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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 Frank Dentener for the thorough reading and his suggestions to improve the paper. One of his major points is the use of a small perturbation approach, which is later scaled to 100%. We agree that it is important to emphasize the difference between such a perturbation approach and a total removal of the emission source. To clearly illustrate this, we included a sensitivity study, which show that there are significant differences between both approaches. We added a new section which discusses these differences and we make clear that we are aware of this. In addition to the detailed discussion of the different approaches we also added comments on this at the respective sections of the text.

Detailed reply:

p.18221 l.9 the authors need to keep 2 things separated: the sensitivity calculated by the 5% perturbation; and the overall effect of traffic emissions, which in principle can NOT be estimated from 20x the 5% perturbation. If the authors still want to make about overall effects then they should at least show with a couple of case studies comparing effects and mention the associated error from this approach.
We agree, that the scaling of a 5% perturbation is not the same as a simulation removing the total respective emission source. The small scale perturbation approach has been chosen to allow a quantitative comparison of the impact of the respective means of transportation as it has been done in Figure 7 of the manuscript (Figure 8 of the revised version). We discuss the difference to a 100%-simulation in the revised version. Although both approaches are different we believe that a scaling to 100% allows the reader to better estimate the effects albeit this is not the same as a full perturbation simulation and the reader must be aware of this.

p.18221 l.10 the global average boundary layer (defined as?)? I suggest to also give the marine BL;and the continental BL separately.
We will replace the term 'boundary layer' by the pressure range (below 800 hPa), since a pressure criterion was used to obtain the numbers. We did not separate for land and sea, since we want to directly compare the global effects of both emission sources. A separation would not account for the long-range transport related effects since e.g. land based emissions and the related perturbations are transported far downwind the continents. Therefore we did not split the analysis here. The respective Figures may help the reader to estimate regional effects.

p.18221 l.11 if you use 7 models I would like to see the uncertainty on these numbers.
As indicated in Table 4 six models participated of which 5 are used to obtain the ensemble means. The number in the abstract was wrong and included a second GCM (ECHAM5/MESSy) which had to be excluded, since the dynamics and chemistry

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were fully coupled. We included the respective uncertainties in the abstract as they are given in Table 5. Note, that the wrong units were used in the abstract of the manuscript, which has been corrected in the revised version.

p.18221 l.19-21 this sentence is not clear to me.
We rephrased the respective sentence: '...Below 800 hPa both ozone and OH respond most sensitively to ship emissions in the marine lower troposphere over the Atlantic. The effect on ozone can exceed 10% close to the marine surface (global zonal mean) which is 80% of the total traffic induced ozone perturbation...'

p.18221 l.28 I guess you want to say that ozone can decrease due to traffic emissions, the sentence reads akward.
In reply to reviewer 2 the statement on the seasonal cycle of the road induced ozone perturbation was removed from the abstract.

p.18222 l.2 Methane lifetime towards OH, or rather the turnover time? Define.
Rephrased: '...Methane lifetime changes against OH are affected strongest by ship emissions up to 4.1 ($\pm$ 1.)%, followed by road (1.6 ($\pm$0.25)%) and air traffic (1.0 ($\pm$ 0.4)%).

p.18222 l.9 Vice versa? Climate influencing transport? Explain.
The feedback between temperature increase and air pollution level is meant here, rephrased: '... , affecting air quality and climate, which in turn affects air pollution levels.'

p.18222 l.18 the global annual averaged RF.
...included

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p.18223 l.25 I think an important sales argument for this study should be that results are analysed with the same metrics and methods throughout this study, whereas for the most of previous studies these aspects are difficult to assess. Give also the argument why you want to use several models (cancellation of errors; quantification of uncertainties). Do you use common emission databases?
Of course an ensemble of only five models is not a large statistical ensemble. However the use of different models with different properties gives at least some indication on the robustness and variability of the results, particularly when using the same emission data set. We added a coment on this.
Common emissions were used as stated in section 2 of the paper.

p.18224 l.14/15 higher than EDGAR3.2? In table 1 I get the impression that numbers are generally lower than other studies.
The final QUANTIFY road emission inventory is higher by the factors which are mentioned in the text. The road emissions as they were included in the simulations for this study are as stated in Tabel 1 and are indeed somewhat lower than in EDGAR. Note that the final road inventory which is available contains larger numbers for CO, NO_x and NMHCs, which we also mentioned in the text.

p.18224 l.14-25 Please give a couple of sentences explaining the main differences with previous studies; like this the information is too shallow to be able to judge whether the changes make sense.
New emission data were calculated for road and ship, respectively, which both have been published as referenced in the manuscript. The procedure for road traffic is described in an own paragraph and for all emission data sets global numbers are explicitly calculated and compared to other data bases. Therefore we added for the ship emissions a comment explaining the major differences for NO_x and NMHCs. A detailed description of each individual data set can be obtained from the given

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references, particularly Endresen et al. (2007) for ships and Borken et al. (2007) for road emissions.

p.18225 l.11 161/210 what is this number based on? What did von Kuhlman assume, and is this different from usual assumptions?
The conversion factor of 161/210 TgC/Tg(NMHC) for industrial emissions is based on the third assessment report 2001 (IPCC Thirs Assessment report - Climate Change 2001: WG 1: The scientific basis, Chapter 4.2.3.2., Table 4.7.b). The breakdown of NMHCs into distinct species is based on EDGAR v2. The separation has been tested and applied in the MATCH model by von Kuhlmann et al. (2003).

p.18225 l.16 off line fields? Monthly, daily, hourly? How much, was it specific for the year under consideration, or rather climatological?
The fields were calculated with a time resolution of 5 hrs (output timestep) based on a calculation where the model was nudged to ECMWF operational data covering the period 1998 - 2005 (Jöckel et al., 2006). These fields were provided as monthly mean fields for the specific years 2002 and 2003 to the other participants. We changed the text accordingly.

p.18225 Explain how methane was treated- and how were long-term methane effects accounted for in this study.
Methane was initialized using the methane distribution for Jan, 1 ,2002 taken from the 1998 - 2005 simulation by ECHAM5/MESSy (Jöckel et al., 2006). For the QUANTIFY simulation methane was prescribed at the surface using time dependent surface mixing ratios based on observations as in Jöckel et al. (2006). Methane lifetimes were calculated offline using the respective monthly mean CH_4 and OH-fields from each model. As stated in the caption of Tab. 7, no feedback factor is included, which accounts for longterm changes as described in Fuglestvedt et al. (1999). We added

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this to the text.

p.18226 l.16 As explained before, you can not do this, unless you show the error on it. To say it in a different way to calculate the effect of a sectoral impact a 100% perturbation is most appropriate- if properly taking into account issues with changing methane. Perturbations are useful to estimate incremental emission changes starting from the current atmospheric conditions. There is a limited range of validity (typically $\pm$ 50 %). If you work outside of this range you should show that it can be done.
As stated in the beginning, we emphasize, that both approaches are not equivalent by highlighting the differences in the new Fig. 2 and discussing the implications in a separate subsection. We want to test the sensitivity of the chemical response against a small perturbation to assess how the different sectors add to the total traffic induced perturbations. The latter would not be possible when removing the respective sources. It is of course right that a latter scaling of a small perturbation response differs in the same manner from a full decline of a source. We think that the new Fig. 2 and the related discussion emphasize the differences of both approaches to the reader.

p.18226 l.21 It seems that you the model E39/C (ECHAM?) is an outlier in that it runs climatology, and that it does not contain HCs chemistry. You may have reasons for the former, but I think non including HCs is currently not acceptable anymore. I suggest to remove the model; or give the results a minor weight.
We clarified the section, since E39C was not considered for the tables and ensemble means on display anyway.

p.18226 Figure 2: Hard to read. I would expect a relative standard deviation in the middle panel to have a value around 1.
We checked the results, but the perturbation fields turn out to be rather similar for all models, although the ozone fields themselves differ partly by a factor of two between

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the models (a paper about the comparison of the simulations to measurements is in preparation). The simulations as performed in this study have to be regarded as starting point not covering all aspects related to traffic emission changes (e.g. representation of plume chemistry and dilution effects on the subgrid scale). However, together with the relatively small standard deviation this indicates that the ozone perturbations as simulated by the models with the current setup using the same emissions are relatively robust.

p.18232 l.9 Here I understand that E39/C is essential to estimate the effect of variabililty; it is a pity that none of the models that could have been easily used for this purpose has been employed for this.
We agree with that, and note that a surprisingly low number of studies do such a comparison. That also adds to the list potential of additional studies as mentioned in the previous comment.

p.18233 l. 11-15 I would doubt that a climate model can catch all variability, and would rather rely on analysed meteorology. It would also be useful at this point to refer to real measurement and say what they show in terms of variability (of course they can not separate out traffic induced ozone).
Unfortunately there are no multiannual simulations available using analysed meteorological fields and the same emission inventories as in QUANTIFY. Since the ozone perturbations cannot be directly compared to measurements, we did not include comparisons to ozone or other tracer observations. This will be done in a separate comparison paper comparing in-situ observations (sondes and airborne) to the QUANTIFY simulations.

p.18233 l.24 effect is inverted. Try to rephrase this.
Rephrased: ...During winter the average effect of road traffic on ozone in the highly

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industrialized regions of the extratropics almost vanishes or even changes sign, i.e. an increase of road emissions leads to a decrease of ozone during winter.

p.18234 l.25 explain UTLS.
inserted: UTLS (upper troposphere/lower stratosphere)

p.18235 l.11 this is perhaps a counterintuitive and interesting results that may be highlighted more. At the same somewhere (discussion?) you may want to indicate the growing share of aircraft /ships.
As also stated in reply to the comment of Ulrich Schumann we included a separate calculation for the northern extratropics which highlights the effect of surface traffic emissions in addition to those from air traffic and discuss possible future implications.

p.18237 l.1-11: I am not convinced at all that using a global O3 lifetime for such spatially different emissions sectors is possible. E.g. lifetime of O3 is very different in the upper troposphere compared to the humid marine boundary layer. I would commend the authors to double check with budget numbers from the models that this is a valid approach. E.g. the TM4 model carries these budget numbers standardly, and probably other models as well. In general of course it will be true that the O3 production efficiency of aircraft is higher than from the two other sectors.
Unfortunately we only have the net ozone production rates available, which do not help to address that point. Indeed one global number for ozone lifetime is crude. Note also, that splitting up into different regions is difficult, since it is not immedeately clear how to relate e.g. surface emissions to perturbations in the upper troposphere. However, to allow for an estimate of ozone perturbation per NO_x-emission not using a gloabl lifetime, we changed the table using the average ozone burden per NOx-molecule emitted.

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p.18240 l.6: Here it should be explained what you mean with methane lifetime. It is strange that there is nowhere information on how methane was constrained in this study.
We agree and added more detailed information to the revised version, see also comment above.

p.18240 l.10 the difference with Eyring is very high, and I wonder whether it is not partly determined by small ensemble sizes of different sets of models. I suggest the authors to find out whether those models that participated in Eyring's study, also gave such a large difference, or even better perform a sensitivity study with one of the current models and Eyring's ship emissions. If you can show that this is robust, it of course deserves more attention.
As stated in the manuscript the emission distribution is rather different in both approaches, since the distribution of ships in Eyring et al. (2007) is very localized (compare our Fig. 1 with Fig. 1 in Eyring et al.) along the major shipping routes. All the models assume that the emissions are distributed uniformly in the respective grid-boxes. Therefore a larger spread of the emissions affects a larger region (of course to a different degree depending on source strengths). Although not addressed systematically, this seems to be an important point. Furthermore our results are derived from a small perturbation. Even if the NO_x response of the ship emissions were linear, the OH-response does not necessarily behave in the same way. We added an indication to this to the text.
The results of Fuglestvedt et al., PNAS, (2008) (their supplement) are very similar to our results, indicating a methane lifetime change of 5.2% using 4.4 TgN of NO_x for shipping. The associated radiative forcings of -43$\pm$13 mWm^{-2} (τ-CH_4) and 32$\pm$9 mWm^{-2} for ozone burden bracket our individual model results and are much closer to our mean than to Eyring et al. (2007).

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