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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.

Anonymous Referee #2

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This study uses multi-regression analysis of ground-based total ozone time series in order to evaluate the effectiveness of the Montreal Protocol on stratospheric ozone. For that purpose, two regression models are tested, which use a different function to mimic the effect of ozone depleting substances on ozone levels, e.g. a linear trend starting in 1970 and the equivalent effective stratospheric chlorine (EESC) function, that describe the evolution of ozone depleting substances in the stratosphere. The study shows that the number of ground-based stations for which the EESC based model provides the best fit is increasing since the mid-nineties, providing evidence of the effectiveness of the Montreal protocol.

Although previous works have already pointed to the stabilization of ozone levels in the stratosphere (e.g. Newchurch et al., 2003, Reinsel et al., 2002), emphasizing the fact that linear trends are not suitable anymore for the representation of long term behavior of ozone, this study is interesting as (1) it is based on a different approach in terms of multi-regression analysis and (2) it provides rather robust results by scrutinizing the various ground-based total ozone time series available at WOUDC. The article is well written and suitable for publication in Atmospheric Chemistry and Physics. However some revision is needed to improve the quality of the manuscript, as described below:

Major comments:

1. The authors should better 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 of ozone recovery as the CUMSUM method?

2. The EESC term used in the regression lacks explanation in the article. How is it defined and from what source it is taken?

3. More explanation is needed on the regression terms selected by the model for the various latitude bands. Indeed, this study uses the results of a former one (e.g. Mäder et al., 2007), but the selection of some proxies still seems somewhat arbitrary as no sufficient explanation is provided in the former study. For instance, in the Southern polar stratosphere, the selection of both 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.

4. The article should quantify the test value T with respect to the variance explained by both models. The supplement material shows that the difference between both fits is very small as compared to the explained variance (on the order of 0.3 as compared to 94% explained variance). To what extent the results are sensitive to the inclusion of a larger number of proxies and to the iteration algorithm itself?

5. The discussion of figure 4 (the most interesting of the study) should be expanded

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and the figure itself should be presented in a better way, showing the results for the various latitude bands in 3 different panels for example. The simple exponential fit is not satisfactory as it indicates a preference for the EESC proxy already in 1990, when both proxies should not be distinguishable from the results. The somewhat strange results obtained for the Southern polar region (dashed blue curve) should be emphasized and better explained.

Minor comments

P19007, l4: Provide more citations for the decline of ozone.

P19007, l16: Explicit the differences in the evolution of the ozone layer as found in the Hegglin and Shepherd study.

P19009, l10: Explicit to what extent the validation of climate models is a challenging task.

P19011, l5: Equation 1 should be better explained. For example the term M is poorly described: is it the seasonal variation of total ozone or the residual seasonal variation? Some indices (month, latitude) should also be included.

P19013, l1: The sentence is not clear to me, please explain.

P19013, l5: the average time span of ozone data at each station seems quite low for this study. Can you comment on that?

P19014, l8: the Robock et al. study deals with the effect of Mount Pinatubo eruption on the atmospheric circulation and not on ozone.

P19014, l28: Figure 2 should be better described and explained.

P19015, 20: The sentence is not clear. Do you mean that the difference between northern and southern polar latitudes is due to the saturation of the ozone loss in the Antarctic?

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Interactive Comment

References

Reinsel, G. C., E. C. Weatherhead, G. C. Tiao, A. J. Miller, R. M. Nagatani, D. J. Wuebbles, and L. E. Flynn (2002), On detection of turnaround and recovery in trend for ozone, J. Geophys. Res., 107(D10), 4078, doi:10.1029/2001JD000500.

Newchurch, M. J., E.-S. Yang, D. M. Cunnold, G. C. Reinsel, J. M. Zawodny, and J. M. Russell III (2003), Evidence for slowdown in stratospheric ozone loss: First stage of ozone recovery, J. Geophys. Res., 108(D16), 4507, doi:10.1029/2003JD003471.

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