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Comment

Interactive comment on “Attribution of stratospheric ozone trends to chemistry and transport: a modelling study” by G. Kieseewetter et al.

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We thank the Referee for his/her comments on our manuscript. 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,

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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 EESC², 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.

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- 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 5:

General comments.

I think that this paper is not clear enough about which processes are in the different runs (and therefore what are the key driving factors for the ozone changes) and the paper is too long.

The focus of our study is to enable attribution of ozone changes to contributions from changing gas phase chemistry, changing polar chemistry, and changing meteorology. With the simple linearized ozone chemistry used, further distinction between different chemical reactions is impossible, since either all source gases used for creating the Linoz tables (N_2O , CH_4 , and halocarbons) are changed in time or left constant at year 2000 levels. We have tried to clarify the distinctions between different processes more strongly, e.g. by providing more details about the Linoz ozone chemistry scheme. Due to several requests for clarifications from five referees, and a revision of the regression methodology that resulted in an additional discussion of the significance of trend changes (in Sect. 5.2), we have not succeeded in shortening the paper, but we have attempted to clear out unnecessary passages that impeded the flow of reading.

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There is also a lot of mention of agreeing with past studies, without real emphasis on what is new.

We have now 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).

*Details of Linoz scheme. Please give details of the chemistry used in the model which constructs the Linoz coefficients. Depending on this, there are a number of issues which need to be addressed. I presume this Linoz model *does* include midlatitude heterogeneous chemistry (e.g. $N_2O_5 + H_2O$).*

The Referee's assumption is correct. Linoz does include mid-latitude heterogeneous chemistry. Accordingly, the term "gas phase chemistry" is not entirely correct for describing the Linoz ozone chemistry. However, aerosol levels are kept at clean background levels throughout the study, and it is reasonable to assume that gas phase chemistry is dominant in driving ozone changes induced by Linoz in our study. For simplicity, we decided to keep the term "changes in gas phase chemistry" to describe ozone changes inferred by the Linoz scheme. We have expanded Section 2.2 describing the Linoz chemistry scheme (a detailed discussion is provided by Hsu and Prather, 2009), and we have added a passage to clarify the issue of mid-latitude heterogeneous chemistry.

Please also say what Linoz does in the polar region - are there any inconsistencies with adding in the additional polar ozone loss term?

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Heterogeneous activation of CI on polar stratospheric clouds is explicitly not included in the Linoz scheme. Ozone lifetimes in lower stratospheric polar spring are of the order of a few months to a year, and hence there is no interference between the Linoz chemistry and an additional polar chemistry (which has ozone lifetimes of a few days). An additional polar chemistry must be employed in order to generate an ozone hole – e.g. Hsu and Prather (2009), who describe the Linoz tables used in this study, use a parametrization of polar stratospheric clouds (Cariolle et al., 1990) that is similar to the polar chemistry scheme used in this study. We have added a sentence in the manuscript (end of Sect. 2.2) to clarify this issue.

You also need to say what changes between 1978, 2000 and 2010. Obviously Cl and Br source gases, but also N₂O and CH₄? In that case you need to be careful when analyzing these runs that you don't ascribe differences from Linoz to just "halogens". I noted that you use "ODS" – N₂O would qualify as that but CH₄ is not so clear. In any case most people would take ODS to be halogens. Use of the word recovery would open a debate if the runs were not just changing halogen source gases.

As stated above and in the revised manuscript, Linoz does account for changes in all relevant source gases (N₂O, CH₄, and halocarbons). Changes in Linoz are ascribed to (mainly) changing gas phase chemistry, and although we do mention the turnaround in EESC as the main driver of Linoz changes between the two analysis periods, we avoid a sole attribution to just halogens.

Chemical ozone loss following volcanic eruptions. In the past many studies have looked at the role of heterogeneous activation on enhanced aerosol has played on mid-latitude ozone loss. This study really ignores this issue. Reference is made to the dynamical effect of aerosols but not the chemical effect. The implication is that

midlatitude aerosol chemistry is not needed to explain the ozone changes? You need to be clear on this.

We have added a section (now Sect. 4) in which we explicitly analyse to what extent differences between modelled and observed ozone levels can be explained by aerosol variations and the solar cycle. Offsets between modelled and observed TO3 are well explained by these two parameters, enabling us to construct an observational time series with solar cycle and aerosol signals removed. This time series, which is better comparable to the CTM output, is then used in the trend regression analysis in Sect. 5. In general, we believe that the “deficit” of the model chemistry, ignoring aerosol changes, can also be seen as an advantage, since all ozone changes present in the model must be induced by changing source gases or meteorology, and the ambiguity of ascribing late-1990s ozone changes to Pinatubo recovery or EESC turnaround is avoided. Apparently, the manuscript text has been misleading, and we have tried to clarify this in the revised manuscript. We do not propose that enhanced aerosol, e.g. as a consequence of large volcanic eruptions such as Pinatubo, is unimportant for mid-latitude ozone.

Generally the paper is well written. However, it is long (i.e. a lot of text is often used to describe figures or refer to other parts of the paper) and there are a number of awkwardly constructed sentences.

We have tried to tidy up typos, awkward sentences, and unnecessary passages that impeded the flow of reading.

References

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