounted for in this study.*

Through the O_3 damage to vegetation, non-CO_2 emissions can impact the terrestrial carbon storage and therefore the atmospheric CO_2 concentration. Sitch et al. (2007) suggest that the impact of indirect forcing from CO_2 through the vegetation feedback might be larger than the direct O_3 impact. Furthermore, Collins et al. (2010)

ACPD
11, C12232–C12247, 2011

show that the inclusion of this feedback from O_3 on CO_2 caused by NO_x emissions possibly changes the sign of the absolute global temperature potential.

We have not explicitly taken this into account as this effect is not included in the simple climate model with carbon cycle used to calculate the atmospheric CO_2 concentration. We mention this now in the manuscript in Sect. 2.3 where we list the impacts not taken into account, and added:
"We also do not take into account the indirect impact of increased O_3 concentrations (from NO_x emissions) on the CO_2 uptake by vegetation (Sitch et al., 2007; Collins et al., 2010)."

NO_x emissions influence aerosol formation by modifying the abundance of oxidants (Shindell et al., 2009). In principal, this is taken into account to some extent as the aerosol distributions for 2000 have been calculated with the INCA-AER model which both treats aerosols and gases to ensure coherence between gas-phase chemistry and aerosol dynamics as well as possible interactions between gases and aerosol particles. For later years there might be disagreement, as we have just scaled the aerosol perturbation fields from the year 2000 with the global SO_2 emissions, whereas a change in ratio between NO_x and SO_2 emissions might possibly also affect the aerosol distrubution.

3. *For the statement: "These models are currently able to reproduce the temperature change observed in the 20th century, and confidence exists in the quality of their projections of future climate change." Specify the magnitude of confidence and please supply a reference.*

For this, we will refer to IPCC (2007, Chap. 8). They argue that climate models have been demonstrated to reproduce observed features of recent climate and past climate changes, and indicate that there is considerable confidence that AOGCMs

ACPD
11, C12232–C12247, 2011

provide credible quantitative estimates of future climate change. This confidence is stronger at continental and larger scales than regional scales, and higher for some climate variables (e.g., temperature) than for others (e.g., precipitation).

We have added this reference and mention this shortly. The text is now:
"These models are currently able to reproduce the temperature change observed in the 20th century, and confidence exists in the quality of their projections of future climate change (IPCC, 2007, Chap. 8). This confidence is stronger at continental than regional scales, and higher for variables as temperature than for precipitation."

4. *The statement "We slightly modify the data between the year 1990 and 2010 to obtain a smooth transition between total observed CO_2 concentration until 2000 and the total modeled CO_2 concentration from 2000 onwards." Please offer more quantitative replacement for 'slightly modify'.*

With the simple carbon-cycle model, one can calculate the evolution of the atmospheric CO_2 burden when the anthropogenic CO_2 emissions are known. Based on best estimates for the CO_2 emissions in the 19th and 20th century, and based on scenarios for the 21st century, the expected CO_2 concentration is calculated. For the 19th and 20th century best estimates for the atmospheric CO_2 concentration exist, based on direct observations or proxies. Comparing them with the results from the carbon-cycle model showed good correspondence over the 20th century, with a deviation of 3.5 ppmv at around 1990–2000. Whereas we used for the 21st century the results from the simple carbon-cycle model in the AOGCM, we preferred to use the observed values for the 19th and 20th century. To avoid a small discontinuity in the prescribed CO_2 concentration in CNRM-CM3.3 around the year 2000, we decided to make a gradual transition from the observed to the modeled CO_2 concentrations.

We have replaced the text by:

ACPD
11, C12232–C12247, 2011

"In our simulation, we use observed CO_2 concentrations until 2000 and the SCM modeled CO_2 concentrations from 2000 onwards. To avoid a discontinuity due to a small difference of around 3.5 ppmv between the observed and modeled CO_2 concentrations around 2000, we phase out this transition over the period 1990–2010."

5. *Why is the simple formula for CO_2 RF shown (equation 1) if the model's radiation scheme determines the radiative forcing for the imposed CO_2 concentration changes?*

The impact of CO_2 is accurately taken into account by the models radiative transfer calculation. The CO_2 radiative forcing based on the simplified formula is added only as illustration for comparison with the radiative forcing of other forcing agents. The actual on-line CO_2 forcing is not a separate output variable of the model.

We mention this now shortly in the text in Sect. 2.2.1. In the text was already quoted between brackets:
"(Note that the radiative forcing in the AOGCM simulations are computed using the model's radiation scheme, rather than this simple formula.)".
To make it clearer, we replace in the sentence "it is possible to calculate the corresponding radiative forcing" the word "calculate" by "estimate".

6. *It is not obvious to me why the 2 different ozone methodologies are applied in this particular study and not use only the 3-D fixed fields only from the CTM QUANTIFY studies that may be more consistent with the off-line aerosol fields used anyway? For example, in the dynamical run the NO_x and CO concentration distributions have been determined using different dynamics and radiation than the resultant O_3 . At the same time, I am confused by the non-CO_2 and non-CO_2* definitions. In Section 2.3 'Experiments', these are defined as non-CO_2 = dynamical ozone and non-CO_2* = fixed*

ACPD
11, C12232–C12247, 2011

ozone (or consistent with CTM aerosol forcings). Then in section 3.2 'TOA Forcing', " With this method we obtain from the non-CO_2 simulations the summed impact from contrails and aerosols, and from the non-CO_2 simulations the summed impact from O_3, contrails and aerosols. By taking the difference between these approaches one can also derive the separate O_3 impact." Is this because in non-CO_2, the dynamical O_3 is not coupled to the model radiation scheme? Then, why include this dynamical O_3? Why not just run an aerosol-only non-CO_2 simulation? Related, in terms of the aviation results in Figure 10, I do not understand why (1) there is local cooling at high latitudes for aviation non-CO_2* towards the end of the century and (2) non-CO_2 is more warming towards the end of the century than non-CO_2*. Does not seem consistent with the forcings presented in other studies (e.g. Lee et al., 2009; 2010).*

We try to address the different concerns.

How is O_3 radiatively taken into account?

In the non-CO_2* simulation, the O_3 seen by the radiative transfer code is the sum of the free-running O_3 (varying in time and space) plus a monthly mean 3-D perturbation from transport. In the non-CO_2 simulation, the O_3 seen by the radiative transfer code is just the free-running O_3, which contains an extra source or sink-term driven by the external 3-D NO_x perturbations from transport. During the model integrations, we also calculated for diagnostic purposes the radiative transfer where we left out some of the climate forcers. Comparison with the standard TOA radiative fluxes allows to calculate the radiative forcing of these climate forcers. In the non-CO_2* simulations, we left out the O_3, aerosol and contrail contribution from the transport sector, which allows us to obtain an idea of the summed RF of these climate forcers. In the non-CO_2 simulation (with dynamical O_3), we left out the aerosol and contrail contribution from the transport sectors. We were not able to take out the transport contribution on O_3 during the model integration.

ACPD
11, C12232–C12247, 2011

We have combined the estimates from these two different simulations to obtain the RF from the (fixed) O_3 approach.

Why the dynamical O_3 formulation?

In general, by modeling the source and destruction terms of O_3 together with its transport (advection, turbulence and convection), we presume that a realistic distribution of O_3 can be obtained, possibly allowing for certain feedbacks between meteorology and O_3 (in the original parametrization, source and sinks terms are calculated taking into account the local O_3 mixing ratio, the overhead O_3 column, temperature and dry deposition rates). In the dynamical approach, we also take into account the impact of NO_x in the description of source (or sink) terms for O_3. Although the distribution of NO_x is prescribed as a monthly mean distribution, we assume that the dynamical approach still increases the realism of the O_3 distribution as the lifetime of NO_x in the troposphere is shorter than the lifetime of O_3. We admit, however, that O_3 perturbations and aerosol perturbation are treated with a different methodology. But even for the non-CO_2* case we lack some consistency between the aerosol and the O_3 perturbation field as they are calculated using year 2000 and 2003 meteorology, respectively.

Why not just run an aerosol only simulation?

We agree that it would have been useful to make a separate aerosol simulation. However, in order to limit the number of simulations, we have decided to separate between CO_2 and non-CO_2 forcers, and to investigate the difference between the fixed and dynamical O_3 approach.

Why is there a local cooling at high latitudes for aviation non-CO_2 towards the end of the century? Why is there more warming for the non-CO_2 than for non-CO_2 at*

ACPD

11, C12232–C12247, 2011

the end of the century?

The differences between the non-CO_2 and non-CO_2* impact are caused by the stronger O_3 perturbation in the non-CO_2 approach. These differences are strongest for aviation (see Fig. 6). The O_3 perturbation in the dynamical O_3 approach is additionally more shifted northward than the fixed O_3 perturbation: one can see a much stronger gradient in the NO_x perturbation than in the O_3 one (see Fig. 3).

The cooling at high latitudes for non-CO_2* is probably caused by the increase in SO_4 from aviation at around 2050 (see SO_2 emission scenario in Table 4 and the aviation SO_4 distribution in Fig. 3). The impact from sulfate in our model is stronger than, e.g., in Balkanski et al. (2010). This cooling becomes visible in the non-CO_2* case as the forcing from O_3 is probably weaker than in the non-CO_2 case. This can also be seen in the RF distributions in Fig. 8.f, where the aerosol plus contrail forcing from aviation is mainly positive, except for some regions of low aviation traffic (see Fig. 2) in the NH in the period 2046–2065 at low latitudes over the Atlantic and Pacific Ocean, but also close to the north pole. This figure also illustrates that contrail and sulfate forcing do not have the same geographical pattern. This might be a consequence of the fact that the aerosol perturbation fields have been generated using meteorological data from year 2000, while the contrail (but also the NO_x, CO and O_3) perturbations have been generated using ECMWF data from the year 2003. Also different life times for sulfate and contrails might contribute to the observed differences. Finally, the fact that only for 2000 aerosol distribution estimates were available, might contribute to the observed impact. For O_3 and NO_x, 3-D distributions are available for the years 2000, 2025 and 2050. For SO_4 however, the perturbation in 2050, e.g., is based on the year 2000 perturbation (but scaled to the year 2050 global emission totals). Therefore our modeled SO_4 distribution still has a strong N-S gradient, whereas it is assumed that this gradient will reduce in the 21st century.

Finally, when looking directly at the temperature impact one must be aware that there

ACPD

11, C12232–C12247, 2011

are large uncertainties in the temperature response in the 60°N–90°N region (see 95 % confidence interval in Fig. 11.i).

In the text, a possible explanation was given in Sect. 3.3 on P19795, l23–25: "This local negative impact is possibly a consequence of the over-estimated sulfate (see the aviation sulfate distribution in Fig. 3.c) and underestimated O_3 impacts."
Earlier in Sect. 3.3. there was the sentence on P19793, l8–9:
"For the non-CO_2 impact from aviation we see a strong difference between the non-CO_2 and non-CO_2* approaches."
We add now: "..., caused by the rather different O_3 perturbations (see Fig. 6)."

7. *The scenario for the future projections assumes large growth in aviation and large decrease in road vehicles 2100–2000. Is it realistic that aviation will increase in global source strength by a factor of 7–10 across this time period without efforts by humanity to mitigate emissions? What about NO_x stringency rule?*

The SRES scenario A1B is a scenario with strong technological renewal, where changes are assumed to be more economically driven than by climate change or environmental concern. The SRES scenario B1 is an environmentally more optimistic scenario than scenario A1B and takes into account, e.g., rapid introduction of clean and resource-efficient technologies. Specifically for aviation, a mitigation option scenario B1 ACARE (Advisory Council for Aeronautical Research in Europe, Owen et al. (2010)) has been developed and studied where the assumed increase in NO_x emissions is much lower. The B1 ACARE scenario for aviation still shows an increase in the NO_x emissions over the period 2000–2025, but already a strong reduction over the period 2025–2050 (see Hodnebrog et al., 2011). Lee et al. (2009) created two different aviation emission scenarios for the same underlying SRES scenario A1B GDP, assuming two different levels of NO_x technology. Our emission estimates correspond with the case where it is assumed that advances in airframe and engine

ACPD
11, C12232–C12247, 2011

technology will be typical and market-driven. In their second case more emphasis will be placed on reducing NO_x levels to the slight detriment of CO_2 emission levels.

We mention therefore in the manuscript in Sect. 5 (Conclusions) that our emission scenario is based on the SRES scenario A1B storyline for GDP development but that A1B is only one out of more GDP scenarios, and also alternative assumptions for the implementation of fuel efficiency and emission factors exist (Lee et al., 2009; Eyring et al., 2005).

Minor comments:

1. *Figure 8 - too much yellow, can you change colour bar to show more information in the figure?*

We have changed the colour bar.

2. *Figure 9 (RHS) the thin black lines are difficult to read, is it possible to change to light grey?*

At the moment, we have not yet changed the colour. We recognize that the density of the black lines is so high in this figure, that the results from the individual simulations are not distinguishable. We fear, however, that this might not improve by changing the curves in light grey.

3. *Include some evaluation/validation of the model contrail-cirrus model representation.*

For the evaluation/validation of the contrail parametrization, we would like to refer mainly to Myhre et al. (2009). In Myhre et al. (2009) one can find a comparison between the impact of contrails on the radiative transfer in CNRM-CM3.3 and the

ACPD
11, C12232–C12247, 2011

impact in other AGCMs or in detailed radiative transfer codes. Myhre et al. (2009) investigated the impact of the solar zenith angle on the TOA radiative forcing and found that the sign of the net RF due to contrails for various solar zenith angles is similar among most of the models. They also conclude that the pattern of geographical distribution of the RF for a homogeneous contrail cover is consistent between five models (including CNRM-CM3.3), but the magnitude is significantly different. They finally mention that there is a consistent pattern between the models with rather low RF values at high latitudes.

The parametrization in CNRM-CM3.3 used in the present study is different from the one used in Myhre et al. (2009) in two aspects. While in Myhre et al. (2009) the greybody emissivity formulation was replaced by a two-stream approximation in order to accommodate the prescribed optical properties of the contrail, this was not activated for the simulations presented here. This has some impact on the RF, as for a global 1% contrail cover the impact was reduced from 0.190 $W m^{-2}$ in Myhre et al. (2009, Fig. 5) to 0.160 $W m^{-2}$ in our study (see Sect. 2.2.5). This has possibly also impacted the geographical distribution. Secondly, whereas in Myhre et al. (2009) contrails were confined between between 10 and 11 km, in the simulation here we allowed contrails at different altitudes.

We have no separate experiments to estimate the temperature impact from contrails only. This response is not straightforward, e.g., regions for highest contrail cover do not necessarily correspond with highest RF regions: Ponater et al. (2006) found a southward shift of the maximal impact. Furthermore, the climate sensitivity to a contrail perturbation can be considerably smaller than the CO_2 climate sensitivity (Ponater et al., 2005).

We now mention explicitly in Sect. 2.2.5 that CNRM-CM3.3 participated in the comparative study of Myhre et al. (2009). We mention the comparison of the global averaged RF and the spatial distribution of this forcing. Additionally, we corrected the

ACPD
11, C12232–C12247, 2011

value of 0.15 into 0.19 $W\,m^{-2}$ from Myhre et al. (2009, Fig. 5).

The actual text is now:
"This value of 0.16 $W\,m^{-2}$ corresponds well with the values mentioned in Myhre et al. (2009) which is a comparative study among different line by line radiative transfer codes and codes used in AGCMs, including the one used in CNRM-CM3.3. For an experiment with a 0.01 global contrail cover at 0.3 optical depth, where the grey body emissivity formulation had been replaced by a two-stream approximation in order to accommodate the prescribed optical properties of the contrail, a value of 0.19 $W\,m^{-2}$ was reported for CNRM-CM3.3, close to the best estimate of 0.163 $W\,m^{-2}$. Myhre et al. (2009) further mention that for this experiment they found a strong similarity in the spatial pattern of the radiative forcing among the models, with rather low values at high latitudes."

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ACPD
11, C12232–C12247, 2011

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ACPD

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ACPD
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