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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Using GOME NO_2 satellite data to examine regional differences in TOMCAT model performance" *by* N. H. Savage et al.

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This paper by Savage et al. discusses a comparison between GOME NO_2 measurements and the TOMCAT global chemistry-transport model. Several similar comparisons have been published before in the literature. Despite this I am in favour of publication. I regard the GOME NO_2 measurements as very valuable to test tropospheric chemistry models, and results of inter-comparisons with different models deserve to be published independently. The comparisons contain improvements of the technical approach over previous studies (although not in all aspects). The paper provides a good overview of TOMCAT model aspects and includes a balanced set of references.

I have several general and specific remarks and questions and I would encourage

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the authors to address these points and to produce a revised version of the paper accordingly.

General remarks:

The discussion chapters provides a nice overview of TOMCAT model aspects of relevance to the model/GOME comparisons. At the same time the discussion is qualitative. Quantifying uncertainties related to aspects of the transport and chemistry would be very valuable. I realise that this is non-trivial and often beyond the scope of this paper. However, rough error estimates are possible for some aspects, e.g. the photolysis rate difference between clear sky and a climatological cloud cover can be quantified.

I find the discussions in the article a bit long at some places. For instance it will help to eliminate the forward referencing of sections and to discuss aspects at one place only.

The paper presents inter-comparisons of monthly means. A better approach is the comparison of individual pixels with collocated model values (which can then be presented in a monthly-averaged way). This is especially important for time-varying aspects such as the outflow over the oceans which is strongly dependent on the changing wind direction and convection. Because the number of GOME observations in the monthly mean is often small and far from uniformly distributed in time (depending on the clouds) this sampling issue may introduce considerable differences between model and GOME monthly averages. The authors note that such a space-time sampling of the model at the observation locations would be valuable. Therefore it is not clear to me why the authors have not chosen this approach. One step further is the inclusion of the averaging kernels in the comparison to also account for the vertical sensitivity of the measurements.

The article tends to attribute the model-GOME differences to model shortcomings. It is important to stress that also the GOME retrievals are characterised by uncertainties that are not small. Readers may get the impression that the paper presents the intercomparison as a "validation" of a CTM (with many shortcomings) with an "observational

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truth" from GOME. The difficulties in the GOME retrieval are mentioned but despite this model-GOME differences are mostly attributed to TOMCAT, see e.g. the abstract. If the authors claim this is justified they should be able to show that typical GOME retrieval errors (plus inter-comparison sampling errors) are smaller than the observed model-GOME differences. If not, they should provide more balanced conclusions.

Specific comments:

- Abstract: "... modelled columns are too large ... " Should this be replaced by " ... modelled columns are larger that the GOME retrievals ..."? In other words: is it justified to attribute the difference to TOMCAT, and is the error bar of the GOME retrieval (systematic error) significantly smaller than the difference observed?

2572, l9: " ... this might reduce OH concentrations thus reducing the lifetime of NO_x." Please explain the mechanism here. What about the direct reaction with OH to form HNO_3?

2574, bottom: It is mentioned that daily NO_2 fields of SLIMCAT are used. I assume that these are daily-average fields instead of local-time fields. One important point here is that day-time NO_2 has quite a different profile than night-time NO_2 in the stratosphere, the latter peaking at higher altitudes. Since dynamics-related features are strongly height-dependent this may introduce error patterns in the troposphere as well. Please provide a remark on this.

2575, l15: Tomcat profiles are used to calculate the air-mass factor: " .. the vertical profile of NO_2 used is taken from the daily results of the TOMCAT model run .. ". Please provide more details how this is done. Does this mean the AMF is calculated for each individual GOME pixel separately based on a collocated model profile? Or does it mean something else?

2575, l27: " .. data were selected to be cloud free .." Which cloud algorithm is used here? I assume there is no cloud correction applied in the retrieval for cloud fractions

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between 0 and 10%. Please state that this is the case.

2576: Please provide more information how TOMCAT describes the daily cycle. E.g. discuss the development of the boundary layer in the morning, the use of meteo (on 6h basis?), and the daily variation of emissions (if present)?

2579, l25 and 2580, l3: It is mentioned that Velders and Lauer find model values that are much higher compared to GOME. Is this due to differences between the models, or to differences between the GOME retrievals? Please provide some information on the differences in the GOME data sets. How large are these? A brief summary of retrieval details would be interesting as well. How do the models inter-compare (on average).

2580: It is not very useful to have forward referencing to sections 4.3 and 4.4 - please remove and discuss aspects at one place only.

Table 2: The authors may consider to provide a map to replace the table. This will be helpful for the reader.

Table 3 caption: mention if gradient is the ratio of observation divided by model, or model divided by observation.

Fig 4: text in figure mentions June 1999 instead of June 1997 !?

The large seasonality of the comparison over Europe is quite mysterious to me!? Emissions should be well characterised here I would think. Convection is mentioned as one explanation, however this is not fully convincing to me. I would challenge the authors to address this point in more detail. Can there also be retrieval aspects playing a role? Please provide a more extensive discussion of the comparisons between models concerning the seasonal cycle over Europe, perhaps including results of regional models.

Fig 6 and fig 7: It looks like TOMCAT has negative model columns ?! Is this true?

Sec 4.3: The long-range transport comparison may be misleading due to availability of a limited number of cloud-free GOME pixels. As mentioned above, a better approach is

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a comparison of equal place/time collocated model with GOME. For studies of export over the Atlantic ocean this may be crucial, especially when outflow and cloud occurrence is somehow correlated. Please add a remark on the additional uncertainty that is introduced by the (monthly-mean) inter-comparison approach.

2587: Discussion of retrieval errors. Although I do not like to promote our own work, I think a reference to Boersma et al. (JGR 109, doi:10.1029/2003JD003962, 2004) would be appropriate here.

2587, bottom: Good point. There are relations between clouds, frontal systems and the absence of GOME observations. This shows again that comparing individual measurements with model values collocated in space and time is more convincing.

5.3.1, horizontal transport. I do not understand how fig 10 proves that horizontal transport will not be the main source of error. Please explain or weaken the conclusion.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 2569, 2004.