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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.

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This in an interesting and relevant study, however with some methodological flaws. While I think that most problems can be solved by careful wording and re-interpretation, I also recommend that some additional sensitivity analysis and/or more in depth analysis of current model output can help to improve the paper. My detailed comments are found below.

Abstract 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.

p.18221 l.10 the global average boundary layer (defined as?)? I suggest to also give the marine BL;and the continental BL separately.

p.18221 l.11 if you use 7 models I would like to see the uncertainty on these numbers.

p.18221 l.19-21 this sentence is not clear to me.

p.18221 l.28 I guess you want to say that ozone can decrease due to traffic emissions- the sentence reads akward.

p.18222 l.2 Methane lifetime towards OH, or rather the turnover time? Define. Introduction:

p.18222 l.9 Vice versa? Climate influencing transport? Explain.

p.18222 l.18 the global annual averaged RF

p.18223 l.25 I think an important sales argument for this study should be that results are analysed wit the same metrics and methods throughout this study, whereas for the host 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?

p.18224 l.14/15 higher than EDGAR3.2? In table 1 I get the impression that numbers are generally lower than other studies.

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.

p.18225 l.11 161/210 what is this number based on? What did von Kuhlman assume, and is this different from usual assumptions?

p.18225 l.16 off line fields? Monthly, daily, hourly? How much, was it specific for the year under consideration, or rather climatological?

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Interactive Comment

p.18225 Explain how methane was treated- and how were long-term methane effects accounted for in this study.

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 +/- 50 %). If you work outside of this range you should show that it can be done.

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.

p.18226 Figure 2: Hard to read. I would expect a relative standard deviation in the middle panel to have a value around 1.

p.18232 l.9 Here I understand that E39/C is essentail 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.

p.18233 l. 11-15 I would doubt that a climate model can catch all variability, and would rather rely on analysed meteorology. I 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).

p.18233 l.24 effect is inverted. Try to rephrase this.

p.18234 l.25 explain UTLS

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.

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Interactive Comment

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.

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.

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 the 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.

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Interactive Comment