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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.

D. Brunner et al.

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We would like to thank the referee for very valuable comments and criticism. We believe that the changes and additions suggested by this and the other referees 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 general comments

The main concern of this referee is whether CATO is able to reproduce actual long-term variations in vertical ozone. Similar concerns were raised by the other referees. It is true that the original paper (Brunner et al., JGR 2006) does not provide enough material to demonstrate this. However, the present paper provides a lot of support for this by demonstrating that variability associated for instance with the QBO or with volcanic

aerosols is represented in CATO in a way that is highly consistent with previous studies. Referees #1 and #3 propose to show some time series of CATO and other observations (sondes and/or satellites) at different altitudes to demonstrate that it captures the main features observed in other data sets. Following these recommendations we added a new figure which compares the time evolution of CATO ozone anomalies at 45°N equivalent latitude and at two different altitudes (at 120 hPa or ~15 km and at 40 hPa or ~22 km) with one selected ozonesonde station (Payerne, 47°N, 7°E) and with SAGE-II V6.2 data averaged over 40-50°N geographical latitude. The new figure demonstrates that the variability and the largely different evolution at both levels seen in the sonde and SAGE data are well captured by CATO. A detailed discussion of this comparison is now included in the manuscript along with the new figure. We believe that this additional section convincingly demonstrates the capability of CATO to represent long-term lower stratospheric ozone variability. There is no single data set representing "real trends" because they are all based on measurements with more or less significant uncertainties. Nevertheless, we changed the title of the manuscript to reflect the fact that our analysis is based on the CATO data set and not the "real atmosphere" as suggested.

The referee is concerned that "total ozone long-term variations may have too much influence on CATO long-term variations". We believe that the reconstruction from total ozone measurements is actually a great advantage of CATO, as it is the only available data set providing long-term trends in the vertical distribution that are fully compatible with total ozone trends. There is probably no other ozone data set of the same quality as the network of Dobson stations (against which our satellite data were calibrated) with respect to long-term trends. In particular, data from SBUV and ozonesondes are much more critical in this respect. We believe that the new Figure 1 comparing CATO with SAGE-II and Payerne sondes convincingly shows that the CATO method is able to reproduce the actual variability and trends at the different levels in the stratosphere and does not simply scale a given vertical profile to the changes in total ozone.

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Reply to specific comments

1. Unfortunately, there was a mistake in the paper concerning the number of coefficients actually used. We have tested many different configurations reducing or increasing the number of harmonics for all or only for a selection of proxies. The version finally presented uses 12-month and 6-month harmonics for all parameters, but not 4-month harmonics. We thus only estimated 5 coefficients per proxy not 7 as incorrectly stated in the manuscript. Please note that for most of the vertical cross-section figures a statistical model without harmonic expansion was applied (except for the seasonally varying intercept a), because only annual mean effects were considered. In this case only 7+5 = 12 coefficients had to be estimated. This was explained somewhat hidden on page 6330. We have now placed this statement more prominently next to the description of the statistical model. The choice of number of harmonics was based on an analysis of adjusted R2 values which maximised when using 2 harmonics per variable. We revisited this issue testing each variable separately. For all variables we find a reasonable improvement in the quality of the statistical model (based on R2 values) when using 2 harmonics instead of only 1. A clear exception is the solar cycle, which can equally well be represented by only one harmonic. We have therefore recomputed the total ozone results using 3 instead of 5 coefficients for the solar cycle leaving everything else as before. The referee is right that the EP flux and VPSC proxies were constructed in an attempt to mimic their seasonally varying influence on ozone (Eq. 4 in manuscript). Nevertheless, since the same proxy is used at all (extratropical) latitudes and the timing of the response varies between the latitudes, we found it necessary to include the harmonic expansion.

2. The referee suggests studying the effect of El Chichón separately by excluding the period 1991 to 1995 because the volcanic effects presented in the paper are dominated by Pinatubo. We agree that this would be interesting. The analysis of volcanic effects based on CATO has a large potential in particular because other global observations (e.g. from SAGE) have been heavily perturbed by the enhanced volcanic aerosol.

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However, due to significant other additions we have made to the manuscript (two new figures and corresponding text) we refrain from including a separate analysis of El Chichón effects. Instead, we now make a clear statement in subsection “Volcanic eruptions” that the aerosol signal is essentially that of Pinatubo and that El Chichón would require a separate analysis.

3. Choice of EESC data set: The source of our EESC data was already indicated in Table 1. We are well aware of the fact that EESC as used in this study is more appropriate for mid-latitudes than for polar regions. A corresponding warning remark was already made in section “Ozone trends”. We agree that this information should be given earlier in the paper. We therefore moved this part to the Section “Regression model” and included the Newman et al. (2006) reference.

4. Size of autocorrelations: In case of total ozone the first autocorrelations are on the order of 0.56 +/- 0.11 at the different equivalent latitudes. The first autocorrelations are of a similar order of magnitude for the regressions at the different levels but the decrease from about 0.6 at the lowest to 0.2 at the upper levels. The higher “memory” at lower levels can probably be explained by the higher “memory” of ozone itself due to its long lifetime in the lower stratosphere. I.e. if ozone is anomalously high in one month it is likely to be still higher than expected in the following month. We have added a corresponding remark.

5. We now show deseasonalized EP fluxes in Figure 1 as requested by both referee #1 and #2. The mean annual cycle of the EP-flux proxy in the northern and southern hemisphere is additionally shown in two small insets.

6. Use of abbreviations. Changes made as suggested.

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