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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Tropospheric NO_2 columns: a comparison between model and retrieved data from GOME measurements" *by* A. Lauer et al.

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General Comments

The paper presents a comparison of tropospheric concentrations of NO_2 retrieved from GOME data to those calculated by a fully coupled chemistry-climate model. The paper presents a new approach to such an analysis by using a longer time span of data than previous studies. The paper is clearly structured and is, in general, well written although it is let down by a thoroughly uninspiring conclusion. The results of the paper are of potential interest to the community although the discussion of the large differences observed between the model and the measurements are somewhat superficial. However, the applied methodology is a significant step forward in reducing what are

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currently large uncertainties in the global oxidised nitrogen budget.

I feel the discussion of the results and conclusions to the paper is rather weak and highlight the negative aspects of the work, as the results (given the size and complexity of the study) are quite remarkable. This aside, along with some points for consideration by the authors listed below, the paper is recommended.

Specific Comments

The explanation of the importance of NO_2 in atmospheric chemistry given in the first paragraph of the introduction is a little simplistic. It would be better to refer the reader to a standard textbook such as Finlayson-Pitts and Pitts (The Chemistry of the Upper and Lower Atmosphere).

The first paragraph of page 413 refers to the direct absorption of radiation by stratospheric ozone and implies a direct link between tropospheric NO_2 and stratospheric ozone. This is a rather tenuous link and the authors would be better to consider the link between tropospheric NO_2 and ΔO_3.

The second paragraph of page 413 refers to the differing contributions to the global NO_X budget. Again, it would help the reader if a link were given to a better description such as Brasseur *et al.* (Atmospheric Chemistry Global Change) as the explanation has limitations.

Most of the discussion of the GOME instrument is superfluous whereas important factors such as the spatial and temporal sampling of the instrument are discussed somewhat too briefly. Also, I think it unacceptable that the reader is directed elsewhere to find out the uncertainties of the retrieval method applied here. I would recommend including a table to summarise the assumptions made in the GOME tropospheric NO_2 retrievals.

Bullet point b) on page 416 should include a reference to justify this assumption.

On page 416, the statement describing the model as a "spectral interactively coupled"

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GCM is somewhat confusing. A brief sentence expanding on this description would be useful.

The authors point out that the CHEM part of the coupled model does not include either PAN or N_2O_5 hydrolysis. The effect of these omissions on the model output is not adequately discussed where the subject is introduced in the paper or in the discussion of the observed differences. For example, Schultz *et al.* (JGR, 2000) suggest that PAN could be responsible for up to 27% of the NO_X production in the tropical upper troposphere and that N_2O_5 hydrolysis contributes around 20% the loss of NO_X. I feel a more substantial discussion of the errors that these omissions would give on the model output should be included as they disagree with the suggestions and choice references in the paper.

In general, the discussion of the differences between the model results and the instrument data is not very convincing and appears to repeat the usual arguments found when completing such a comparison, although directed this time towards NO_2 and its chemistry. A more useful analysis would be to show what effect is observed by changing the NO_X emission or running the model over smaller areas at a more appropriate time for overlap with the ERS-2 satellite. A study of this type should be able to answer questions such as: a) How large are the errors on the NO_X emission inventories? What effect does a small perturbation have on the model output? b) Are the errors produced by a lack of understanding of the basic chemistry? I feel the inclusion of PAN and N_2O_5 hydrolysis is necessary to answer these questions. These questions are apparent in the paper but there are no answers.

Technical Corrections

Page 412 line 8: Should read "earthshine radiance and extraterrestrial irradiance".

Page 412 line 10: The sentence beginning "The period of ..." is not grammatcally correct and should be rewritten.

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Page 412 line 23: Change "NO_2can" to "NO_2 can".

Page 413 line 20: Reference to Leue *et al.* should be 2001 not 1999.

Page 413 line 20 and 21: Change to "In this study, in contrast to recent studies by Leue *et al.* (2001) and Velders *et al.* (2001), climatological averages ...".

Page 414 line 15: Change "NO_2amou nt" to "NO_2 amount".

Page 414 line 23: Change "extra terrestrial to extraterrestrial".

Page 416 line 13: Change "below the threshold" to "below a threshold".

Page 423 line 8: Change "lightning NO_X" to lighning produced NO_X".

Page 423 line 18: Change "NO_Xemi ssions" to "NO_X emissions".

Page 424 line 25: change "NOX" to "NO_X".

Interactive comment on Atmos. Chem. Phys. Discuss., 1, 411, 2001.