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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evidence for the effectiveness of the Montreal Protocol to protect the ozone layer" *by* J. A. Mäder et al.

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We thank the referee for his/her valuable comments and suggestions on the paper.

Ref #2: "The authors should put their results in the context of previous studies showing the stabilization of stratospheric ozone. Does this method provide an earlier detection of 1st stage ozone recovery as the CUMSUM method?"

The basic idea of the CUMSUM method and our approach is similar, namely the comparison of a linear (downward) ozone trend with deviations from a linear ozone trend as expected from the evolution of ODS achieved by the Montreal Protocol. The main difference of our study and earlier work based on the CUMSUM method are: (i) we use a backward elimination approach for the selection of covariates (as described by

Maeder et al., 2007), whereas other proxies were applied in the studies following the CUMSUM method, (ii) our concept is more qualitative in the sense, that we quantify the proportion of stations following EESC instead of LT and no statistical significance of a deviation from a linear trend is attempted. This also implies that we don't intend to make statements regarding the "steps of ozone recovery".

For reference to earlier studies using the CUMSUM approach the following paragraph (see lines 117 ff) was included in the revised manuscript.

"Previous studies reported that "simple" linear trends were inadequate to characterize the long-term behavior of column ozone due to the stabilization in recent years (e.g., Reinsel, 2002; Reinsel et al., 2002; Newchurch et al., 2003). Within these studies the so-called CUMSUM method, i.e. the difference of the cumulative sum of residuals from a linear trend, was applied to show that the ozone loss rate has diminished "

Ref #2: "The EESC term used in the regression lacks explanation in the article" We changed the EESC used in the study to the "standard" WMO A1_2010A scenario as suggested by another referee. This had no significant influence on the results and conclusions of the manuscript, providing evidence for the robustness of our approach. A detailed description of the WMO A1_2010A EESC scenario is given in the revised version of the manuscript (see footnote on line 70).

Ref #2: "More explanation is needed on the regression terms selected by the model for various latitude bands . . ."

The revised manuscript contains more information on variable selection based on the backward elimination procedure, see paragraph below (lines 145-158).

"The independent variables (or so-called "explanatory variables") included in the analysis have been selected according to the results of Mäder et al. (2007). The applied backward elimination approach consists of multiple steps. First, for each station, a sequence of elimination for 44 potential explanatory variables is determined by applying

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stepwise backward elimination based on the p-values of the regression coefficients. This sequence defines a rank for each variable at each station. The ranks are then averaged over all stations in a given geographical zone, and the variable with the lowest rank is dropped from the set of potential explanatory variables. Then, the ranking step for each station is restarted with the remaining 43 variables. This procedure is repeated until only one variable is left, leading to a ranking table for each geographical zone (north polar, northern mid-latitudes, tropics, southern mid-latitudes, south polar). The final model for each zone includes only the highest ranking variables. Its size is determined by the number of significant variables and the explained variance (R2) as described in Mäder et al. (2007)."

Ref #2: ". . . For instance in the Southern polar stratosphere, the selection of PV470 and EL is puzzling, since both proxies should be highly correlated in this region. Likewise, the authors should better explain the inclusion of the residual seasonal variation (M term) and to what mechanism they attribute this term."

We agree that the selection of both PV470 and EL for the Southern polar stratosphere is scientifically puzzling. However, there are additional concerns regarding saturation effects of ODS in the Southern polar stratosphere stated in the manuscript (see lines 285-289) and therefore the results of the Southern polar stratosphere should be ignored.

The term M describes the dependences of the column ozone amount on the individual calendar months, taking into account the systematic seasonal variation (note that we do not use deseasonalized data) (see Mäder et al., 2007). This variable is therefore empirical and does not allow attribution to physical processes and therefore we put the word "residual" in brackets in table 1 to avoid confusion with an interpretation as residual circulation, i.e. Brewer-Dobson Circulation, which cannot be directly attributed to the term M.

Ref #2: ". . . to what extent the results are sensitive to the inclusion of a larger number

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of proxies and to the iteration algorithm itself."

We did not explicitly test whether the results on the preference of EESC over LT depends on the number of included proxies. However, we tested for individual stations the stability of the rankings of the proxies showing that the ranking of the approximately 6 highest ranked covariates are reliable (see Fig. 2 in Maeder et al., 2007). Regarding zonal bands we also tested the adequate number of proxies (see Fig. 5 in Maeder et al., 2007). We found that the adjusted R2 substantially increases when additional proxies until up to 6 are added to the model. Additional results on the sensitivity analysis are also presented in Tab. 2 in the manuscript.

As we use a simple sign test over multiple stations (and zonal bands) the significance of the difference between R2 using LT or EESC was not tested. We agree with the referee, that the explained variance as well as the residuals (as shown in the supplementary material) is only slightly larger using EESC instead of LT. However, we argue that this is not the critical point in our analysis as we make use of the statistics of the large number of individual stations. Indeed, this statement is only valid as long as the individual stations contain independent information, which was tested by studying the spatial correlation (see lines 265-273 and Fig. 3 in the revised manuscript). We also tested a potential dependence of the results on the record length (using the T value (see new Figure 2).

Ref #2: "The discussion of figure 4 (the most interesting one of the study) should be expanded . . ."

Figure 4 (in the resubmitted manuscript) shows that the number of ozone series following EESC rather than LT increases with time, which supports the results. All regions besides Southern Polar (panel e), which should not be considered as mentioned before, start in the 1990s around zero, meaning undecided between EESC and LT. At the tropical stations this does not change over time (see panel c). The two Northern zonal bands (see panels a and b) show clear preference of EESC over LT from the

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late 1990s onwards. The picture for Southern mid-latitudes (see panel d) is similar, however unfortunately the number stations is too small to derive a “clear” preference of EESC over LT.

We hope that these comments and the changes in the revised manuscript improve the quality of the paper and clarify the points raised by Ref #2.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 19005, 2010.