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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.

Anonymous Referee #1

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General:

The paper describes an inter-comparison of GOME NO2 tropospheric columns with TOMCAT model results for a specific year (1997). The authors introduced a revised version of the "tropospheric excess method" (TEM), which I think is a good approach. The authors tried to minimise differences in the comparison by sampling model data similar to the satellite data. There is a long discussion of plausible explanations for differences between model data and observations. However, because of the complexity of the system the authors were not able to identify model shortcomings for individual cases, which would have been of real value for the paper. The paper may become acceptable after major revisions.

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Major remarks

- The authors introduced an updated version of the TEM by taking into account the horizontal pattern of stratospheric ozone, based on results from the SLIMCAT model. I think it would be worth investigating the effect of such an approach and to quantify the differences. This could also become an important contribution to this paper (if proven to enhance the quality of the TEM) and should be mentioned then in the abstract.

- There is a principle problem in the discussion of the results. The authors mention that there are uncertainties in the absolute values of the NO2 GOME columns. This has to be taken into account already during the discussion of the results (see 3 "+" below):

+ Differences between Lat x Lon plots from GOME and Model data should be compared to GOME uncertainties, to see where differences are significant (Fig. 1-3).

+ The correlations should take into account a range of GOME data, instead of a single value, which then would lead to ranges of gradients, which then perhaps are not anymore significantly different from 1 ? (Fig. 4,6,7)

+ Using an appropriate test should prove, which of the differences in the seasonal cycle are really significant. (Fig. 5 and 9). Taking only the variability in the area into account already leads to the impression that the differences are hardly significant. (e.g. mean GOME data are only ¡ std. dev. lower than model data, An additional GOME uncertainty would probably lead to no significant differences in Fig. 5. A point-by-point inter-comparison would then help to minimise the variability.

- The GOME data rely on TOMCAT NO2 profiles, which has been discussed in the discussion. This is a crucial point and has to be clarified and quantified in more detail. E.g. How much changes the GOME column when different TOMACT vertical profiles would be taken into account. This has to be included in the uncertainty discussion and should be included in the part satellite data.

- The comparison concentrates on the year 1997 only although more satellite and

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model data are available. The authors are aware of the speciality of this year, namely the El Nino event, which has a large impact on the meteorology and biomass burning. Two points have to be mentioned: (1) How representative are those results for other "normal" years? (2) What impact has the difference in the biomass burning patterns between the emissions data used in the model and in reality on the findings of the comparison? I suggest to extend the comparison.

- Some parts of the interpretation of the differences are separated without further reason. E.g. the discussion of the radon simulations would help to understand the findings in section 4.

Specific Remarks:

Abstract: Last sentence is very unspecific. And in principle I think this statement is correct. However, I do not see in this case what kind of insights are gained by the intercomparison, nor that a specific model deficiency has been identified and corrected.

Introduction: 2572/8-10 Why does a OH reduction reduce the NOx lifetime?

Satellite data:

2574/25ff As shown by the various references the NO2 data are very valuable for modelling groups. There has always been a discussion on the quality of the TEM data. It seems that the method described here improves the methodology. This should be quantified and discussed in this section. If the quality of this satellite product has been significantly increased this would be a nice outcome of the paper and also mentioned in the abstract.

2574-2575: Nothing is said about the uncertainties of the satellite derived tropospheric columns. How good does it pick-up surface NO2, or is there a saturation effect? Is it possible to discuss some sort of error bars?

2575/15 What is the reason for choosing the year 1997 only? As far as I know GOME data, SLIMCAT data and TOMCAT data exist for more than only this year. Taking

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several years into account would also give the opportunity to test the robustness of the results. Moreover 1997 was an El Nino year, which may limit the validity of the results, which also has been indicated by the authors in the following section. I suggest to expand the analysis to more years.

TOMCAT model

Radon 2577/3 only short-range transport! (convection)

Processing Table 1: Figure caption should be clearer. Correlation of what? How is the gradient defined? d(model)/d(satellite)? ....

Results: General remark: I would like to see some uncertainty/error estimate for the satellite data and difference plots for GOME-Model. These two information would give a better insight on the quality of the model, e.g. in cases where the GOME uncertainty is larger than the difference one cannot conclude that the model is doing a bad job. This also affects the correlation plots!!

2579/12 the statement is too general. Please specify what you mean with generally good agreement? It would be much easier to evaluate the quality if a difference plot would be included. 2579/21 I am not sure how well the GOME data may resolve this feature when plotting them on the TOMCAT grid! Again a difference plot would help! 2580/7ff The satellite data do not allow for directly measure emissions. Please re-phrase the passages. One can see elevated NO2, which is likely to result from biomass burning. What is the role of El Nino for biomass burning and how well are the regional displacement of biomass burning areas caused by El Nino simulated by the model? (or included in the emission data)? Here additional years would help to eliminate the El Nino effect.

Correlations 2581/15ff Since nothing is said about the GOME data quality the results are difficult to interpret. Winter in Europe may be dominated by low level clouds, which may reduce the number of measurements dramatically. If only data from a few days

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are available, then the results are hardly significant. In summer high NO2 surface concentrations may not be seen by GOME due to saturation effects, which would bias the results to higher gradients. 2581/23ff I would interpret Fig. 5 exactly the other way round! GOME data are in most cases within ¡ std.dev. of the TOMCAT data, which rises the question whether the results are really statistically significantly different? And the bars are assuming the correctness of the GOME data! Taking some uncertainty of the measurements in addition into account a conclusion from this figure could be difficult.

2584/5 This information should be given in the satellite section, but in more detail, since it strongly influences the quality of the conclusion drawn from the inter-comparison!

2584/7 This passage is confusing. The par starts with Figure 7 (January only), but then discusses all months, can one see that from the figure? 2584/13-15 NO2 columns are a product of different processes: emissions, chemistry, transport. How can one deduce from the fact that the modelled columns are similar to the GOME data that the emissions are correct. Why couldnŠt it be that the emissions are way off but the chemistry and transport balance this? The discussion makes it quite clear that additional information on the export or chemistry is needed to better interpret the results. Perhaps MOPITT CO figures could help? 2585/5 approximately 75% of the No2 could be explained as far as I understood from this additional experiment. However, the huge reduction in the emissions non-linearly affects the O3-Nox chemistry. Ozone production is probably totally reduced. So that also OH production is inhibited, which affects the NO2 lifetime.

2585/10 include the Figures you are addressing 2585/13 I do not see the Asian plume. Please specify. 2585/13ff Another possibility would be a too fast vertical transport of NO2, so that No2 is transported mainly at low levels to the W and in the model at higher levels to the E. In this case the African column wouldnŠt be different but NOx is transported away at different altitudes and therefore different directions. Here CO comparisons would lead to more insights. If CO also shows a plume in the GOME

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data. The NO2 plume is likely to arise from burnings.

Figure 9 + 2586/10ff: Taking into account the std. dev. I think one hardly can derive a statistical significant difference in the seasonal cycle! Again more data would help! Since a lot of emphasis is given to biomass burning, it would be helpful to discuss in more detail the used emission database in comparison to, e.g. ASTR fire counts, although of course from fire counts one cannot derive emissions directly, but it would give a better basis for the discussion of regional pattern and seasonal cycle.

Discussion

2587 Most of the information is already needed in the introduction of the satellite data and for the interpretation of the inter-comparison, to avoid mis-interpretations as discussed above.

2588/6-10 This point should be clarified better. Assuming correct emissions and problems in the transport, why should than be the concentration correct? Or in other words, why a gradient of 1 a indication for correct emissions given that you have discussed problems with the transport?

2588/11 I am not quite sure about this, because air conditioning consume a huge amount of energy, especially in N: America.

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