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**Geoscientific
Model Development
Discussions**

Interactive comment on "The global chemistry transport model TM5: description and evaluation of the tropospheric chemistry version 3.0" *by* V. Huijnen et al.

V. Huijnen et al.

huijnen@knmi.nl

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General comments

We thank the reviewer for his/her positive review of our manuscript. We regret the omission of a proper acknowledgment to the data providers from the INTEX-B campaign. In the manuscript the description of the method for comparing model data to flight data was kept very brief, as the evaluation was considered as 'auxiliary', while the main model evaluation of NO2 was performed via a comparison to OMI observations. However the reviewer is correct in that some essential information is currently missing and is necessary for a better understanding of the comparison. After consulta-

tion of the PI we decided to include two additional references in the revised manuscript, one describing the measurement method in detail and a second describing the exact configuration used during INTEX-B:

J. A. Thornton, P. J. Wooldridge, R. C. Cohen, Atmospheric NO2: In Situ Laser-Induced Fluorescence Detection at Parts per Trillion Mixing Ratios, Analytical Chemistry, 72, 528, 2000.

A.E. Perring, T.H. Bertram, D.K. Farmer, P.J. Wooldridge, D.R. Blake, N.J. Blake, B. Heikes, M.A. Avery, G. Sachse, G. Diskin, H. Fuelberg, W.H. Brune, J. Crawford, H.B. Singh and R.C. Cohen, The production and persistence of ïĄŞRONO2 in the Mexico City plume, Atmos. Chem. Phys. 10, 7215-7229, 2010.

We extended the manuscript to include the following details:

"We use the data that is measured by laser-induced fluorescence from the DC8 aircraft during its ascents and descents (Thornton et al., 2000, Perring et al., 2010). Model profiles were produced by interpolating model output in space and time to individual data sampled from all respective flights. "

Furthermore, the reviewer is correct that the statement on the simulation of the boundary layer height is inaccurate, as we also responded to the first reviewer. The statement is removed. We are aware that the situation is difficult for a global model to match to observations, as discussed in Hains et al. (JGR 2010). For instance, as the TM5 model resolution is 3 degree longitude and 2 degree latitude, it cannot be expected that very local pollution events from Mexico City are captured. In fact, a selection of data samples very close over the mountain plateau, which showed clear signs of local pollution events, have been excluded from the analysis. We included a comment on this in the revised manuscript:

"A selection of data samples which showed clear signs of strong local pollution events have been excluded from this analysis."

As was described in Hains et al. (JGR 2010), a strong variability was found in NO2 observations over the Mexico region, which make these in-situ data a valuable contribution in the current model evaluation. Over the eastern Pacific the measured NO2 concentrations (as well as their variability) is much lower, and would not add much information to the discussion. Furthermore, as written at the start of section 5, in this evaluation paper we choose to focus on the large-scale variability of ozone and its key precursors. The evaluation of more local details with respect to additional ozone precursors and/or aerosols is outside the scope of this work.

Other minor comments:

Section 5: Give further justification for selecting these species for comparison - is it just that satellite observations are available for them?

We selected those species for evaluation which are key ozone precursor tracers and contribute towards global CO, and for which there exist well established observations. We also selected sets of measurements that reflect large-scale variability. In this respect the availability of well documented satellite retrievals is very suitable for the given evaluation, and for future reference. However, note that for ozone and CO the validation is extended with the WOUDZ and GMD network, as well as the MOZAIC flight data, while the evaluation of NO2 is extended with profile information from INTEX-B. OH is further assessed with methyl chloroform surface measurements.

p.1029, l4: over what altitude is the average mixing ratio of 68.9 ppbv determined?

The number 68.9 ppbv that we present in the paper is calculated by dividing the global burden of CO by the global mass of the atmosphere. However, in the following sentence we compare this number to the value reported for TM5 in Shindell et al. (2006), which was 66.5 ppb. But Shindell et al. derive this number from 'the broad MOPITT 500hPa retrieval level', which we overlooked in the original manuscript. Therefore the two numbers cannot directly be compared. We calculate a new global mean annual average CO concentration based on the definition as given in Shindell, which results in

a significantly larger value of 85.4 ppbv, i.e. an increase of 22% compared to the previous TM5 version. This is in fact in better agreement with the rest of the CO analysis, where also larger model concentrations are discussed. We updated the manuscript in this respect.

p.1031, line 13: I think the authors mean "positive bias with respect to MOPITT" rather than *in* MOPITT.

The reviewer is correct. We modified this.

p.1034, the first paragraph is repeated in the 2nd paragraph.

We removed the second version of this paragraph.

I'm not sure why Appendix A B are appendices. Seems they could just be included in the main text of the paper.

The reason why we have included two appendices is because here two different model applications are described which are not identical to the one in the main text.

In Appedix A a separate simulation of methyl chloroform was performed, to evaluate the OH field. The description of this setup is too detailed to be included in the main text, where only the conclusions (the MCF lifetime and its correspondence to observations) are of relevance for the discussion of the OH field.

In appendix B the two-way zooming capability of TM5 for atmospheric chemistry modeling is discussed, presenting the new feature of a maximal resolution of up to 0.5x0.25 degree spatial resolution. Again, two separate simulations have been performed. While in the rest of the manuscript a clear focus is given to the large-scale variability, in this appendix we assess its suitability for regional air quality applications.

Therefore we decided to keep the structure of the manuscript as it currently is.

Interactive comment on Geosci. Model Dev. Discuss., 3, 1009, 2010.