

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

Anonymous Referee #2

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This paper presents a comprehensive description of the TM5 model, including evaluation of this version. The comparisons to versions used in previous studies and model intercomparisons is helpful. The paper is well written. The tables, figures and supplementary material give a good illustration of the capabilities of the model. I recommend publication after addressing the following comments.

My only real concern about the paper is the comparison to the aircraft NO₂ measurements, described in Section 5.4 and shown in Figure 11. First, the specifics of the aircraft measurements need to be included (which aircraft, which instrument, who is the PI, dates of the flights). Has the PI been offered co-authorship? If they declined

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they should be acknowledged, and a reference given for their measurement. How were the model profiles produced? Secondly, it appears the authors do not understand the complexity of the measurements over Mexico and the Gulf of Mexico. Mexico City is on a mountain plateau at 700 hPa. The elevated NO₂ measurements at those altitudes are from Mexico City pollution and its outflow. Measurements near 1000 hPa were made in Houston (if DC8 data) or Veracruz (if C130). I do not see how the conclusion of the model simulating well the boundary layer height can be made. Finally, why were only the Mexico region NO₂ measurements of INTEX-B selected for comparison? Both the Mexico and eastern Pacific phases of INTEX-B/IMPEX/MIRAGE-Mex include a comprehensive suite of tropospheric gas and aerosol measurements from the DC-8 and C-130 in March-May 2006. Additional comparisons to ozone precursors would be a valuable addition to this paper.

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

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

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

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

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

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