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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Variability and trends in total and vertically resolved stratospheric ozone" *by* D. Brunner et al.

Anonymous Referee #3

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This paper presents an analysis of interannual variability and trends in stratospheric ozone based on a new ozone profile data set (CATO; described in a recent paper by Brunner et al, JGR, 2006). The CATO ozone profile data are 'reconstructed' statistically from column ozone observations using meteorological analyses (specifically, temperature and potential vorticity fields) to distribute ozone variability throughout the vertical profile to match the column measurements. The calculations are performed sequentially using a Kalman filter, with specified structure for the ozone and forecast error covariances (intended to optimize the results). While the details of the CATO analysis are substantial (and somewhat beyond my expertise), the net result appears to be that the statistical reconstruction technique is able to provide vertical ozone profile information from measurements of column ozone. This is demonstrated in the Brunner 2006 JGR

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paper by some seasonal comparisons between CATO and ozonesondes and HALOE satellite data, although there are clear biases in the CATO reconstructions in the tropics. The comment is also made in that paper that CATO is able to capture interannual variability, but that is demonstrated only superficially. Overall this is a novel and highly interesting new ozone data set, but substantial work will be needed to document and understand its strengths and weaknesses.

The current paper is a first look at long-term variability and trends in CATO, using a standard multiple linear regression analysis. There are some changes to previous statistical analyses, in particular the introduction of a new EP flux proxy for planetary wave effects. Overall the results seem reasonable and are in broad agreement with well-documented ozone variability, and I think the paper should be published at some point. However, it is unclear to me how reliable the CATO data are for capturing interannual variability in ozone profiles, and this is a key point that should be addressed before interpreting a sophisticated multivariate regression analysis. While the CATO data are probably reasonable for assessing many aspects of global ozone variability, there are several reasons to be suspicious of longer-term changes. The CATO scheme depends on both potential vorticity and temperatures that are derived from ECMWF reanalyses, updated (after 2002) with operational analyses. There can be substantial changes and discontinuities in the ERA40 data products in the stratosphere (including unrealistic temperature trends), that will influence the CATO reconstructions. Also, analyzed PV fields in the tropical stratosphere are poorly constrained by meteorological observations, and are often problematic (this might be one reason for the CATO biases in the tropics). Even using equivalent latitude coordinates, there will be very few (zero?) column ozone observations inside the Antarctic polar vortex in winter, so that I wonder what contributes to CATO results in that region. These details make me suspicious of the long-term variability in CATO that is the focus of this paper. One suggestion I have would be to make it clear up-front that this is an analysis of the (new) CATO data set, and not necessarily the real atmosphere.

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I have a series of comments on details of the analyses and results:

1) I think it would be very useful to show some time series of ozone anomalies from CATO data at specific locations in comparison with other data sets (from sondes and/or satellites), to demonstrate that CATO is producing reasonable results for interannual variability (this might also highlight some of the limitations). Useful comparisons might include time series from ozonesondes in NH midlatitudes (focusing on variability and trends) and satellite data in the tropics (highlighting the QBO).

2) The proxy for EP flux used in the statistical analysis is a new calculation, intended to integrate (and damp) the effects of wave forcing on ozone for each month. This is different from previous calculations (Fusco and Salby, 1999) that demonstrated correlations between ozone tendency and EP flux. Because this is a new calculation in this paper, it would be useful to demonstration that the EP flux proxy behaves in a reasonable way (some time series would help). As a note, it seems curious that the damping time used in the Eq. 4 is changed between the tropics and extratropics (the explanation that the photochemical lifetime of ozone is short in the tropics is not correct below ˜30 km). Also, I don't understand why the sequence of months is shown in Fig. 5, as there is very little month-to-month change (this is not surprising given the construction of the proxy). The explanation for the positive EP flux correlations above 25 km in 3.1.4 is handwaving and unconvincing. Also, given the statement that details of the VPSC signal are spurious due to poor separation with EP flux, interpretation of the details in Fig. 5 is questionable.

3) As acknowledged by the authors, the solar cycle maximum in Figs. 3 seems low (in comparison to results from SAGE), and this is certainly related to details of the CATO data set. The inclusion of SBUV data in the upper stratosphere appears to make almost no difference for this result (Figs. 3 and 4 are very similar), although I am skeptical of the SBUV results due to satellite inter-calibration uncertainties in the v8 retrieval. Can the authors clarify the influence of solar cycle variations in ERA40 data (see Crooks and Gray, J. Climate, 2005) on these ozone results? Overall I think there

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is little confidence in the solar signal vertical structure from the CATO data. There is also a large negative solar response below the tropical tropopause in Fig. 3d (where there is very little ozone). In fact, there are spurious signals in many plots in this paper below the tropical tropopause, and it would be better to omit these from the figures.

4) The ozone QBO pattern shown in Fig. 2a is very similar to that shown in Baldwin et al (Rev. Geophys, 2001, their Fig. 23). The QBO cross sections in Figs. 3a-b look quite good in comparison to Randel and Wu (1999), which is based on high resolution SAGE observations. In fact, I am amazed at the detailed QBO vertical structure in the tropics in these data; can you explain how such complex vertical structure can be derived from column ozone data by CATO? (does it have something to do with the tropical QBO temperature or PV variations derived from ERA40 data?).

5) The cross sections of ozone trends (Fig. 7) show largest negative trends in the lower stratosphere, which is in reasonable agreement with previous analyses (this is where most of the ozone resides). But isn't this result fundamentally constrained by PV and temperature trends in the ECMWF data (in addition to column ozone trends), which can have substantial uncertainties? Although not mentioned in the text, there are significant positive ozone trends over 25-30 km in the tropics in Figs. 7-8 that have not been observed in other data, and seem to be an artifact.

6) The calculation of ozone residuals (Figs. 9-10) for the different statistical models is a nice addition to this paper. I especially like the fact that the full model explains most of the variability. There seem to be persistent positive residuals in the tropics in Fig. 10 that are not commented on (similar location to the positive ozone trends noted above), and also in the extratropical lower stratosphere in both hemispheres. Are these significant?

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 6317, 2006.