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Interactive Comment

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 very constructive review and for detailed and highly valuable 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 shared by all referees.

Reply to major comments

1. Underlying temperature trends: The referee is right that this is a point of relevance for the CATO data set not receiving sufficient attention in the first version of the manuscript. The sensitivity of the CATO reconstruction to the ERA-40 temperature field is fairly complex. A direct influence can be expected because the CATO method reconstructs ozone on potential temperature layers. An erroneous temperature trend

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at some altitude will result in an erroneous trend in the (pressure) thickness of potential temperature layers around that level. The reconstruction method of CATO tries to attribute that O3 volume mixing ratio to a given layer which best explains the variability in total ozone columns associated with meridional excursions of air in that layer. Thus, a wrong layer thickness would result in a wrong volume mixing ratio. The temperature distribution also affects the potential vorticity field. However, CATO uses the PV field only to diagnose transport on potential temperature levels. The reconstruction method does not depend on absolute PV values and therefore erroneous long-term changes in PV are not expected to affect the reconstruction. In section 7(d) of the ERA-40 overview paper by Uppala et al. (2005) it is pointed out that large biases exist in upper stratospheric temperatures (e.g. at 3 hPa) which vary depending on the satellite data available for assimilation. However, temperatures are much better at levels below about 10 hPa which is the main focus of our study. The study of Santer et al. (2004) (see their Figure 5) shows that the evolution of ERA-40 temperature anomalies in the lower stratosphere agrees very well (within about +/-0.2°C) with the evolution of temperatures of the Microwave Sounding Unit (MSU) in channel 4 over the period 1979-2001, though the overall downward trend is somewhat smaller in ERA-40 (-0.3°C/decade compared to -0.39 to -0.49°C/decade in MSU). The MSU channel 4 has a peak sensitivity at 74 hPa and is considered to be a good representation of lower stratospheric temperatures. The high quality of the ERA-40 temperature data in the lower stratosphere is also confirmed by the study of Labitzke and Kunze (2005). Differences in the long-term mean (1957-2001) temperatures at 30 hPa and 50 hPa between ERA-40 and the analysis of the Freie Universität Berlin (FUB) are very small (mostly within +/-0.5 K) over the whole northern extratropics analyzed in that study. In the same study they also analyzed temperature trends over the Arctic. The agreement between FUB and ERA-40 of Arctic temperature trends between 1979 and 2001 is generally very good at 50 hPa. At 30 hPa the agreement is good only in spring and summer but not between late autumn and early winter (October to January) where ERA-40 trends are distinctly positive whereas both FUB and NCEP reanalyses show negative trends. The high quality of ERA-40 temperatures in the lower stratosphere is further confirmed by the SPARC Intercomparison of Middle-Atmosphere Climatologies (Randel et al., 2004) showing that ERA-40 has an excellent representation of the QBO up to 10 hPa and a good representation of the latitudinal structure of 100 hPa temperatures. Stratospheric temperatures of ERA-40 below 10 hPa thus appear to be generally of high quality in particular in the northern hemisphere with good coverage of radiosonde stations. Over polar regions, however, there are some problems including the wrong positive temperature trend at 30 hPa for the months October to January and an unrealistic vertically oscillating temperature structure related to problems with the assimilation of SSU and AMSU satellite data. The latter problem affects Arctic temperatures only after 1998 (Antarctic over whole satellite era) and mainly occurs in winter and spring months (Karpetchko et al., 2005). We have added a few remarks in Sect. 2.1 concerning the use of meteorological fields for the reconstruction of CATO and how they may affect the results including all references mentioned above. At this stage it is very difficult to give a firm conclusion on how ERA-40 may affect the CATO reconstruction before a more complete analysis of ERA-40 stratospheric temperatures is available. ERA-40 data in the stratosphere have also been criticized for showing a too strong Brewer-Dobson circulation leading to low values of the age of air. Note that the CATO reconstruction method does not make use of the noisy and problematic vertical wind data of ERA-40 and thus largely avoids these problems. This was already mentioned in the Brunner et al. JGR paper but we also added a corresponding remark to this manuscript.

2. The way the vertical ozone distribution is reconstructed in CATO (see first section above and first publication by Brunner et al. (JGR, 2006)) explains why CATO is able to capture chemical changes that act only on a part of the profile. Assimilation of SBUV is not required, at least not below 30 km. We think that the results presented in this manuscript clearly demonstrate this capability of CATO. Note that in response to the other referee's comments we have added a new figure comparing time series of CATO at different levels with time series of SAGE and sonde data. The new figure demonstrates that CATO not simply scales the variability observed in total ozone values

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over the profile but that it actually captures the different trends and variability at different altitudes in the stratosphere.

3. The referee suggests showing Rsquare values as a function of latitude, season and pressure. This is indeed a good suggestion. We have added a new figure (now figure 3) as suggested and extended section 3.1 to include a discussion of this figure. The new figure nicely show the strengths and limitations of the regression model.

4. Solar cycle results: We fully agree with this referee's remarks. There is little consensus on the exact solar cycle signal at the moment due to the short records. We never had the intention to show what the solar signal is REALLY looking like. We only tried to point out that the response we find looks different from previous studies and that there might be good reasons for this (different data set, different approach for separating from other effects, problems with separating other effects). Maybe we have been too optimistic regarding the capability for better separating between different effects compared to earlier studies owing to the better time coverage of our analysis. Without further sensitivity studies this remains hypothetical. We have therefore changed several sentences in Sect. 3.1.3 to caution the reader of the limitations of present solar cycle analyses, as suggested by the referee. The referee is also right that issues with the ERA-40 reanalysis data set in the upper stratosphere could affect our SBUV results. SBUV data are available on pressure levels but are assimilated into CATO on potential temperature layers. The pressures of these layers are given by the reanalysis. Thus, if there are errors or steps in the ERA-40 temperature data, there will be errors or steps in the assimilated SBUV values because different parts of an SBUV profile will be assimilated into a given layer. We have added a few cautionary remarks regarding this really important problem.

We find the largest signal lower down than in other studies, which may not be surprising given the limitations of the CATO reconstruction method in the upper stratosphere. Somewhat surprising to us, however, is the fact that assimilation of SBUV data does not change much. The solar cycle signal we find in SBUV data between about 35

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and 42 km is really small. The study of Steinbrecht et al. (JGR 2006) provides some indication that this could at least partly be an artefact of the SBUV version 8 data, which show an upward shift by 4% between 1995 and 1998 (when SBUV was operated on NOAA-9) coinciding with a solar minimum. Such a bias, if real, would clearly damp a positive solar cycle signal. The second problem already mentioned above is that biases known to be present in the ERA-40 temperature data in the upper stratosphere could affect our results.

5. We do not agree with the second comment regarding solar cycle effects. The “self-healing effect” of stratospheric ozone the referee appears to be referring to does not apply to a situation where solar UV radiation is enhanced. The idea of self-healing is that when ozone increases /decreases at an upper level, less/more UV light reaches the lower altitudes leading to an ozone decrease/increase there. However, if solar UV radiation is generally enhanced, ozone can increase at the upper levels without leading to a decrease in UV light available at the lower altitudes compared to the case of normal solar UV output. This seems to be confirmed by a number of model studies cited in the manuscript.

6. Saturation of ozone loss over Antarctica: This is a good point. A simple linear model is clearly not able to deal with saturation effects. We have added some cautionary remarks to both Sections 3.2 (Ozone trends) and 3.3 (Ozone changes since 1996) as suggested. We have included the Newman et al. (2006) reference in response to comments by referees #1 and #3 concerning the use of the “right EESC function”. We also added a second reference to a publication by Jiang (JGR 1996) on saturation of ozone depletion over Antarctica.

Reply to minor comments

1. Abstract. We have somewhat enhanced the abstract by providing more quantitative information.

2. Equation 1 and 2. The time-dependence of the coefficients as stated in Eq. 1

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and 2 may indeed be a bit misleading (though not incorrect and also used e.g. in the study of Ziemke et al., JGR 1997). We have removed this as suggested. The definition of the time dependence of these coefficients, i.e. their relation to the coefficients of the harmonic expansion that are ultimately estimated by the statistical model, is then described in Eq. 3 which we left unchanged. Note that a dependence on time modulo 12 is still a dependence on time.

3. Table 2. We agree that it is more useful to show the values normalized by the standard deviations because this makes the values better comparable and interpretation of the figures more straightforward. We have changed the table accordingly. Standard deviations are still presented in the respective units whereas all other values are normalized. The standard deviation values are still useful because it may be of interest to know how much ozone changes for instance per 1 m/s change of the QBO, or per 1 ppb change of EESC. The significance levels of the trends are still included because they can not be inferred from the magnitude of the (normalized) trend. E.g. the linear trend fit to the QBO proxy for the period 1996-2004 has a large value but is not significant because the shape of the QBO signal does not nearly resemble a linear trend.

4. EP-flux and VPSC proxies in Figure 1: The EP-flux proxies are now shown as deseasonalized values as also requested by referee #1. The mean seasonal cycles are shown as small insets. We think that interannual variability of the VPSC proxies is sufficiently well visible. In addition, subtracting a mean seasonal cycle is not very useful because the amplitude of the seasonal cycle varies with time due to multiplication with EESC.

5. The tropical upper troposphere is now blanked in all figures and text referring to variability and trends in this region has been removed.

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