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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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The paper presents a multi-model study of the impact of NOx emissions of traffic by road, aircraft and ships on O3 and OH for one unified set of emission data. The study is of high importance for practical reasons. Its results have the potential to influence future assessments of traffic impacts on climate, with potentially practical consequences. Hence the results need to be discussed carefully.

I have several comments and suggestions for improvements:

The essential results are summarized in the abstract.

As a minor point: the values listed in the abstract appear to be inconsistent with those listed in Table 5; could it be that the abstract values refer actually to the ppbv changes

and not the % changes?

Anyway, the percentage changes are small, apparently smaller than in previous studies, at least compared the results from previous studies cited in the introduction of this paper.

One of my main points: I miss a discussion of the reasons why these values are so small.

Previous studies, as partially discussed in the introduction of this paper, reported aviation impact on ozone changes of the order 3 to 6 %. Beck et al. (1992, Atmosph. Env.) reported 12 % O3 changes at the tropopause. The AERONOX project (Schumann, 1997) found maximum changes of zonal mean values of O3 of 4 % in January and 9 % in July.

The present study reports 0.72 % (Table 5) changes of O3. This small ozone change needs to be discussed. Obviously, most of the discrepancy is because of different averaging methods. This paper lists global mean in the upper troposphere (200-300 hPa) while previous studies refereed to maximum values or upper tropospheric mean values in the zonal mean at Northern latitudes. However there are other differences like different emission rates, perhaps also different chemistry with different kinetical data, different lightning sources etc. What are the essential reasons for the small impact of NOx emissions on O3 and OH, as reported in this paper?

This needs to be clarified. In fact, I ask that this paper also lists the maximum changes in zonal means and the means in a certain latitude band (30-60 deg N) and altitude band (200-300 hPa) in the Northern hemisphere. Moreover, this paper should compare the new results with those of previous studies and should discuss possible reasons for the differences.

Moreover, I see the need that this paper clearly identifies the open issues, such as - coarse resolution (the use of T21, for example, does not appear to be state of the art



**ACPD**

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anymore) - mixing from plume scale to grid scale - diurnal cycle of NOx emissions (the AERO2K project provided such data) - heterogeneous chemistry, including take up of NOy onto ice particles - impact of the recently observed reaction NO+HO2 -> HNO3 on NOx , O3, and OH (is this reaction included in the models used for this study or not? how important is this reaction for aviation impact on O3 and OH?). - impact of different convection and lightning parameterizations - any experimental evidence for NOx impact on O3 and OH - impact of CH4 boundary conditions (fixed or variable?)

I also miss a discussion of the nonlinearities implied with the 5% or a 100 % change in emissions from the three traffic modes. How much different would the results be if using 100% disturbances instead of 5% disturbances? For which traffic mode is this kind of nonlinearity most important. Why are the authors putting so much emphasis on this nonlinearity but ignore the strong nonlinearities resulting from lightning, the diurnal cycle of emissions, and the plume to grid scale mixing?

Finally, the paper should give some indication of what the results imply in terms of radiative forcing. I have the impression that the results of this study implies smaller radiative forcing of NOx via O3 and CH4 than reported in Sausen et al. (2005) and IPCC (2007).

An editorial remark: pptv and ppbv are non-SI units. Please use SI units.

References:

D. Cariolle, M. J. Evans, M. P. Chipperfield, N. Butkovskaya, A. Kukui, and G. Le Bras, Impact of the new HNO3-forming channel of the HO2+NO reaction on tropospheric HNO3, NOx, HOx and ozone, Atmos. Chem. Phys., 8, 4061-4068, 2008.

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