Atmos. Chem. Phys. Discuss., 7, S6063–S6066, 2007
www.atmos-chem-phys-discuss.net/7/S6063/2007/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "A climatology of surface ozone in the extra tropics: cluster analysis of observations and model results" *by* O. A. Tarasova et al.

O. A. Tarasova et al.

Received and published: 17 October 2007

Thank you for your interesting comments and suggestions.

In order to avoid - as you mentioned - an over-interpretation of the results, we will provide more information in our revised version of the manuscript, e.g. in form of additional graphs and an updated table.

By fulfilling the requests of the other referees, we have re-done the analysis using the same number of clusters for the model and for the measurements and we re-arranged the cluster numbers, so that related model and observational clusters have (more or less) the same numbers.

To specific comments

- The idea of using both the diurnal and seasonal cycle at the same time is very interesting. A problem is that production, titration by NO, vertical mixing and deposition can hardly be disentangled. I think that the authors should try in the future to use Ox(O3+NO2) because this at least removes the local NO titration effects, maybe a sentence could be added in the conclusions.

You are right and the mentioned approach can be interesting, but in our current analysis we are limited by the available data. We are not aware of the availability of sufficient NO_2 measurements (as for O_3). The analysis requires sufficiently long data sets (more than 10 years) and the approach you suggest can be done only for a limited number of sites. We think that the number of sites with available simultaneous measurements of O_3 and NO_2 is not sufficient. This approach could be applied to arbitrary samples from the model output, but no comparison with observations is possible then.

-In the introduction, the paper of Ordonez et al (Geophysical Research Letters, 2007, L07805) should be mentioned as it provides a possible explanation for the observations in the 90s as described by the authors;

Thanks for the suggestion. We added a corresponding text and reference.

- I assume that the model run used in this paper assumed a constant stratospheric concentration (?). It should be mentioned that this involves some uncertainties.

The stratospheric mixing ratios are calculated in the model using the full stratospheric chemistry. For more details we refer to the original description of the model setup and the performed simulations given by by Jöckel et al. (2006).

- My main concern is the over-interpretation concerning the influence of atmospheric transport versus chemistry. Even if the diurnal cycle is nearly negligible it does not mean that chemistry is not the main driving factor for the seasonal cycle. In remote areas, the ozone concentration change per day is often less than one ppb (can be both

ACPD

7, S6063–S6066, 2007

positive and negative). That means that changes over a couple of weeks or in general concerning the yearly cycle can still be due to chemistry and not to transport because the chemical processes might be slow. So the authors should discuss transport processes versus slow and fast chemical processes; I do not think that the authors can rule out chemistry that are important on time scales more than a couple of days.

Of course, chemistry plays a role at remote locations. We added some discussion concerning the formation of the seasonal minimum in the cluster of polar/remote sites, where bromine chemistry is important. We meant the role of fast in-situ photochemistry when discussing the diurnal cycles. It is difficult to separate the role of chemistry versus transport, so we can use only some indications to the processes. Moreover to discuss the role of certain processes more quantitatively, much more work is needed including meteorological and precursors information. That can be a next step in the work.

- A spring maximum is postulated but it is actually a February maximum; please correct.

Thanks for noting that, the text will be corrected. In fact this is not true for all clusters. The point which caused this misleading statement is the high standard deviation in the clusters, so that the difference between late winter an early spring is smaller than the standard deviation in each cluster detection.

- Page 12555 line 19: no seasonality of the difference between MC2 and OC2 as an indication that the stratospheric contribution is correct.. This would be a very important result. However one might argue that this could be due two compensating errors, e.g. too high stratospheric ozone contribution and too little chemistry in winter. The stratospheric contribution of 68% in winter is very high in this model and should be put into context of previous studies and other models. Please provide the exact numbers of the winter versus summer differences between MC2 and OC2?! . In any case I would add more discussion and tone down the possible implication.

In the revised version the differences between the different model and observational

ACPD

7, S6063–S6066, 2007

clusters do have seasonality. An additional graph will be included to show these numbers. A high relative stratospheric contribution does not mean really high mixing ratios. It can be rather low in absolute values, but due to the high local destruction the absolute value of the ozone mixing ratio is even lower.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 12541, 2007.