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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A model intercomparison analysing the link between ozone and geopotential height anomalies in January" *by* P. Braesicke et al.

Anonymous Referee #1

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The paper compares 4 different chemistry climate models with the ERA-40 re-analysis. The period is 1980-1999 and the comparison is focused on the NH inter-annual variability in January. The coupling between the geopotential heights at 200 hPa and 30 hPa is considered as well as the coupling between the the geopotential height at 200 hPa and the ozone column. The methodology includes point-by-point correlation maps and Principal Component Analysis. The paper is relatively well written and addresses an important issue. I have, however, some serious concerns about the methodology.

1) The analysis is based on a dataset with only 19 samples. But the statistical significance of the results is never mentioned. E.g., the authors should consider the statistical significance in Fig. 1 both of the individual models but also of the differences between models and ERA-40.

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In Table 2 which deals with spatial patterns the significance is mentioned but it is wrong to say that even small numbers are significant because of the large number of points. What matters is the number of degrees of freedom which is much smaller.

2) When the EOFs are considered it is important to assess if they are well separated. If the eigenvalues of a set of EOFs are close the "true" pattern could be any linear combination of these EOFs. When the number of samples is low the risk of non-separated EOFs is high. E.g., I would not be surprised if EOF2 and EOF3 at 30 hPa can not be separated. That could explain the different polarities in Fig. 7. North's rule of thumb could be used (there are also other ways) to determine if EOFs are well separated.

3) In section 6 the authors present a study of "Covariances for reconstructed anomalies". I am not quite sure how this is done, but if it is what I expect then Fig. 10 shows the point-by-point covariance between PC1*EOF1 at 30 hPa and PC1*EOF1 at 200 hPa. I have to say that I find these plots rather meaningless. E.g., they would be forced to be zero where the EOFS's are zero. Figure 11 might give more meaning although it is not very clear how they are calculated - perhaps it is basically the correlations between the different PC's.

If the authors want to compare the coupling between the fields at different layers they could use either Canonical Correlation Analysis or Maximum Covariance Analysis (see e.g., chapter 14 in Statistical Analysis in Climate Research by von Storch and Zwiers or Bretherton C. S., C. Smith, and J. Wallace, J. Climate, 5, 541-560, 1992).

Minor comments:

The first 5 lines on page 15416 are very difficult to understand.

Are the time-series detrended before the analyses are performed.

Are the numbers in Fig. 3 the explained % of the total variance?

In section 5.1.1 there is a lot of speculations: hinting, likely, presumably. But if the

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results are not significant (as is stated but not shown) how can you say? Also note that Fig.3 could suffer from the mode mixing described above.

Section 5.1.3: It is interesting that the pattern shown in Fig. 6 for the ERA-40 data is not seen in the leading EOFs at 500 hPa. It is however found after a rotation that makes the PCs statistical independent (like Independent Component Analysis). See Christiansen, GRL 29(16), 10.1029/2002GL015208, 2002.

Page 15420, l13: developed -> confirmed

Page 15420, l21: Why should a low horizontal resolution result in more loading on EOF1 and EOF2?

Page 15421, l3: This is only in agreement if the effect of the volcanoes is mediated through the stratosphere.

How much of the analysis and results in this paper actually have anything to do with chemistry? Most of the paper deals with dynamical features, even the results involving ozone seem to be dominated by transport and dynamics.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 15409, 2007.