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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.

Anonymous Referee #2

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This study uses a simplified model to attribute observed stratospheric ozone trends to changes in chemistry, transport, and temperature. This is an interesting, important topic and is of interest to the readership of ACP. And I am convinced that there are important questions that may be addressed with this model study.

However, the 'take-home message' from the paper as it stands is disappointing. The paper says that the effect of ODS change is visible in the model runs and that the agreement with observations is excellent. Further, the main conclusion from the paper is that "Observed ozone changes can be reproduced well with the CTM driven with meteorological reanalyses, indicating that the observed evolution of ozone over the

past decade is consistent with our current understanding of chemistry and transport" or that "the evolution of the stratospheric ozone layer as observed during the last three decades is very well explained in our CTM by the combined effects of changes in gas-phase chemistry, changes in polar chemistry and its export, and meteorological variability". This is not really new, is it? Moreover, these conclusions are reached without taking changes in upper stratospheric temperatures into account and without a representation of volcanicly enhanced aerosol in the model. Does this mean that these two processes are of little relevance for stratospheric ozone? I do not think so.

On the other hand, I believe that important conclusions emerge from the study the authors have conducted that are not properly discussed in the paper or, at least, not clearly stated in the conclusions and in the abstract. The most important questions are:

- The quantification of the contribution of polar ozone loss to mid-latitude trends.
- Is there a trend in lower stratospheric tropical ozone?
- Is the ozone layer recovering at the poles and at mid-latitudes. Is there support for studies that state that recovery is already detectable?
- How much halogen driven chemical ozone loss has occurred before 1980?

I recommend to focus on these questions and work out more properly the answers emerging from the work done here. I believe this could be very valuable.

In summary, I think that problematic points of the study (neglect of upper stratospheric temperature dependence, neglect of volcanic aerosol, linear fitting of ozone trends) should be discussed in more detail (and possibly changed) in the paper. At the same time, a lot of important results are hidden in the text and the paper could make a much greater contribution than in its present state, when these results are more clearly and

more cleanly worked out. A paper revised along these lines should be acceptable for publication in ACP.

Comments in detail

- The onset of recovery (page 17493, l. 7): The citation (Newman et al., 2006) refers to the recovery of the Antarctic ozone hole, which occurs on a different time scale than mid-latitude recovery (WMO, 2007); these two time scales need to be distinguished here. Further, there are studies (e.g., Newchurch et al., 2003; Yang et al., 2008, and references therein) that claim that the onset of recovery (first stage of recovery) has already occurred in several regions of the atmosphere. It would be important for the present study to take notice of these papers and make a statement whether or not the results of the study agree with the notion that the onset of recovery has already occurred.

- A deficit of the study is that it neglects both the influence of the solar cycle and of volcanic eruptions. While the former point results only in a modulation of ozone on a 11-year time scale the latter point is problematic, as sulfate aerosol impacts the ODS driven chemistry that the paper focuses on (e.g., Portmann et al., 1996; Solomon et al., 1996). This aspect should be be discussed in more depth in the paper than in the present version.

- The temperature dependence of gas-phase chemistry in the upper stratosphere: First, the fact that upper stratospheric ozone chemistry is highly temperature dependent is well known (IPCC/TEAP, 2005) and it is therefore not surprising that this fact is reflected in the Linoz chemistry scheme. Second, it is clear that there is a significant temperature trend in the upper stratosphere (Randel et al., 2009) and, clearly, this trend must have an impact on trends of upper stratospheric ozone. Therefore, I do not understand how fixed climatological temperatures, as they are assumed here, can be justified. I understand that there can be model

problems and I see that different temperature trends in the two ERA data sets make life difficult. But just ignoring the temperature dependence of upper stratospheric ozone chemistry is not a solution.

- It is stated in the paper that a “significant part of the decadal-scale deviations between modeled and measured TO3 in Fig. 1 appears to be related to the 11 yr solar cycle, which is not directly accounted for in our CTM”. However, e.g. using multiple linear regression models and other time-series analysis tools (e.g., Fioletov, 2009; Mäder et al., 2010), it is possible to remove the 11-year solar cycle from the time series. Why is it not attempted here to create such a ‘solar cycle free’ observational time series for better comparison with the model results?

- Column ozone trends: I am not convinced by the arguments presented here for using a linear piecewise fit in place of an EESC fit. It has been shown (e.g., WMO, 2007; Mäder et al., 2010, and references therein) that it is possible (or even preferable) to use an EESC fit over the time period when the EESC trend changes sign. I suggest revisiting this issue and try employing EESC as an explanatory variable.

- Tropical ozone trends: There is currently a scientific debate about whether or not there is an ozone trend in the lowermost tropical stratosphere. Vertical profile measurements seem to indicate a reduction in lowermost tropical stratospheric ozone in agreement with model results (V. Eyring, T. G. Sheperd, and D. W. Waugh (Eds.), 2010) whereas total ozone measurements (including those analyzed here) do not show a trend. It would be very interesting if this study could make a contribution to this debate by making clear statements as to the possible cause of this discrepancy.

- Polar ozone trends: A positive aspect of the results is that the temporal variability of polar ozone in both hemispheres is well captured, as the paper correctly states. However, from Fig. 4 it is not obvious that the polar trends are underestimated, as

it is stated in the text discussing this figure. Also visible in Fig. 4 is a substantial low bias in SH column ozone. This low bias is in contrast with the authors suspecting that they are underestimating the period over which chemical ozone loss occurs in their simplified chemistry scheme. These issues should be discussed. Further, in Fig. 4 the average over the polar region is taken from a latitude of 65°; I recommend using 63° as it is conventional (WMO, 2007). Moreover, the authors should consider using the minimum of daily average total ozone poleward of 63° equivalent latitude in spring, recently suggested as a more appropriate measure of polar chemical ozone loss (Müller et al., 2008).

- Polar ozone loss: The quantity used here to diagnose polar ozone loss (monthly average total ozone over the polar region is not a very good measure of chemical loss. A number of more sophisticated methods have been developed and applied over the last decades (e.g., Harris et al., 2002; Rex et al., 2002). In particular, using a passive ozone tracer to compare with the chemically active ozone in CTMs has proven to be an effective method to diagnose modelled polar ozone loss (e.g., Singleton et al., 2005, 2007). I recommend considering to better quantify the chemical polar ozone loss in the model, e.g. by implementing a passive ozone tracer.

- It is stated that substantial polar ozone loss is present in the model before 1980. But this point is neither more precisely quantified (is it more that 30%?) nor discussed in detail. But of course this is an important point. Commonly, 1980 is used as a reference value for ozone recovery ('return to 1980 values').

Minor issues

- The presentation and the wording throughout the paper is often somewhat unclear. For example: Linoz-mode and polarchem mode (p.17497). Theta levels (p.

17503). Jargon like that should be avoided throughout the paper. Further there are some errors in the English, e.g., tabled should be tabulated.

- Overall the wording regarding climate induced changes is a bit confusing: the words meteorology, dynamics, transport, and temperature are all used. The wording should be consistent and precise throughout the paper.

- page 17493, l. 11: the changes are likely not only possible.

- page 17494, l. 3: be more clear and more explicit here: what you mean is 'constant halogen loading' and 'EESC dependent halogen loading'.

- page 17496, l. 16: What is the condition for the onset of ozone destruction?.

- page 17498, l. 25-28: The offset gets worse when switching from ERA40 to EI. Isn't that worrying as EI should be the superior product?

- p. 17500, l. 5: more citations are required here than just Sinnhuber 2009 (e.g., Portmann et al., 1996; Solomon et al., 1996).

- p. 17501, l. 29: It is not surprising that there are small differences here – EESC does not change much of this time period, so the differences must be small. The analysis should go beyond this point.

- p. 17502, l. 7: I suggest to list more papers here for the 2002 event than just Sinnhuber 2003.

- p. 17503, l. 15-18: To me this is bad news. A well known driver of upper stratospheric ozone trends is neglected in the model and the model represents the observations. I would suspect then that the model will no longer represent the observations if the observed temperature trend is taken into account.

- ECMWF is 'European Centre for Medium-Range Weather Forecasts'

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