Atmos. Chem. Phys. Discuss., 7, S8020–S8022, 2007
www.atmos-chem-phys-discuss.net/7/S8020/2007/

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 #2

Received and published: 28 December 2007

The authors use 20-year simulations of the current climate with chemistry-climate models to investigate the mid-winter co-variability between geopotential height in the stratosphere and in the troposphere, as well as column ozone. They rely on empirical orthogonal function analysis to isolate the two leading modes of dynamical variability, namely the northern annular mode and the Pacific-North America pattern. They examine how these basic modes are represented and coupled in the various models. This is a well worthy and innovative endeavour, that is necessary for improving our confidence in CCM simulations, as pointed out by the authors. I have however some major comments that need to be addressed before this paper is acceptable for publication.

I have a major concern that the results between the different models are on many occasions very dissimilar. This is not a reason per se for not comparing them, but the authors should try to better identify the reasons behind these discrepancies. What are the important conclusions? Ultimately, one might be interested in the pattern of ozone variability linked to the annular mode, or the PNA, and whether this is well represented in CCMs (?). Sometimes the authors consider them to be similar, but for example the EOF-1 at 200 hPa (Figure 4) seem very different to me, despite the pattern correlations in Table 2: the mid-latitude centers of actions are quite differently positioned from the ERA40 data (e.g. for UMUCAM). Second, the UAquila model is in a class of its own, with extremely different patterns from ERA40 or other models (e.g. in Figs 1,2,3,6,7, 10). The results are so different that it seems to me that this should be investigated separately. For the other models, there are many instances, when model correlations or even EOFS are very different (Figs 10 and 12 for E39C), (Figs 1, 9 for UMUCAM).

2) I have a concern about their interpretation of column ozone and upper troposphere geopotential correlations. In mid latitudes, they find the familiar anti-correlation (trough or low tropopause associated with high column ozone), but the correlation reverses at high latitudes. Even at high latitudes, a depressed tropopause should lead to elevated ozone column. They attribute the reversal to the more controlling role of the polar vortex, which influence meridional motions (negative geop anomaly associated with stronger vortex, less meridional motions). Another way to look at this is that the strong vortex is associated with high PV, upward bulging isentropes, hence higher tropopause, and lower ozone. This mechanism, not invoking meridional motions, is discussed in Ambaum M., Hoskins B. et al. (2001) or Orsolini and Reyes (cited, 2003). This needs clarification.

3) I wonder about the robustness of results, based on 20 Januaries; the annular mode is often calculated for the cold season (Nov to Apr) or for the winter months at least. Have the authors checked that the identified EOFs are near-identical if they use D, J, F. In the case of the EOF-1 at 200 hPa, there are model structures in the Pacific and North Atlantic that are not readily similar to the ERA40 pattern. Same point for EOF-2 of ozone. A standard method for looking at such joint variability would have been the

ACPD

7, S8020–S8022, 2007

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

SVD method. Why have the authors rather chosen their approach ?

4) Figure 3 is not terribly useful as we do not see the calculated EOFs pattern for the other months. There are several papers showing that the summer Northern Annular Mode has a different meridional extent than its winter counterpart (Ogi M. et al., JGR, 109, D20114, 2004).

6) There is a lot of literature on the PNA extension into the stratosphere through the forcing by the ENSO phenomenon. It seems that this could be discussed more in relation to Figure 11 (coupling modes 2 and 2).

7) The link of Figure 14 showing mean temperature structure to the rest of the paper and its conclusions (e.g. the dominant coupling of the stratosphere with PNA for the E39C model) is rather unclear.

Minor comments :

1) The sentence 'Note the use of a very direct approach in deriving EOFs.' on page 15417 makes little sense. 2) In Figure 6, the other wave train emanating from North America across the North Atlantic in addition to the PNA (see ERA40 map) is likely related to the Aleutian-Icelandic seesaw, extensively discussed by Honda and Nakamura (J Climate, 14, p4512, 2001), and its strong impact on column ozone by Orsolini (J. Met. Soc. Japan, 82, p941, 2004). Only one model seems to capture it (UMUCAM).

3) Care should be taken in using column ozone rather than ozone, and not using 'total column ozone' 4) The paper is a bit long and wordy. I would suggest to remove the use of partial column ozone, as it seemingly brings little extra information. Also Section 6.1 is very difficult to follow without the corresponding figures, and could be made shorter.

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