**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Attribution of stratospheric ozone trends to chemistry and transport: a modelling study" *by* G. Kiesewetter et al.

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We thank Anonymous Referee 3 for his/her constructive comments and suggestions on how to improve our manuscript. Referee comments are cited in *italicized* font.

We have revised the manuscript, trying to include as far as possible the suggestions by five referees. The most important changes in our manuscript are:

- We have conducted a rerun of the model runs analyzed in the study. In this rerun,

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the temperature dependence of our linearized ozone chemistry scheme (Linoz) is switched on in the upper stratosphere as well. As a consequence, former Fig. 8 has been dropped.

- We have added a model run in which the empirical polar ozone depletion rate is scaled with $EESC^2$, in order to include an upper estimate for the influence of polar chemistry on mid-latitude column ozone trends. In the runs analyzed in the original manuscript, the polar ozone depletion rate was scaled linearly with EESC.

- The trend analysis methodology has been changed. We now apply the method of connected piecewise linear trends described by Reinsel et al. (2002). This eliminates difficulties due to the misalignment of trends at the intersection of the two trend analysis periods, 1979–1999 and 2000–2009.

- The whole modelled TO3 dataset is analysed in one piece now, including overlaps between ERA-40 and ERA-Interim driven periods. While accounting for an offset between the different meteorological reanalysis periods, equal trends are used for regressing the overlap period, thus increasing the robustness of the analysis.

- A regression analysis of TO3 differences between model and observations is used to remove solar cycle and aerosol signals from the observational time series. This has resulted in a new section (now Sect. 4) and an additional figure (now Fig. 6). The modified observational time series, which is better comparable to modelled ozone, is then used in the trend analysis.

- We have included an explicit analysis of changes in column ozone trends. This has resulted in a new figure (now Fig. 9).

- The analysis of profile trends has been extended to the period 2000-2009.

- We have tried to make the specific findings of this study clearer. In particular, the abstract has been altered, and the conclusions have been completely rewritten. Although our major conclusions do not change, new points have been added that emerge from the revised regression analysis (e.g. discussion of the significance of trend changes).

Replies to comments by Referee 3:

Page 17492, line 9: Why was 1999 chosen as the breakpoint in your trend analysis given that ODS concentrations in the stratosphere peaked long before then? Is it an issue that your selected breakpoint is 5 years later than in the Hadjinicolaou study cited on line 12 of page 17493?

The selection of 1999/2000 as a breakpoint is a compromise between high and low latitudes. While mid-latitude EESC peaked around 1997, polar EESC peaked in 2001. A break point as early as 1994 is hardly justified by the EESC curve, and especially in the mid-1990s effects of Pinatubo aerosol played a major role. Using 1999 as a break point allows us to analyse contributions to trends from both polar chemistry and gas-phase chemistry in a period before and after their (approximate) maximum. Shifting the pivot point in our regression analysis by two years does not affect our results significantly, and thus we do not expect a significant effect from the slight delay of the mid-latitude EESC peak in our study.

Page 17493, line 11: Is it the polar chemistry or the ozone that's exported to lower latitudes?

What is meant here is the dilution of ozone-depleted polar air masses after the breakup

of the polar vortex. We have clarified this in the text.

Page 17493, line 12: No mention here of changes in stratospheric aerosols, especially sulfate aerosols? The Mt. Pinatubo eruption caused a far bigger decline in total column ozone over northern midlatitudes than ODSs.

We have clarified this in the text.

Page 17494, line 9: To what extent does ignoring e.g. Pinatubo, which significantly suppressed ozone close to the time of maximum ODS concentrations, affect your conclusions?

In this context, ignoring the direct effect of volcanic aerosols is an advantage of our CTM, since all changes in modelled ozone must be caused by either changing chemical composition or meteorology. In order to improve comparability with observations, we now use a regression analysis to eliminate direct volcanic and solar signals from the observational time series (indirect effects are partly present in our CTM through the external wind and temperature fields). The observational time series with solar and aerosol signals removed is then used in the trend regression analysis for comparison to modelled ozone trends. Using the "solar cycle- and aerosol-free" time series instead of the original TOMS/SBUV and GOME/SCIAMACHY/GOME2 time series causes minor changes of 1979-1999 trend estimates, up to 1 standard deviation or 0.1% of year 1980 TO3 per year in NH winter and spring (more negative in the uncorrected time series).

Page 17495, line 2: What are the differences between these two ERA data sets and

how did you deal with these differences during their overlap period (1989-1999)?

Owing to the different data assimilation algorithms applied, the ERA-40 and ERA-Interim datasets have a number of differences. Wind and temperature fields do not agree perfectly, especially in regions where observed data are scarce, such as the upper stratosphere. ERA-40 and ERA-Interim temperatures show considerable differences in their overlap, including constant offsets, differing long-term trends, and also relatively sharp jumps. E.g., ERA-Interim shows a steep increase in upper stratospheric temperatures around the turn of the century that is not present in the ERA-40 runs. Over the period 1989–1999, a linear regression yields differences of up to ~ 10 K/decade in the uppermost stratosphere, partly due to the steep increase in ERA-Interim. Due to the new regression analysis method we have applied, differences are neutralized as ozone from the different model runs are forced to a common trend during the overlap period, which indeed solves some of the problems.

Page 17496, line 22: These EESC parameters are appropriate for the Antarctic stratosphere, but what did you use for other regions? If you used the same parameters for the whole global, your EESC will peak too late outside of the Antarctic stratosphere and I suspect that this will affect your attribution. Maybe you are saved by the fact that the attribution happens inside the same model that ran from the prescribed EESC, but I think that this point needs to be clarified in the manuscript text.

Two different evolutions of stratospheric chlorine are used: The EESC curve referred to in the quoted passage (which peaks in 2001) is used for scaling the rate of polar ozone depletion. Gas phase chemistry is scaled to tropospheric source gas concentrations that are interpolated linearly between 1975, 1997, and 2007 and used with a delay of 3 years to account for the average stratospheric age of air. Thus the gas phase chemistry effectively uses an EESC curve that increases linearly from 1978-1999, peaks on 1 Jan 2000, and decreases linearly to 2009. This choice may not be ideal for

all regions of the stratosphere but fits the two trend analysis periods best.

Figure 1: Having the E4 and EI axes also shifted with respect to each other hides the huge differences in ozone between the two simulations that result from the temperature biases between the ERA-40 and ERA-Interim reanalyses.

While we do agree that the TO3 biases between ERA-40 and ERA-Interim reanalyses must be mentioned and discussed (which they are), we do not feel that Figure 1 hides information. Biases are explicitly mentioned at several places throughout the text and quantified in the paragraph referring to Figure 1. Also, the axis shift is mentioned explicitly in the figure caption.

Page 17498, line 27: Is it just the wind fields or also the temperature fields that drive these large differences?

Both wind fields and temperature fields show differences between the two reanalysis datasets used here. Ozone differences are a result of both, and the use of the expression "wind fields" was misleading here. We have corrected it.

Page 17499, line 3: Is the aim here to quantify the ability of the model to reproduce inter-annual variability, or to quantify the ability of the model to reproduce the ozone changes in general?

The aim here was not explicitly to quantify the consistence of inter-annual variability, but rather consistency of the time series themselves (which is almost the same in the 1989-2009 ERA-Interim period). We have clarified this in the text.

Page 17500, line 21: You can perfectly adequately fit the regression model to the data over the full period and then use the EESC fit coefficient to say what the effective linear trend would have been over e.g. 1979-1999.

We agree that there are good reasons to apply an EESC fit to ozone observations. However, the chemistry in our model is constructed in a way that favours piecewise linear trends – Linoz chemistry tables are interpolated linearly between 1978, 1999/2000, and 2010. Moreover, a linear trend regression model can be applied to all sensitivity runs equally well, while there is no reason to use an EESC fit to a model run with constant chemistry (cc). In an EESC fit model, the fit functions should change with every model run, depending on which processes it includes. We believe using one and the same model for all model runs is more consistent, and have therefore decided to apply the piecewise linear model with connected trends as suggested by the Referee.

Page 17500, line 23: You can use the whole 32 year record in one piece. [...] Better still you can even then apply the regression model to ALL of your model output. I would strongly suggest that you follow the methodology outlined in Reinsel, G. [...]

We thank the Referee for these suggestions. We have altered our analysis to apply the piecewise linear model described by Reinsel et al. (2002) and use the whole CTM output in one piece now, allowing for an offset between the two meteorological reanalysis periods. We believe that the study has been significantly improved by this approach, while our conclusions remain largely unchanged.

Page 17501, line 7: It's not clear to me how you obtained the uncertainties on the fit

coefficients from the residuals.

The uncertainties were calculated from the diagonals of the covariance matrix of the fit residuals, $\sigma_{coeff} = (\mathbf{A}^T\mathbf{A})^{-1}\sigma_{res}$, where A is the fit matrix, σ_{res} is the covariance matrix of the residuals, and σ_{coeff} is the covariance matrix of the fit coefficients. In the case of TO3 trends, time series of monthly TO3 with 1 year interval are analysed in order to arrive at monthly trends, which are then averaged to seasonal trends. Due to the 1 year spacing, the TO3 time series are not autocorrelated. In the case of profile ozone, we analyse the whole time series in one piece, assuming a 1-month autocorrelation model as is frequently done, and thus the standard errors of trends are calculated with the formulas provided by Reinsel et al. (2002). We have added a section on the regression methodology to clarify these issues.

Page 17501, line 12: No, the agreement of trends does not in any way indicate that your model captures the pattern of variability. The pattern of variability could be completely different but still have the same trend as the observations.

This is true, and we have changed the sentence in order to avoid misleading language. What is meant here is that the model captures long-term changes in ozone well.

Figure 6: Something is very wrong with this figure. There is absolutely no way that the SON total column ozone trends over Antarctica from 2000-2009 are negative and even more unlikely that they are larger than the trends from 1979-1999 shown in Figure 5. This just absolutely does not make sense to me.

As explained in the text, the negative trends in the Antarctic were strongly influenced by the unusual winter of 2002. As suggested by the Referee, "*Page 17502, line 8: You see I think this problem will go away when you follow the approach I have listed above*

where you include two linear trends terms in the regression.", applying the Reinsel et al. (2002) approach has indeed eliminated this problem. Trends are positive (although not really significant) at the poles now.

Figure 7: I wonder how different this figure would look if the analysis was also done using ozone number density rather than ozone mixing ratio?

We have changed the plot to show trends of number density now. Since relative trends are shown, the overall picture does not change much (differences may only arise as a result of temperature trends). Of course, the figure would change if absolute trends were plotted, as the relevance of upper stratospheric ozone is decreased. In the period of 2000–2009, such a figure shows that due to the very large trends in observed upper stratospheric ozone, the upper stratosphere contributes to the slight positive offset of observed versus modelled trends.

Page 17503, line 7-9: How does this conclusion mesh with the findings of Engel et al. (2009)?

The significant negative trend in tropical lower stratospheric ozone that is described here is only observed during the 1979-1999 period. During the 2000–2009 period, no significant (2σ) negative trend is observed, and trends are zero or positive above 400K.

Page 17503, line 10: How do you know that it's the influence of polar ozone depletion and not just increasing halogen driven gas phase ozone depletion?

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The Referee is right here. At this stage we do not distinguish between influence of changing polar ozone depletion and effects of changing gas phase chemistry. We have changed the text accordingly.

We thank the Referee for several corrections and comments on typographic, grammatical and stylistic issues.

References.

Reinsel, G. C., Weatherhead, E. C., Tiao, G. C., Miller, A. J., Nagatani, R. M., Wuebbles, D. J., and Flynn, L. E.: On detection of turnaround and recovery in trend for ozone, J. Geophys. Res., 107, 4078, doi:10.1029/2001JD000500, 2002.