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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.

Anonymous Referee #1

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This is an important and valuable study tackling an issue of major scientific interest: the impacts of different modes of transport on atmospheric ozone and OH. The paper quantifies the effects of emissions from road transport, shipping and aviation by applying several different models together in a coherent way, and it compares results with those from earlier studies of road and shipping emissions with individual models. Of particular interest in this study is the differing ozone responses of the transport emission sources through their location, differing chemical environments, and interaction with convection and lifting processes. The study is well-conceived, and the manuscript is well-written and organized. The paper is suitable for publication after minor revisions, and I highlight a number of issues which need to be addressed in the final version.

General Comments:

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Interactive Discussion
Discussion Paper

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.

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.

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).

Specific Comments:

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.

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.

p.18225: How is methane treated in this study? Is it constrained, and if so, how?

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.

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 areas where the mean column perturbation is small and where small differences in

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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!

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?)

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.

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 the non-linearities 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.)

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.

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.

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

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p.18239, l.1)

p.18239, l.17: See also Granier et al., GRL, 2006 for further discussion of the effect of ship emissions in the Arctic.

p.18241, l.5: Are the results shown in Fig 10 from a single model? If so, which one?

Tables and Figures:

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

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.

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.

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.

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

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

Typos:

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p.18235, l.8: add "is" before "larger"

p.18238, l.20: add "emissions" after "road"

p.18265, l.1: Januray -> January

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 18219, 2008.

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