

Interactive comment on “Multi-model ensemble simulations of tropospheric NO₂ compared with GOME retrievals for the year 2000” by T. P. C. van Noije et al.

Anonymous Referee #2

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This paper describes a comparison of modelled NO₂ columns from 17 different models with three retrievals from the GOME satellite data for the year 2000. Overall, the models compare well with the retrievals. Those models which used meteorology for the year 2000 produced the best comparisons, indicating that, not surprisingly, the use of different meteorology will bias the modelled results. Similarly, the use of biomass burning emissions specific to the year 2000 also improved the results. The authors show that significant differences between the models exist. The three satellite retrievals also showed significant differences from each other, making comparison with the models harder.

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This paper should be published in ACP, but I have some comments that I would like addressed first.

Comments:

Page 2983, lines 6-13. The finest model resolution is 1 x 1 degree, many are coarser than this, yet the modelled data were mapped to 0.5 x 0.5 degrees initially; why not 1 x 1, or why not go directly to 5 x 5 in the first place ?

The GOME forward scan size is quoted in km, yet the model resolutions are given in degrees. It would be worth converting (say) the GOME pixel size to degrees as well, so it may be compared directly with the model resolutions.

Was the same treatment of the modelled data from ensemble A applied to ensemble B? The authors state that 2 models (MATCH_MPIC and IMAGES) only supplied monthly means.

Page 2969, Line 24-25: Rephrase to read "... combination of ground-based and air-borne measurements, as well as those derived from satellites."

Page 2980, lines 12-13: Rephrase to read "None of these models were set up in a fully coupled mode; the ..."

Page 2986, lines 25-27. Even the smallest model resolution (1 degree) corresponds to a distance of 111 km at the equator, and many of the models have a coarser resolution, so it is not surprising that they don't reproduce small-scale features.

Page 2987, lines 8-11. The authors should explain that despite the fixed global methane levels, a methane lifetime can still be calculated by dividing the global burden by the loss rate due to reaction with OH. In fact, such an approach gives a good comparison of individual models' OH fields which in turn will influence the NO2 lifetime.

lines 13-15, the word 'relatively' appears many times. It might be better to quote the magnitude of the differences.

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Table 4, page 3019. I suggest quoting the means and standard deviations as, for example, 4.63 +/- 0.84, so the same number of decimal places are used for both quantities.

Page 3035, caption for Fig.8. It would be worth explaining briefly what the fields in Fig. 2 are as well, as currently the caption is confusing.

Pages 3037-3040, Figure 10. The p-TOMCAT and TM5 lines are difficult to distinguish. Could the authors use a wider range of colours?

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 2965, 2006.

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