

Response to anonymous referee #1

We thank reviewer #1 for the positive reply. Please find below our answers to the specific comments.

General comment:

This is a clearly written paper showing new results on the impact of traffic on tropospheric chemistry and radiative forcing for future emission scenarios. The results are presented alongside previous results for other transportation sectors to give a summary of the impact of aircraft, shipping and road traffic emissions. The results are presented clearly, but concisely in figures and tables. Relevant literature is cited and compared to the new results. Except for a couple very minor points, I recommend publication.

Comment:

Fig. 2 & 4. I don't understand what the white contours show. I thought the color contours are the difference between a 5% perturbation in the emissions of each sector and the BASE run. Please explain in the text and the figure caption.

Answer:

The colour contours show the *absolute* difference (in ppb for O₃ and 10³ molecule cm⁻³ for OH) between the BASE run and a 5% emission perturbation for each sector, while the white contour lines show the *relative* difference (in percent) between these runs. The figure captions have been updated to clarify this.

Comment:

p. 20984, l.10: whereof should be whereas

Answer:

This has been corrected.

Response to anonymous referee #2

We thank reviewer #2 for valuable comments which helped improve the manuscript. All the reviewer's points have been carefully considered, and our manuscript has been modified accordingly. Based on the reviewer comments #7 and #8 we decided to carry out detailed radiative transfer simulations for all cases where we had previously (in the ACPD version) used a scaling approach based on normalized radiative forcings. This was a useful exercise, and as the new results were only slightly different (as explained in more detail below in the answer to comment #7), it showed that the NRF scaling method was relatively robust. This is an interesting finding which has now been added to the manuscript. Please find below our answers to the general and specific comments.

General comment:

This is a well written paper further addressing the question of how much different traffic sectors (road, ship and aircraft) contribute to the radiative forcing. All models involved use the same meteorological fields, which presumably make the results dependent mostly on the emissions. There are however some issues that need to be addressed before the manuscript is ready for publication, as discussed below. I will preface my comments by saying that a lot of the details of the manuscript need to be found in other publications. This makes sense, except that I did not have time to go through all the other papers to answer my questions. The authors may consider adding some clarifications in areas where there are questions to clarify, as discussed below.

Finally, this is a question for the editors. This manuscript is essentially the same as previous one. The main difference is that (as the authors state), it considers a different emission scenario (A1), and puts the results in the context of other results. Methodology, etc., seems to be the same as in previous papers. So my question is whether this is sufficient for a publication? Assessment exercises run a host of different scenarios, and I am not sure that every time there is a new run there is a need for a new publication.

Answer:

We argue here that our manuscript is indeed sufficient for publication. Considering the underlying comprehensive modelling work (involving six different CTMs and one radiative transfer model) and thorough analysis, the amount of material was too large to fit into one paper. Instead, we found it more sensible to present our results in two companion papers, each using essentially the same methodology but with very different focuses: Hodnebrog et al. (2011) covering optimistic low emission scenarios (B1 and B1 ACARE) for the aircraft and shipping sectors, and the present manuscript dealing with a more pessimistic high emission scenario (A1B) for all three transport sectors. This also allowed us to reach out with our results more quickly as the B1/B1 ACARE paper could be published at an earlier stage.

In addition, the previous papers on this topic were slightly inconsistent in the sense that different model ensembles were used (i.e., the MOCAGE model was not included in Hoor et al. (2009) and Myhre et al. (2011)) and not all transport sectors were covered (i.e., the road traffic sector was not covered in Hodnebrog et al. (2011)). For these reasons, the results (e.g., the O₃ RF caused by the different sectors) from the various studies were difficult to compare, hence there was a need for a synthesis of the various results. In the present manuscript, all these “gaps” were filled as we present radiative forcing numbers from all six models and all three transport sectors for each of the years and scenarios. When comparing RFs between different scenarios, changes are therefore driven mainly by differences in emissions. We also feel that the new RF simulations which have been performed for this revised version of the manuscript further strengthen the value of the paper.

We hope the editor supports our view on this matter.

Comment:

*1. One of the main problems I have that needs clarification is how they treat the ship emissions. There has been some work done on this, since diluting the ship emissions to the whole grid size results in producing large amounts of ozone, which disagree with some observations. This also impacts the OH. As a matter of fact, I am curious about what the methyl chloroform lifetime is when the treatment of ship emissions is included (maybe it is in one of the other papers?). In any case, I think that the authors should discuss this uncertainly. A recent paper on this issue is, for example: Vintken et al., *Atm. Chem. Phys.*, 11, 11707-11722, 2011. This is an important issue, since it is the ship emissions that change the sign of the radiative forcing in some of the scenarios considered.*

Answer:

The effects of aircraft and ship plume chemistry have been discussed very briefly in the introduction of the companion paper (Hodnebrog et al., 2011), but we agree that the current paper would benefit from a more thorough discussion of plume chemistry. Over the last decade or so there have been several studies investigating the effect of plume chemistry, but at the time of planning the model simulations the effects of plume chemistry were uncertain and there did not exist a general plume parameterization that was easy to implement in the CTMs. Hence, the models used here assume instant dilution of emissions to the whole grid size, and this may lead to overestimation of ozone formation. We have added the following paragraph to the manuscript (near the end of Section 2):

“It should be noted that the CTMs used here have a rather coarse grid resolution, and previous studies have shown that ozone formation may be overestimated when the emissions from e.g., aircraft and shipping are instantly diluted in a large grid box (e.g., Meijer et al., 1997; Kraabøl et al., 2002; Franke et al., 2008; Paoli et al., 2011; Vinken et al., 2011). One study suggests that ozone production due to aircraft emissions may be reduced by 10-25% in the Northern Hemisphere when subgrid-scale plume effects are taken into account (Cariolle et al., 2009), and another study suggests a similar reduction for ship emissions – around 10-30% over parts of the North Atlantic Ocean (Huszar et al., 2010). Although there are relatively large uncertainties related to the effects of including plume chemistry, the possible overestimation of O₃ production due to the neglect of plume processes should be kept in mind when interpreting the results presented in the subsequent sections.”

Comment:

2. Page 20981, lines 14-23. This paragraph makes an important point. However, there is another element that is not described, mainly what is the numerical advection algorithm used by the different models. This could affect the latitudinal and longitudinal distribution of the perturbations (see below).

Answer:

To avoid too much repetition of the companion paper (Hodnebrog et al., 2011) we have greatly reduced the section with the description of models in the present manuscript. As stated in the paper, more details about the individual models, such as which numerical advection schemes that have been used, can be found in the companion paper (Table 2).

Comment:

3. Page 20982, line 23, and Figure 2, etc. There is an inconsistency here: the text says that in the perturbed scenarios the emissions are reduced by 5%, but the figures show increases in NO_x, ozone, etc. Which one is it? Also, given the above ambiguity, the authors should clarify what kind of perturbation they get when the results are scaled to 100%: is it taking out the aircraft completely, or doubling the emissions?

Answer:

Figures 2-4 show the *unscaled* impact (based on a 5% perturbation in emissions and not scaled to 100%) on O₃ and OH from the emissions of each of the traffic sectors. The figure captions have been updated to clarify that “the scales have been reversed in order to show O₃ reductions, arising from a 5% decrease in emissions, as positive numbers”. Hence, the scaling to 100% is similar to, but not equal to (as described in the ACPD version page 20982, line 23 -> page 20983, line 14) taking out the emissions from one traffic sector completely.

Comment:

4. Figure 2. What are the authors showing here, i.e., what is the difference between the color contours and the white lines (if the white lines is the change relative to the BASE run, what are the color contours?). Also, “the right column shows zonal mean perturbations for all transport modes”. Do you mean for the corresponding sectors on the left-hand column?

Answer:

The colour contours show the *absolute* difference (in ppb for O₃ and 10³ molecule cm⁻³ for OH) between the BASE run and a 5% emission perturbation for each sector, while the white contour lines show the *relative* difference (in percent) between these runs. The figure captions (Figures 2 & 4) have been updated to clarify this. Regarding the right column, it shows the zonal mean perturbations for the corresponding sectors on the left-hand column. The caption of Figure 2 has been changed accordingly.

Comment:

5. Again in Figure 2: The ozone perturbations show a maximum over the pole, whereas the strong corridors are at lower latitudes. Other model calculations (see, for example, the results in the IPCC Aircraft assessment) show more of a maximum at around 60N, similar to what the authors see for OH. Any idea why this is? Could this be a numerical transport issue?

Answer:

The aircraft-induced ozone maximum near the pole in July is seen in all six models in the ensemble (please see Figure B1 in Hodnebrog et al. (2011)) and is also in agreement with previous studies (e.g., Figure 3a in Brasseur et al. (1996), and even for annual means in Figure 4 in Gauss et al. (2006)). This feature is related to the poleward transport patterns and the stronger incoming solar radiation which makes ozone formation more intense during summer than winter at these high northern latitudes. As far as we can tell, the IPCC special report on aviation (IPCC, 1999) shows zonal mean ozone fields only as *annual averages* (their Figure 4-1) when the ozone maximum is closer to 60N – in agreement with our results.

Comment:

6. Page 20985, around line 15. The O₃ production increases with NO_x up to a center value, and then it turns around and decreases. The implication here is that the NO_x is higher than the “turn around” point for the UT/LS conditions? Which is this value, approximately? It may be useful to give the background values of NO_x, since they may be different for the different models, and they would impact the non-linearity. Along these lines, what are the different models using for lightning NO production?

Answer:

We mean that the number of ozone molecules enhanced per aviation NO_x-molecule emitted becomes lower when NO_x increases, and not necessarily that the NO_x values reach such high levels that the ozone enhancement efficiency becomes negative. It is therefore not meaningful to calculate a “turn around” point, but we expect such a value to be far higher than the modelled UT/LS NO_x values in the 2050 A1B scenario (which is the scenario with the highest aircraft NO_x emissions), otherwise the 5% perturbation of aircraft emissions should reveal negative aviation-induced O₃. As model intercomparison is beyond the scope of this study, the background values of NO_x are not analysed. The lightning NO_x emissions we have used are described in the companion paper (Hodnebrog et al., 2011) and is for most models specified at 5 TgN yr⁻¹ following Schumann and Huntrieser (2007).

Comment:

7. The robustness of the scaling approach for the RF also implies that the scaling factors were also derived from a complete radiative transfer calculation that had the same cloud field. Is this the case? (I am assuming that these are not clear-sky RF). One could also question whether the scaling would hold for perturbed scenarios such as A1 in 2050. Also, is the RF instantaneous, or is the stratosphere relaxed to equilibrium?

Answer:

All the radiative forcing calculations used meteorology from the year 2003 (same year as for the CTM simulations), meaning that the same cloud field has been used when deriving the normalized radiative forcing factors. The revised manuscript has been updated to clarify that the RF is for cloudy-sky including stratospheric temperature adjustment.

In light of the comments regarding uncertainty and the question of validity of the scaling approach for RF, we decided to carry out complete radiative forcing calculations using the same RF code and setup as in the companion paper (Hodnebrog et al., 2011). The new results show that the RF scaling method was relatively robust since the new calculations were, for the most part, only slightly different from the results obtained using NRFs. More specifically, the ensemble mean O₃ RF for the different years were changed by only 2% or less, except for ROAD when the difference from the old results (using NRFs) reached 12% (2050 B1). This is not surprising as the ozone precursor emissions from this transport sector are almost eliminated in this scenario and percentage differences in RF may therefore be large.

Tables 4 & A2, Figure 5 and the manuscript text have been updated with the new results. All in all, the ensemble mean RF numbers were only slightly changed and all conclusions from the original manuscript are not affected.

Comment:

8. Page 20990, lines 19-21. The change in RF per DU of ozone would presumably depend on the altitude profile of the ozone change. Is the factor quoted here for a decrease in the UT/LS, or uniform? How uncertain is this scaling factor?

Answer:

Please see answer to the comment above (complete RF calculations have now been carried out).

Comment:

9. Page 20992, lines 12-15: The statement is made that the results of Lee et al. for the RF are higher "because they removed all aircraft emissions, whereas we used a 5% perturbation approach". I presume that the RFs shown are relative to a "clean" background, with no aircraft. Now, a statement is made above that scaling the 5% results to 100% is a good approximation, but here the implication seems to be that one would get different results if one did a run with 100% reduction. So which is it? In general, this points to the need to be very clear as to what they are calculating, and consider the dependence of scaling, parameterization, etc., on the background atmosphere.

Answer:

We agree that the above statement is confusing and it has therefore been removed in the revised version of the manuscript.

Comment:

10. Table 4: What is the meaning of “the history of emissions being taken into account”? This suggests a time-dependent simulation.

Answer:

It means that we have corrected for the transient response, i.e. that the CH₄ concentration may not be in steady state with the OH change during the simulation year. This is described briefly in the text (ACPD version page 20990, lines 21-26) and in a bit more detail in the companion paper. The caption of Table 4 has now been slightly modified for clarification.

Comment:

11. Tables A1 and A2 are very useful. One of the problems with assessment studies in general is trying to diagnose why model results are different. I would suggest that the impacts on lifetime be further broken down into the categories in Table A2. What is the impact of methane changes on its own lifetime? In addition, my understanding is that the change in ozone due to the change in methane is calculated from the scaling factors given in the text, and the results are given in Table A2. However, if we scale down the O₃ because of the changes in methane, this would also change the OH and thus the lifetime. Is this included anywhere?

Answer:

The methane lifetime changes in Table A1 are calculated from the change in OH caused by emissions from each of the transport sectors. This information is then used to calculate the RF terms for methane and methane-induced ozone in Table A2. A further decomposition of the impacts on lifetime should not be necessary, and we choose to keep the original Table A1. This also allows for easy comparison with the results of previous studies (Hoor et al., 2009; Hodnebrog et al., 2011; Myhre et al., 2011). A factor of 1.4 from IPCC (2001) has been used to take into account the impact of methane changes on its own lifetime (please see ACPD version page 20990, lines 13-14).

The scaling factor we have used to calculate the change in ozone due to the change in methane is taken from Berntsen et al. (2005) (see Eq. B6 in their study) who have assumed an ozone response of 0.64 DU per 10% enhancement in methane. They calculated this value based on results from six global 3-D CTMs and it includes the assumption that the change in methane is in equilibrium with the OH change. The revised manuscript has been slightly modified to make clear that the scaling factor is for a system which has reached a new steady state.

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