Atmos. Chem. Phys. Discuss., 7, S6067–S6072, 2007
www.atmos-chem-phys-discuss.net/7/S6067/2007/

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

We thank the referee for the detailed review and the interesting ideas.

Introduction *Some discussion should be included of previous uses of cluster analysis for the analysis of air pollution; both to cluster back-trajectories, (e.g. Cape et al 2000) and local air pollution e.g. Gramsch et al. Atm. Env. 40 (28): 5464-5475*

We do not think that a more extended introduction is necessary. Cluster analysis is one of the standard statistical methods. It is widely applied in geophysics for classification of different data sets, including air transport and air quality issues. The reference to the paper of Cape et al. was given in the paper. We do not think that the reference to Gramsch et al. should be added. In this paper the classification is carried out in a different metric (using correlation coefficients as a measure of distance between objects) and the classification procedure is unclear. We don't want to point the readers to a non-appropriate publication, this would be misleading.

Data *Model output: was all the data taken from the lowest model grid box? This should be stated.*

The lowest model level is used (90). This will be corrected in the data description.

If it was, it would have been more appropriate for some of the elevated stations e.g. Jungfraujoch to have use an elevated model box. The reason for this is that the orography at 2.8x2.8 degrees is far below the highest mountain stations. This would probably increase the number of output points for the model (there might be a lower and a higher box in the same column) and possibly allow the model clusters to capture the elevated cluster as well. Depending on the amount of work involved in rerunning the clustering this would be worth doing.

Following this comment we have re-done the analysis using different approaches in the data sampling, including horizontal interpolation of the closest grid-boxes and taking into consideration the sites altitude (by converting it to the corresponding model level). Both of the approaches showed different results, which as we suppose, do not reflect reality and therefore we decided to keep in the revised version the sampling at the lowest model level. The inclusion of the altitude information increases the number of the data sets used for the analysis. For example, taking into consideration the altitude of the sites increased the number of datasets from 72 to 93 (for some grid-boxes several levels had to be selected). But taking into account the altitude for all sites, the majority of non-elevated sites were sampled at level 89 or 88 instead of level 90. Hence, the role of dry deposition substantially decreased. This caused an overestimation of the ozone mixing ratio in most of the clusters in comparison with the measurements. Indeed, we reproduced the regime representative for the elevated locations, since this regime is represented by the grid-boxes at the levels between 85 and 82. But it is still unclear for which altitude we have to correct the sampling procedure. Since this approach is not straight-forward, we prefer to keep the processes description correct (dry deposition) and sample the data at the lowest "terrain following" level.

Statistical Analysis *I have 2 main questions on the clustering technique: 1) How were the clusters found? Was this done with a brute force method? How were the two clustering rules combined? Please give more details.*

As it is described in the text, the cluster centers were calculated as an average of the seasonal-diurnal matrices of the cluster members. The cluster membership was detected at the corresponding step of the agglomeration procedure. There is only one cluster rule: to minimize one sum (dispersion inside the cluster) AND to maximize the other sum (sum of distance of each particular cluster from all the others) at the same time. Functional minimization/maximization is a standard routine. As a measure of distance the squared Euclidean distance is used, which is not a cluster rule but a measure of distance between the objects. A graph of the total system dispersion will be included in the revised version to show the optimal number of clusters.

2) How stable is the technique to perturbations in the input data e.g. interannual variability, numbers of sites included, random error?

Cluster analysis is rather sensitive to the initial choice of data. Therefore different approaches can give slightly different results (not principally different). The classification is based on seasonal-diurnal matrices for each site, which consist of 288 numbers or variables. Each variable in the matrix is considered to be independent. To avoid the stronger impact from one variable in comparison with another, several normalization procedures can be applied. In the revised version we used the "Z score" normalization (average is 0 and standard deviation is 1.0) which gives an equal weight to all variables. This allowed us to select more clusters in the measurements. The inter-annual variability does not impact the results of the classification, since we use multi-annual averages, where inter-annual variability is already smoothed. Because we consider

ACPD

7, S6067–S6072, 2007

Interactive Comment

reliable measurements and model output and use for the analysis multi-annual averages, the random error can not be presented in the data. A sensitivity analysis of the algorithm to a random error goes beyond the scope of the present study.

Results *It seems odd to me that 2 of the stations in Anatartica shown on the map (South Pole and McMurdo) are in cluster 1 not cluster 3. Is there any reason for this?*

This can be an artifact of the statistical analysis. Actually McMurdo has a shape of the seasonal-diurnal matrix, which is much more similar to polar/remote stations, but the seasonal cycle amplitude and the average mixing ratios are closer to those of the background cluster than to those of the remote cluster. The same holds for South Pole. It is situated in the center of the elevated continent and hence has higher surface ozone mixing ratio, due to the vertical gradient. The shape of the seasonal cycle at both locations is closer to the "remote-like" shape, but the absolute mixing ratios are much higher than the average in the corresponding (remote) cluster.

In the model clusters section you state: "the maximum of the stratospheric contribution in absolute values is observed in spring." However the peak is in February for most clusters i.e. in Winter. 1 which suggests that either the observed spring maximum is not solely dynamical in origin, or the seasonality of STE is incorrect in the model. See also conclusions. It is also possible though that some of this is an artefact introduced by the fact that the lifetime of stratospheric ozone will be longer in winter than in spring as a result of lower photolysis rates. It may also depend on how the photolysis of stratospheric ozone is treated

Thanks for this note, it will be corrected. Actually, the difference between late winter and early spring values is smaller than a standard deviation of the cluster centers. Corresponding information will be added to the Table 2. We do not make any strong statement about the spring maximum being solely dynamical, we just say it is likely to be caused more by dynamics rather than by photochemistry, or in other words, that a contribution of the dynamical processes in the spring maximum formation is bigger

ACPD
7, S6067–S6072, 2007

than the photochemical contribution , which does not exclude the latter. It is likely that the seasonality of the STE in the model is not completely correct, since a certain shift has been found in the revised analysis between the observed and the simulated seasonality in the remote cluster.

It also seems to preceed the maximum in observational cluster "the model exceeds the observations by less than 8 nmol/mol". The model is lower than the observations. Therefore it is also wrong to conclude that the stratospheric contribution in the model may be overestimated.

This point will be corrected.

Any discussion of the model's contribution from stratospheric ozone should include evidence from other model evaluation exercises. For example how does the annual STE compare to the average found in the ACCENT Photocomp Experiment (Stevenson et al, 2006, J. Geophys. Res., 111, D08301, doi:10.1029/2005JD006338.) and any other model evaluations performed.

The corresponding numbers will be added in the revised paper. ECHAM5/MESSy1 is at the lowest end of the reported ranges, both w.r.t. STE and dry deposition estimates in comparison to other models in the cited publication.

Why would the stratospheric contribution be incorrect for one cluster (MC1) and not for another (MC2)?

This can happen because the model clusters cover different locations

You suggest that based on the results of model cluster 4 that the role of chemistry is overestimated. An alternative explanation would be that dry deposition in the model is too strong. Also to clarify this sentance it would be better to say that chemical destruction of ozone is overestimated.

We will reformulate that, saying that the role of in situ chemistry (both production and destruction) is overestimated. This result is even better seen with the revised cluster

ACPD

7, S6067–S6072, 2007

Interactive Comment

centers.

Conclusions *I do not agree with the conclusion that because the spring maximum is the same for all times of day that this means its origin is purely dynamical.*

Actually we don't state that, but rather suppose it.

Most of technical corrections are taken into considerations.

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

ACPD

7, S6067–S6072, 2007

Interactive Comment