

## ***Interactive comment on “Interannual variation patterns of total ozone and temperature in observations and model simulations” by W. Steinbrecht et al.***

**W. Steinbrecht et al.**

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In response to the reviewers comments, we have prepared a revised manuscript, which we intend to submit to ACP. With several additional figures and corresponding text this revised manuscript addresses most of the reviewers concerns. In the following we respond to the reviewers comments, and indicate how they have been addressed in the revised manuscript.

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## 1. Response to Reviewer #1

The main points criticised by reviewer #1 are a.) a lack of quantitative plots for time series and zonal mean differences between observations and models; b.) a not quantitative enough description of differences and a too uncritical discussion of the models, especially of E39/C; and c.) a lack of significant and important information in many of the plots for tropospheric temperature at 400 hPa.

To some degree this critique is justified. We agree that it can be problematic to just show the results of a complex regression. To give a better indication of the performance of the regression (or lack of it), we have added several time series plots, as suggested, for typical examples at low, mid and high latitudes. For a more quantitative description of the agreement and disagreement between observations/ reanalyses and the simulations we have also added, as suggested, line plots for zonal mean results. The reviewer correctly points out that both models have definite short-comings, which were highlighted in the recent CCMVAL intercomparison. Nevertheless, our analysis indicates that for most metrics we have investigated, the 40 year transient simulations from both models give results that agree with observations and reanalysis. The models do not fully reproduce the TOMS/SBUV long-term ozone trends. This is both due to some model deficits, but also due to a lack of observations in the Arctic in the TOMS/SBUV data, especially in the very cold winters of the 1990s. These aspects are now emphasized in abstract, conclusions and in the text.

The tropospheric information presented in the discussion paper is indeed not always substantial and important. Following the advice of the reviewer, and also considering the addition of several new plots, we have decided to omit all tropospheric plots and their discussion. This helps to focus on the stratosphere in the revised version of the manuscript. The title has been changed accordingly, and text has been modified throughout. Many passages were reworded, and several new paragraphs were added to address points that were criticised by the reviewers. Due to the addition of the zonal

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mean results, some numbers have changed slightly in conclusions and abstract. The main message of the paper, however, remains unchanged.

regarding reviewer #1's point 1.) The regression was applied to each grid point of each data set independently (for each of the four seasons). This is stated on pages 9214 and 9215 of the ACPD paper. In addition, for the revised manuscript, we now state that interdependences between the different predictors are generally low (correlations  $< 0.2$ ) and are not a serious problem. In fact, when one or more predictors are left out of the regression, results for the remaining predictors do not change very much. This has been checked by many tests and is stated in the discussion paper (pg. 9215, lines 16 to 26). For the revised manuscript, we have added a reference to Steinbrecht et al. 2003, where questions regarding interdependences between predictors are discussed in more detail. The fairly high correlation between tropospheric temperature and ENSO, is an exception and is specifically mentioned several times, e.g. on page 9215, line 16 of the ACPD paper. All points in each plot come from the same regression run. Except for the ENSO plot, which does not use 400 hPa temperature, all plots use the same set of predictors. The different treatment for the ENSO plot is mentioned on page 9227, line 25.

regarding reviewer #1's point 2.) While it is true that some results for E39/C are different, overall the regression results for the E39/C simulation are not fundamentally different or substantially worse. See also the newly added zonal mean figures. We disagree with the statement that our analysis fails to validate E39/C for long-term trend results. Nevertheless, we have changed the manuscript text to be more critical about the models, and more critical about the capability of our simple regression method.

regarding reviewer #1's point 3.) This point is also brought up by reviewer #2. If we were to use data up to 2002 or later, it would certainly be advisable to use some form of a non-linear trend, because the previously steep ozone decline has not continued after some point between 1995 and 1997 (Reinsel et al., 2005). However, since both model runs ended in 1999, and the observations are only used up to 2000, the effect

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of a non-linear trend would be minor, and not very relevant for the current analysis. We have added a few sentences for the revised manuscript.

As suggested, we have added a line plot comparing zonal mean trends. Questions about the latitudinal variation, and about differences between observations and models are now discussed in more detail, in additional paragraphs.

regarding reviewer #1's point 4.) The reviewer is absolutely correct to point out possible problems with temperature trends from reanalyses, particularly in the troposphere. We are aware of this problem, but for the lower stratosphere several papers (e.g. Ramaswamy et al., 2001) indicate that NCEP reanalyses can be used. The general agreement between the simulated 50 hPa trends and NCEP reanalysis in our paper also seems to indicate that it possible to look at trends from this reanalysis. Possible problems with reanalysis have been mentioned on page 9212, lines 16 to 28. They are obvious in the Southern hemisphere around 1979 in Figure 1 and are discussed in the ACPD paper. The large change in the reanalysis from the introduction of satellite data in late 1978 is also the reason why the disussion paper only shows 1979 to 2000 temperature trends, a time frame where the observation system used by NCEP is more consistent. Nevertheless, we have added a paragraph and references that discuss possible problems with temperature trends derived from the NCEP reanalysis, particularly in the troposphere. We have also followed the reviewers advice and have removed all tropospheric plots and their discussion.

regarding reviewer #1's point 5.) We have added a few sentences to clarify this. The reviewer is correct in pointing out that the regression primarily returns a coefficient for each proxy, plus error bars for this coefficient. However, by itself the coefficient does not show how large its associated ozone (or temperature) variation is. Also, use of the coefficients alone makes it hard to compare ozone variations e.g. from the QBO, where the coefficient has units of DU/m/s, with ozone variations e.g. from the solar cycle where the coefficient has units of DU/W/m<sup>2</sup>/Hz. Therefore in the dicussion paper the coefficents are not plotted directly, but rather the magnitude of the ozone (or

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temperature) variations associated with their respective proxies. In all plots a second scale gives the (approximate) size of the coefficients, in their respective units. The magnitude of ozone (or temperature) variations can be measured by two standard deviations of the associated time-series term  $c_X X$  in Eq. 1. Note that this standard deviation should not be confused with the standard error of the derived coefficient.

We agree with the reviewer that our results only show that QBO effects coming from the nudged real wind field are more or less correctly propagated to the ozone and temperature fields of the model. We have added a sentence to point this out.

regarding reviewer #1's point 6.) Inconsistencies with the NCEP data may be a problem for solar cycle effects in the troposphere, which are not discussed any more in the revised manuscript. For the stratosphere, however, we are convinced that the derived solar-cycle effect on temperature is realistic: a.) Despite the change in the observing system around 1979 we see nearly the same magnitude and patterns, whether using the complete 1958 to 2000 data set, or the 1979 to 2000 data set only. b.) The observed total ozone pattern and its magnitude are quite similar to the results for 50 hPa temperature - consistent with most other variations. c.) Both models generally reproduce the results derived from the NCEP reanalysis. They also reproduce the similarity between total ozone and temperature patterns. All 3 points give us substantial confidence that the derived solar cycle variations are indeed realistic. We have added a few sentences to point this out. The zonal asymmetry is significant at least in those plots where we have areas of significant positive and of significant negative response. Since both are significantly different from zero, they must also be significantly different from each other.

regarding reviewer #1's point 7.) We have reworded the text, and added some discussion, e.g. regarding formation conditions for PSCs in cold years. We have also added some references on Annular Modes, and their relation to the polar winter stratosphere.

To better account for the large cold pole bias and late Southern vortex breakup in the

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E39/C simulation, we have modified the Figure for Southern vortex strength. Instead of showing results for September to November, where the E39/C simulated vortex is still stable, we now show results for December/January/February, which includes the time when the too late breakup occurs in the E39/C simulations. This provides more comparably results than the old figure.

regarding reviewer #1's point 8.) As mentioned, we have followed the reviewers advice and removed all tropospheric plots and their discussion. Regarding the small panels: We feel that the authors criticism would be fully appropriate for a print-only journal. There, small graphics can not easily be enlarged, and can be a real problem to decipher. Atmospheric Chemistry and Physics is, however, an electronic journal. For electronic files, it is no problem to use the zoom function and instantly enlarge any figure to look at the details. Enlarged versions of our plots show substantial detail.

regarding reviewer #1's point 9.) As mentioned, throughout the revised paper we have modified the text to be more critical of the model results. Nevertheless, our analysis shows that the models do a reasonable job in reproducing the main observed variations in total ozone and lower stratospheric temperature.

## 2. Response to Reviewer #2

We thank reviewer 2 for his positive review, and his useful comments. We agree with the reviewer that there are problems with the long-term consistency of the NCEP re-analyses. This is also mentioned by reviewer #1 and is discussed above, in our response to reviewer #1's criticism.

As mentioned, in our analyses the derived solar effects were not affected much even by the substantial temperature change in the NCEP reanalysis around 1979. We have compared the solar effect for 50 hPa temperature, derived from the NCEP data set

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since 1958, with the same effect derived for the NCEP data set since 1979. Differences are minor. This was found not only for solar cycle effects, but also for most other proxies, except for the long-term linear trend. Since most other changes in the observing system are smaller than the large change around 1979, we have substantial confidence that the derived temperature solar-cycle effect is not affected much by changes in the observing system. The fairly good agreement between the results for total ozone, where the data are largely corrected for satellite changes, and the, possibly more affected, results for 50 hPa temperature also indicates that there are no major errors in our solar cycle results.

We agree with the reviewer that there is no complete information on the vertical distribution of variations in our paper. A full discussion of the vertical distribution would require analysis of a different ozone data set (e.g. the merged SBUV profiles). It would generate an extraordinary amount of information: The current plots are just a specially selected fraction of all the available information. With just 2 atmospheric parameters (total ozone and 50 hPa temperature), about 10 proxies, and 4 seasons, you need about 80 plots to present the complete information. Using 5 altitude levels, this increases to 400 plots, from which about 10 holding the core information would have to be selected. We feel that this would clearly exceed the scope of the paper at this point. It is, however, something that should be looked at in the near future. Similarly a discussion of the processes behind the observed solar cycle effect in our paper would exceed the scope of the paper. Instead we have added a paragraph which gives better context and several references for solar cycle effects.

We agree with the reviewer that a linear trend term alone will not be sufficient for analysing data sets that end after about 2002. However, the current model runs end in December 1999. For consistency reasons we only used TOMS/SBUV and NCEP data until December 2000. Given these shorter records and a possible trend turning point sometime between 1995 and 1999, the benefits of adding a change of trend term are small. We agree with the reviewer that most modes of variability would not be affected

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much by such a refinement. However, we have added a sentence indicating that a non-linear trend term is advisable in the future.

### 3. References

Ramaswamy, V., et al.: Stratospheric temperature trends: observations and model simulations, *Rev. Geophys.*, 39, 71–122, 2001. See also [http://www.atmosphysics.utoronto.ca/SPARC/News18/18\\_Ramaswamy.html](http://www.atmosphysics.utoronto.ca/SPARC/News18/18_Ramaswamy.html).

Reinsel G.C., Miller, A.J., Weatherhead, E.C., Flynn, L.E., Nagatani, R.M., Tiao, G.C., and Wuebbles, D.J.: Trend analysis of total ozone data for turnaround and dynamical contributions, *J. Geophys. Res.*, 110, D16306, doi:10.1029/2004JD004662, 2005.

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