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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The impact of traffic emissions on atmospheric ozone and OH: results from QUANTIFY" *by* P. Hoor et al.

P. Hoor et al.

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We thank referee 1 for the thorough reading and her/his comments and suggestions to improve the paper.

The reviewer suggests more detailed comments with regard to the small perturbation approach and as referee. Like referee 2 he/she also questions the use of a global ozone lifetime to estimate the ozone sensitivity per NO_x-perturbation and asks for a clearer description of the methane initialization.
We tried to follow all the referee's suggestions as described in detail in the list below.

The abstract summarizes the results of the study, but this is done more effectively and

concisely in the conclusions. I recommend that the abstract is revised to shorten it and to emphasize how the responses from the different modes of transport differ before quantifying the responses from particular sources.
We shortened the abstract and focus on the general findings.

The paper needs to acknowledge that the results of the study are dependent on the analysis method chosen (scaling small perturbations) and that the contributions of different sources therefore reflect the sensitivity around current emissions rather than an attribution of the full effects of each source. The approach taken here is valid, but comparison with previous studies applying a different approach can be misleading.
Since this point was mentioned in all comments we included a new Figure, which clearly illustrates the differences of both approaches. We also added a paragraph to emphasize the differences between both approaches and to discussed the differences. We think this clarifies the consequences of the small scale approach and a later scaling to the reader.

The paper is very well written in general, but the English needs polishing in a number of places (for example, "of" -> "in" in the second line of the abstract).
We checked the spelling and grammar and corrected the errors.

p.18224, l.20: The fuel consumption decrement here is not meaningful on its own. Either give the figure from Endresen as well, or (better) express the decrement in fractional or percentage terms.
We agree and added the fractional decrease.

p.18225: What seasonal variations are applied to these emissions, and are these consistent between models? It is important to make this clear here so that later

comparison of January and July conditions can be interpreted appropriately.
Thus seasonal cycles are represented in the applied biogenic emissions as well as in the biomass burning emissions and the aircraft emissions. Road and ship traffic emissions and the other EDGAR-based fields are derived from annual mean numbers and therefore do not include a seasonal cycle. Since all models used the same emissions the seasonal cycles are consistently included in all models.

p.18225: How is methane treated in this study? Is it constrained, and if so, how?
As stated in the reply to Frank Dentener and mentioned in the original manuscrpt methane was prescribed at the surface. We extended the description and added: '...methane was prescribed as a surface boundary condition using time dependent surface mixing ratios as in Jöckel et al. (2006) based on surface observations from the AGAGE database...'

p.18226, l.9: You should note that the scaled small perturbation approach used here provides a measure of sensitivity to current emissions, not a full attribution of the effects of that source. Note also this method is likely to bias the relative effects of the different modes of transport, and it is not immediately clear which way these biases operate.
We added a clarification on this as suggested to avoid misinterpretations. Indeed the upscaled sensitivities are not the same as a 100% perturbation. We also added a Figure showing the magnitude of the effect using one of the models as an example and added a comment on a possible bias. The different effects are worth a study on its own, but here we want to make clear that we are aware of the differences of both methods, when we provide scaled numbers.

p.18231, l.18: The middle panels of Fig 2 appear to show normalized standard deviation expressed as a percentage (add units to figure!) This will inevitably emphasize

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areas where the mean column perturbation is small and where small differences in transport will lead to large relative differences ion standard deviation. It would be more appropriate to show absolute standard deviations here to highlight where uncertainties are greatest and to allow comparison with the interannual variability in the lower panels. If the present approach is to be retained, it needs to be defended more vigorously!
We included the absolute standard deviations to the respective Figure. We agree that both quantities (i.e. the absolute as well the relative standard deviation) provide important information to the reader. The relative values show directly, where the models exhibit largest differences of the simlated ozone perturbation, whereas the absolute deviation allows to compare to the interannual variability.

p.18231, l.28: How is interannual variability defined here? Is it the interannual standard deviation (which would make a valuable comparison with the middle panels?)
We clearified this in the text as it is indeed the interannual standard deviation.

p.18233, l.11: This might also suggest that the models differ more greatly in their vertical distribution of perturbations than they do in their horizontal distributions, perhaps reflecting how much convection schemes differ, and also in the responses to aircraft emissions, where the variability appears large (see Table 6). These appear more likely explanations than the one given.
Here we wanted to make the point that the interannual variability is remarkably small in this case. This might indicate that the effect of convection on the zonal mean is relatively robust despite interannual variations of its location. It is however, also clear that the different convection schemes introduce a large variability to the transported species. We changed the revised manuscript accordingly.

p.18234, l.11: Niemeier et al. zeroed road traffic emissions instead of applying a small perturbation, and therefore a larger response would be expected because of

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the nonlinearities in ozone production. A factor of 2-3 difference is therefore not inconsistent here (particularly in light of the marginally higher NOx emissions they used.)
We thank the reviewer for this comment and included it in the revised version.

Section 4.4: This section is interesting and worth retaining, but needs to carry a caveat that it reflects current sensitivities and not full source attribution: non-linearities in production may be expected to influence these relative contributions quite strongly.
We agree that the relative contributions as discussed in section 4.4 and shown in Fig. 7 (Fig. 8 in the revised manuscript) are different from those accounting for the full respective traffic emission source. However, here we wanted to compare source strengths sensitivities and their contribution to the total traffic perturbation, which is only possible with the small scale approach. Otherwise, due to the non-linearities the perturbations of individual means of transportation would add up to more than 100% which would make it difficult to compare the relative contributions of the individual transport seactors. In the revised version we included a Figure illustrating the differences based on a sensitivity study by p-TOMCAT in section 2 and added a discussion on the limitation. Here in section 4.4. we added a comment on that.

p.18237, l.1: The estimation of production efficiency here is very crude as the ozone lifetime differs substantially with altitude and season. Did none of the models used here diagnose ozone production? This would allow the robustness of this estimation approach to be confirmed more reliably.
We admit that the use of a single mean lifetime for ozone is rather crude, but we want to illustrate that from a global perspective the ozone amount per unit emission of NO_x differ substantially. We do not want to look into regional differences of the the ozone production or loss. Furthermore, it is difficult to account for transport related effects when separating different regions since the NO_x-perturbations and the respective

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ozone responses are transported very differently due to the different lifetimes of NO_x and ozone. Therefore we changed Tab. 6 and display annual average ozone burden change per NO_x-emission as a global annual average without accounting for an ozone lifetime in the revised version.

p.18238, l.4: "Since road emissions are largely emitted at some elevation..." This explanation seems odd, as population centers are generally weighted towards lower elevations; is it not more likely to reflect weaker OH impacts in the most polluted conditions at low elevations where the bulk of emissions occur? (This point is later alluded to on p.18239, l.1)
We thank the reviewer and added a comment with regard to the pollution here since in fact it may play the most important role as stated later. The effect of land elevation might add to that to a minor degree since indeed most of the road emissions are released between 950-1000 hPa.

p.18239, l.17: See also Granier et al., GRL, 2006 for further discussion of the effect of ship emissions in the Arctic.
We added the reference and the respective key results.

p.18241, l.5: Are the results shown in Fig 10 from a single model? If so, which one?
The results in Figure 10 are from the ensemble mean. Although the individual models clearly exhibit differences, the respective sensitivities as discussed in more detail for ozone are also relatively robust for the OH-perturbations.

Tables and Figures:
The simulation table (Table 3) is inconsistent. The reductions should be labeled "95%", or alternatively the "100%" entries should be zeroed.

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We changed the table.

Table 4: It would be useful to provide summary details about emissions differences in this table, so that differences between model runs are clearer. Use of additional emissions data, seasonality, reduced lightning NOx (LMDz-INCA), etc., should be indicated here.

A statement on the seasonalities has been included in the emission description and we added the lightning NO_x to the table.

Table 6: The figure for aircraft in the TOMCAT model stands out as vastly different from the other models; some explanation for this is required.

Some later sensitivity studies using p-TOMCAT in T42 horizontal resolution (unchanged vertical resolution) for aircraft emissions indicate that the effect of aircraft emissions as given in Tab. 6, will be reduced strongly. The results show that the ozone burden change is reduced by 34% (similarly for NO_x), see also comment to Ulrich Schumann.

The figures would be clearer if there were fewer color bars - particularly in cases like Fig 1 when all panels have the same scale. Constructing the final figures as single objects (rather than as assemblies of component panels) would allow better use of the space available, and would also help the reader interpret them more easily. The aspect ratio of the panels varies between figures, and it would make comparison of different figures easier if this was standardized. It is also confusing to have the individual panel titles underneath the panels (as in Figs 2, 3) rather than in the conventional location above the panel.

We clarified the Figures and removed double legends and axes titles. We also changed the position of titles and removed unneccessary information.

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Fig 7: The left and right columns should be labeled by month, or this information should be included in the caption.
We included the month in the caption.

Fig 9: The altitude levels in the caption are not consistent with those in the figure labels.
We changed the caption accordingly.

p.18235, l.8: add ’is’ before ’larger’
p.18238, l.20: add ’emissions’ after ’road’
p.18265, l.1: Januray -> January
We accounted for the specific technical comments, but also re-checked the manuscript.

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