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

D. Brunner et al.

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We would like to thank the referee for a constructive and stimulating review and for the detailed comments. The suggested additions to the manuscript will allow the reader to better understand the strengths and limitations of the presented data set, a concern which was most prominently raised by this referee but also shared by the others.

Reply to major points

The first concern of this referee is whether CATO is able to capture actual interannual variability and trends and that this question is not sufficiently addressed in the Brunner et al. 2006 JGR paper or in the present manuscript. Since the same concern was also raised by referee #1 we here refer to the detailed response to that referee. In order to better demonstrate the capabilities of CATO with respect to the representation of

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interannual variability and trends we have added a new figure comparing anomaly time series of CATO with SAGE and ozonesonde data at different levels and significantly extended Sect. 2.1 presenting the CATO data set (see response to referee #1).

Concerns regarding the quality of the meteorological data sets used for the reconstruction of CATO have also been raised by referee #2. It is true that there are substantial changes and discontinuities in ERA-40 data. However, in the lower stratosphere (below 10 hPa), which is the main focus of this study, the quality of ERA-40 temperatures is considered to be high and more or less stable since 1979 (Adrian Simmons, personal communication). The data are suitable for trend studies as demonstrated by several studies on ERA-40 temperatures in the lower stratosphere now referenced in the revised manuscript. Sect. 2.1 has been extended in the revised manuscript to include this information. For details see our response to referee #2. It should also be noted that ERA-40 data have already been used to study solar cycle and QBO effects (Crooks and Gray, J. Clim., 2006; Pascoe et al., J. Geophys. Res., 2005).

Because winds are not constrained by geostrophic balance it is generally believed that PV values in the tropics are not very reliable. Our comparison with HALOE data in the Brunner et al. 2006 JGR paper (see Figure 15 in that paper), however, provides a different picture. The agreement between individual HALOE O3 profiles and profiles of CATO interpolated onto the equivalent latitude profiles of the HALOE measurements surprisingly showed the best agreement in the tropical lower stratosphere. This suggests that variability in tropical ozone profiles associated with meridional transport can very well be diagnosed by PV. There are also a number of other studies (e.g. Borchi et al., ACP, 5, 1381-1397, 2005; Zachariasse et al., JGR, doi: 10.1029/2001JD900061, 2001) indicating that PV fields contain valuable information also in the tropical lower stratosphere and upper troposphere.

CATO results over Antarctica indeed have to be interpreted with care. The problem of missing data during southern hemispheric winter was already addressed to some extent in our JGR 2006 paper (see Figures 4 and 14 and discussion in Sect. 4.2).

In response to a comment of referee #2 we have added a new figure showing the performance of the regression model in terms of R2 values as a function of equivalent latitude and month. This new figure (Fig. 3) clearly shows the limitations of CATO over Antarctica during winter. We have added some cautionary remarks in Sect. 3.1.

Reply to individual points: 1. We have added a new figure as suggested comparing time series of CATO O3 anomalies with SAGE data and with a selected sonde station (Payerne). The new figure (Fig. 1) is included in Sect. 2.1 (description of CATO data set) which has been significantly extended.

2. EP flux proxy: The way we are relating changes in O3 to EP flux is not that different from the approach of Fusco and Salby (1999). They correlated wintertime ozone tendencies with wintertime accumulated EP flux. Since total ozone levels are always very similar in autumn (e.g. in October), variations in spring (e.g. April) ozone values are very similar to variations in October-April ozone tendencies. Despite the construction of our proxy including a relaxation term, its April values, for example, are more or less identical to an EP flux accumulated from October to April (R=0.975). This information is now included in Section 2.2 which presents the regression model. The referee is right that the large change in damping time between tropics and mid-latitudes is not justified. Our estimate of a 1 month decay time in the tropics was actually based on Figure 5.3 in the book “Aeronomy of the middle atmosphere” by Brasseur and Solomon (2nd ed.) from which we derived an ozone lifetime near the altitude of the tropical ozone maximum of about 1 month. Looking at the figure more carefully and comparing with the clearer Figure 14.10 in the book "Atmospheric Chemistry and Global Change" by Brasseur, Orlando, and Tyndall, (Oxford University Press, 1999) a lifetime of 3-4 months actually seems more realistic. We therefore repeated all regression model calculations using a 3 month decay time in the tropics (same as in mid-latitudes during summer). Figure 5 is intended to show the evolution of the ozone signal associated with the Brewer-Dobson circulation throughout the ozone build-up (winter) and decay (spring-summer) phase. There may be a misunderstanding concerning the way Figure

5 was created. The colour contours show the percentage change in ozone for the given month of the year for a 1 sigma change in the EPFLUX proxy for the same month. We changed the figure legend to make this clearer. The individual panels thus show the interannual variability in ozone in a given month that can be expected from interannual variations in the strength of the Brewer-Dobson circulation. We strongly believe that the figure contains valuable new information that has not been presented before. We have changed a few sentences in Sect. 3.1.4 to reflect the reviewers concern that we should probably not over-interpret the results in Figure 5 above 25 km. Note that the possible interference with VPSC does not influence the results in the southern hemisphere because southern hemispheric VPSC is not correlated with northern hemispheric EP flux.

3. Solar cycle response: The maximum solar cycle signal in CATO around 30 km altitude made us wonder whether this is only an artefact of the reconstruction method which is not able to reproduce the variability in the upper stratosphere. Any real variability there with a measurable influence on the total column therefore tends to be placed lower down in CATO. We therefore decided to make an additional run assimilating SBUV data into the upper levels (above 820 K potential temperature) where the CATO methodology fails. Surprisingly this did not change the picture much because the SBUV V8 data showed little response to the solar cycle at the assimilated levels. The solar signal in SBUV data may actually be affected by an upward shift in SBUV levels by about 4% from 1995 to 1998 (when SBUV was measured on NOAA-9) that has recently been reported by Steinbrecht et al. (JGR 2006). This upward shift happened to be during a solar minimum and may therefore contribute to an underestimation of the solar cycle signal. A second problem described in our response to referee #2 may be the sensitivity of the assimilation of SBUV data into CATO to the ERA-40 temperatures. This information is now included in Section “Solar cycle” which has been further adjusted following recommendations by anonymous referee #2 and J. McCormack. Signals in the tropical upper troposphere are now blanked in all figures and are not discussed in the paper anymore. Crooks and Gray (J. Clim. 2005)

investigated the solar cycle signal in ERA-40 temperatures using a regression model based on classical variables to describe solar, volcanic, NAO, ENSO, and QBO effects. Variations in EP-flux were not considered (directly) which may be an important difference from our approach. They found a general increase in low latitude stratospheric temperatures between 15 and 55 km with a distinct maximum around 42 km. They further found statistically significant positive anomalies at about 25° latitude in both hemispheres which resemble our solar O3 responses. However, their anomalies are at a lower altitude (below 20 hPa, our signal maximizes above 20 hPa) and not exactly at the same latitude. Thus, there is not an exact match between the signals. Even then, there would be no reason to believe that the CATO signal would be wrong unless the ERA-40 temperature signal would be wrong as well. For further information on the way the CATO reconstruction is connected to ERA-40 temperatures see our response to referee #2 (and Sect. 2.1 in the revised manuscript).

4. QBO structure: The successful reproduction of the QBO in CATO is an excellent verification that the reconstruction method is working properly. It demonstrates that the method does not simply scale a given vertical profile to the measured total ozone value but that it can capture different changes at different levels. The reconstruction method attributes that O3 amount (sub-column) to a given potential temperature layer which best explains the variability in measured total O3 columns associated with meridional transport in that layer (as diagnosed by PV). If the ozone volume mixing ratio in the layer were generally too small then also the variability due to transport in that layer would be too small. The reconstruction thus only uses the short-term fluctuations in PV. Long-term PV changes associated, for instance, with the QBO have no direct influence, but the QBO temperature signal certainly has an effect on the reconstruction as it varies the altitude and thickness of the potential temperature layers. This does not cause an artefact in the reconstruction but this is exactly what it should do (see Wohltmann et al., GRL 2005).

5. Long-term trends in PV are not expected to have any influence on the reconstruction

because it is only the short-term component of PV fluctuations that is used. ERA-40 temperature trends in the lower stratosphere, on the other hand, could potentially affect CATO, but they appear to be of sufficient quality in the lower stratosphere such that we do not expect a large bias in CATO. However, this question can not be answered satisfactorily at this stage without a more complete study of the quality of ERA-40 temperature trends in the stratosphere and a detailed study of the sensitivity of CATO to such uncertainties. We have added a cautionary remark in the conclusions that this is a critical issue. We have also significantly extended Sect. 2.1 to include more information on what meteorological information goes into CATO and how this may affect the reconstruction. For further details we again refer to our response to referee #2. It is not quite correct that a positive trend over 25-30 km in the tropics has not been seen in previous studies. Trend analyses based on SAGE also show such a positive tendency (see Cunnold et al., JGR 105, 4445-4457, 2000 or Wang et al., 2002) though usually not statistically significant. A possible explanation is that it is a response to the ozone depletion above which could lead to increased UV radiation at this level. This is now stated in the manuscript.

6. It is true that there are persistent positive residuals in the tropics (Figure 10c in first manuscript, Fig.12c in revised version). These values are significant but their origin is not clear. This feature may be related to the oscillatory structure that is seen in the downward trends (former Fig. 7b, now Fig. 9b) showing a local minimum around 40 hPa and larger trends below and above. The comparison with SAGE and ozonesonde data (new Fig. 1) suggests that this is an artefact of the reconstruction method. It seems that trends are overestimated at 30 hPa but underestimated at 40 hPa. This is now mentioned in the discussion of Figure 1 as well as in Sections 3.2 and 3.3.

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