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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 No. 1 for his/her constructive comments to our manuscript. Original referee comments are quoted 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, 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 1:

One small thing that I feel is missing, is a statement on the magnitude of chlorine/EESC changes over the two considered periods (1979 to 1999, and 2000 to 2009).

As requested, we have incorporated a more detailed description of the EESC used in our study, which are actually two different curves. Polar chemistry is scaled to EESC with a mean Age of air = 5.5yr, while the gas-phase (Linoz) chemistry is scaled to tropospheric source gas concentrations adding an age of air of 3 years. Polar EESC doubles between 1979 and 2000, peaks in 2001, and then declines by $\sim 5\%$ until 2009. Tropospheric chlorine loading doubles between 1975 and 1997 (which are used for scaling stratospheric concentrations of 1978 and 2000), and decreases by 10% between 1997 and 2007 (used for 2000 and 2010, respectively). Passages have been added in the sections describing gas-phase and polar chemistry.

Page 17492, line 19: Can you give a percentage value for this fraction?

We have included quantifications of the trend contributions in the abstract.

Page 17492, line 20: You might want to say that the ODS changes themselves are *much smaller than over the 1979 to 1999 period. The ozone sensitivity to ODS is probably the same.*

We have followed the reviewer's suggestion.

Page 17493, line 7: Also: what about Newchurch et al., JGR, 2003?

We have included the cited reference here and now refer to this study in the discussion of trend changes (now Sect. 5.2).

Page 17493, lines 18 and 23: I think you also have to mention the Mt. Pinatubo eruption here. It has resulted in the very low ozone levels observed between 1992 and 1995, and has a major impact on trend estimates until the late 1990s.

We have changed the text accordingly.

Page 17494, line 3: Don't you mean EESC-varying? Isn't your chemistry always depending on EESC/ chlorine?

This is correct, and we have clarified this in the text.

Page 17496, line 10: Can you be more specific here? How unrealistic do the T-trends have to be, to throw the ozone trends off significantly? Are we talking 1 K per decade off, or 10 K per decade off?

Since the treatment of upper stratospheric ozone in our study has been criticised by reviewers 2, 3, and 4, we have decided to change our model configuration to include full T dependency, and dropped the cited paragraph. 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, some 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 addressed in the cited paragraph. Nonetheless, agreement between modelled and observed upper-stratospheric ozone trends is slightly decreased in the fully T-dependent model runs, which may point to either problems in the meteorological reanalyses, or in Linoz.

Page 17498, line 4: To be consistent with the GSG data set, the website for the TOMS/SBUV dataset should be given as well.

We have included this reference in the acknowledgements.

Page 17498, line 18: It would probably be good to add an observation-based reference for the magnitude of the 11-year solar cycle in total ozone as well, e.g. WMO 2007, or one of Lon Hood's papers.

We have added a reference to Soukhaev and Hood (2006).

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Page 17500, lines 11 to 13: I would drop this last sentence here. When I look at Fig. 4, I don't see an underestimation of trends. The agreement is almost perfect, especially if I consider that the shift between curves is somewhat arbitrary anyways (and Pinatubo aerosol and solar cycle are missing in the model).

Since we have included an additional model run with stronger scaling of polar chemistry that is also shown in the mentioned figure, we have decided to modify but not drop this sentence. Mentioning the slight underestimation of the polar TO3 trend in the tt run here (that is indeed difficult to diagnose with the naked eye in Fig. 3 (formerly Fig.4)) is an anticipation of Fig. 7 (the difference between tt and TOMS/GSG trend in Antarctic OND is larger when the pure TOMS/GSG time series as shown in Fig. 4 is regressed instead of the one used in Fig. 7, in which the solar and aerosol signals are removed). In contrast, the tT run clearly over-estimates the magnitude of the trend.

Page 17504, lines 1 to 11: I think it would be important to give some numbers for the difference in T-trends in the E4 and EI analyses.

See our reply above. Since we have conducted a rerun of the model runs in which the Linoz temperature dependence is switched on in the upper stratosphere, this passage has been dropped. Due to large inter-annual variability and temperature jumps, temperature trends are prone to large uncertainties in both ERA-40 and ERA-Interim datasets, and we have avoided an exact quantification in our manuscript, in particular as observed temperature trends exhibit large uncertainties in the upper stratosphere as well (Randel et al., 2009). In the improved ozone trend regression model, ozone trends during the overlap period are forced to a common trend, thus largely eliminating the effects of inconsistencies in temperature evolution.

Fig. 10: Maybe this would get a bit cluttered, but: Would it not make sense to also plot the tt trends in Fig. 10?

We have followed this suggestion, which has hopefully contributed to improve clarity in Figs. 11 and 12 (formerly Figs. 10 and 11). In addition, also the relevant quantities for a stronger scaled polar chemistry are shown ($tT - tc$, the polar chemistry contribution, and tT, the overall trend).

Also: To me it would be quite helpful to add in the legend “polar” after “ct-cc”, “gas-phase” after “tt- ct”, and “meteorology” after “cc”.

We have followed the reviewer’s suggestion.

page 17507, line 24: You might want to also quote Hood , McCormack and Labitzke, JGR, 1997, or Steinbrecht et al., JGR, 1998, some of the first papers to mention large meteorological contributions to decadal ozone trends.

We have followed the reviewer’s suggestion.

page 17508, line 2: Certainly the large ozone declines in the lower stratosphere are also a big contributor to the temperature trend in the lower stratosphere. This needs to be mentioned.

This is correct. However, trend contributions originating from meteorological changes are not distinguished into effects of changing circulation and temperatures throughout the rest of the paper, and in an effort to condense the scientific discussion in our paper as requested by referees 2–5, we have decided to drop the whole paragraph, as it rather distracts the reader and does not add significant conclusions.

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We thank the reviewer for helpful corrections and suggestions regarding typos, grammar, and style.

References.

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 17491, 2010.