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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Atmospheric oxidation in the Mexico City Metropolitan Area (MCMA) during April 2003" *by* T. R. Shirley et al.

Anonymous Referee #1

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General comments

Overall I found this paper to be very interesting and a worthwhile addition to the growing amount of literature regarding measurements and modelling of HOX radicals. The paper describes the recent Mexico City Metropolitan Area study that took place in April 2003, and included measurements of many important atmospheric constituents including OH and HO2. Relatively high concentrations of both OH and HO2 were observed, and a steady-state photochemical model did a reasonable job at reproducing the results, when averaged over the campaign. The paper is written well and contains an appropriate amount of detail, figures etc. (with the exception of the comments below).

Specific comments

My major criticism is that the authors did not discuss the day-to-day model-measurement comparisons, which would provide more insight into the chemistry occurring at the site. Average profiles such as the ones shown in figure 9 can obscure the real level of model-measurement agreement as a model over-prediction on one day can be cancelled out by a model under-prediction on another, giving a false impression of the level of agreement. I suggest that the daily model profiles are added to figures 7 and 8 to allow the reader to assess the model performance more fully. At the very least, a table should be added detailing the modelled and measured HOX values around solar noon on each day.

There are several points throughout the paper when the authors make conclusions based on hydrocarbon measurements that don't exist without an appropriate caveat. For instance, in section 3.3, the authors state that 'OH production from O3+alkenes Ěwas less than 5% of the total'. This statement is based on estimated hydrocarbon measurements if I have understood the process properly, so a caveat needs to be added. Similarly, the greater than expected production referred to later in the same paragraph could presumably be under-estimated loss due to 'missing' VOCs. The impact of missing VOCs should also be returned to in the summary and conclusions section when the authors talk about possible reasons for model-measured differences.

Page 6046 - the authors need to state explicitly how kwall was derived. Was it measured directly or estimated?

At the end of section 3.3, have the authors included the RO2+NO=RO2NO channel in their model?

Technical corrections

Page 6048 - still appears to be some confusion between ppt and ppb in the last paragraph

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Interactive comment on Atmos. Chem. Phys. Discuss., 5, 6041, 2005.

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Interactive Comment