

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.

Anonymous Referee #1

Received and published: 7 October 2007

General comments

The authors present a novel cluster analysis using both observational data and model output. Such analyses should be explored more in the future. I see this paper as one of the first steps in this direction. I think that some of the found features are over-interpreted. But I think that this can be published after minor revisions.

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Interactive
Comment

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. - 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; - I assume that the model run used in this paper assumed a constant stratospheric concentration (?). It should be mentioned that this involves some uncertainties.. - 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 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. - A spring maximum is postulated but it is actually a February maximum … please correct. - 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.

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

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)