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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 #2**

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On the whole this is a well written paper. The analysis is thorough and generally well founded. The presented material usually supports the conclusions (see below for exceptions). Results are put into the context of existing publications. Although the paper presents no major new results, it uses a new data set and obtains plausible and very clear information on long-term and interannual ozone variations.

I suggest publication in ACP, after the authors have addressed the following comments.

# 1 Major Comments

Pages 6322/6323: I think you need to say more about the stratospheric temperature trends and about CATO in general. Which meteorological fields go into CATO? What trends/ step-like changes do they have (also relevant for page 6325, line14/15)? How realistic are the underlying temperature trends? Give some information plus references. Since CATO is based on total ozone and meteorological fields only, what confidence can we have that it captures chemical changes that act only on a part of the profile? Solar cycle changes might be one such example. I realize that assimilating SBUV may help a bit, but still this seems to be a limitation of the CATO data set that needs to be mentioned.

Section 3: I am missing a section on how well the regressed time series capture the full variance. What fraction of the full variance are captured by the regression terms. How large are the $R^2$ values, as a function of latitude and month of the year, and as a function of altitude. I think a statement on this important aspect is absolutely necessary, but currently missing in the paper. I think two additional figures, analogous to Fig. 2 and 3 are required.

Section 3.1.3: I concur with John McCormacks comment on the solar cycle results. We should all beware of over-interpreting details of our solar-cycle results. The current records are too short. Competing effects from QBO phase, QBO vs annual-cycle, volcanic aerosol, and increasing EESC in the last maximum cannot really be separated on the basis of existing records. Most models are highly idealized (e.g. compare steady state solar min vs. solar max, do not have a fully realistic QBO with changing phase against an annual cycle, cannot explain the lack of volcanic effects in the Southern hemisphere, do not include possible effect of high $NO_x$ events in the mesosphere ...).

For all data sets, but especially for SBUV there may also be a problem with intersatellite calibration (NOAA 9 SBUV/2) shifting the data level by several precent after 1993 (e.g.



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Steinbrecht et al., 2006). This, or changes in the upper stratospheric reanalysis data, may very well affect the magnitude of the solar cycle effect in CATO data. Lets not over-interpret details. Please consider these informations, and add some cautionary remarks to your text (section 3.1.3, conclusions, abstract).

One more comment on the solar cycle effect: I would expect that increased ozone near 10 hPa (or higher) would also reduce photochemical production of ozone below. So having a low ozone region below a high ozone region seems plausible. I don't see anything like that in Fig. 4.

Page 6341 lines 10-15, also Discussion of Figs. 9d and 10d, Appendix, Fig. 11: I think some statements on the saturation of ozone loss in the Antarctic hole need to be added. In large parts of the ozone hole, all ozone is destroyed. Despite increasing chlorine levels, ozone loss has, therefore, not been able to increase. Several indices describing the ozone hole have been saturated since about 1996 (e.g. Newman et al., 2006). The fact that the pre-1996 trend has not continued in the same way after 1996 is not a sign of a beginning recovery. Saturation is not correctly described by a model that is linear in chlorine loading.

## 2 Minor Comments

Abstract: Please provide quantitative information in the abstract. How large are variations associated with QBO, volcanic eruptions, the solar cycle, and the other factors? How large are the ozone trends?

Eq. 1 and 2. I think the $t$-dependence of the coefficients is misleading. They do not depend on $t$ as defined in the text. They only depend on $t$ modulo 12. I would omit the $(t)$ from $a(t)$, $b(t)$, $c_j(t)$.

Table 2: I think in the present units, the trend/decade columns are not very meaningful.



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To me the question is, how large a trend is compared to the standard deviation. If small compared to the standard deviation, then it is not a very meaningful trend. I think it would be better to give the trends in terms of standard deviations per decade. If all values in Table 2 were normalized to 0 mean and standard deviation 1, then the columns Units, Mean, 1 sigma, and possibly also the significance columns can be omitted. Standardization would also be consistent with Figs. 2 to 5, where ozone changes per 1 sigma of explanatory variable are given. I suggest to use normalized values in Table 2 and to omit the irrelevant columns.

Figure 1: Year to year variations in EP-Flux (N and S) and VPSC-S are very hard to see. I would much prefer to see data where the average annual cycle has been subtracted. (The annual cycle could be shown in a small inset, or in a small extra figure). After taking out the annual cycle, the year to year variations will become much clearer. It will probably become obvious that VPSC-S is very similar to EESC, and therefore both together won't work well in a regression.

All Figures that show a quantity versus altitude and latitude: The authors repeatedly state that CATO results should not be trusted at low latitudes (-30S to 30S ?) in the upper troposphere/ lower stratosphere (below 100 hPa). Would it not be much better to blank this region in the figures, rather than show large features and then explain that they should not be believed? I think the authors are presenting enough reliable information. They should spare the reader the unreliable information.

## 3 References

Newman P. A., E. R. Nash, S. R. Kawa, S. A. Montzka, S. M. Schauffler (2006), When will the Antarctic ozone hole recover?, Geophys. Res. Lett., 33, L12814, doi:10.1029/2005GL025232.



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Steinbrecht W., et al. (2006), Long-term evolution of upper stratospheric ozone at selected stations of the Network for the Detection of Stratospheric Change (NDSC), J. Geophys. Res., 111, D10308, doi:10.1029/2005JD006454.

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