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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Reevaluation of stratospheric ozone trends from SAGE II data using a simultaneous temporal and spatial analysis" *by* R. P. Damadeo et al.

Anonymous Referee #2

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1 Overall

The paper describes a new, more complex, multiple linear regression approach for analysing ozone and aerosol time series and trends from the SAGE II instrument. This new method accounts better for SAGE II sampling characteristics, and provides similar but more reliable variability estimates, compared to "classical" results based e.g. on monthly zonal means. SAGE II has provided one of the longest data records on stratospheric ozone and aerosol from 1984 to 2005. This is clearly an important paper on an important dataset, and is well suited for publication in ACP. Overall, the discussion pa-

per is in good shape, but I feel there are some areas where improvements are needed. My corresponding suggestions are listed below as major and minor comments.

Generally, however, this discussion paper will be publishable once these concerns are addressed.

2 Major comments

Section 2 discusses used proxy variables and their orthogonalization at length. The resulting proxies are somewhat different from what is often used in regressions. I think it is absolutely necessary to explicitly show the time series for these resulting proxies. A corresponding Figure should be added.

In Section 4, the regression methodology does not really become clear to me. Nowhere the paper clearly shows what is regressed against what. This really major point has somehow gotten lost. Instead, the discussion focuses very much (too much?) on residuals and statistical details. Eq. (5), in my opinion, is wrong. In the current form, the regressed temporal series $T(t)$ would be the same everywhere, and a latitude dependence $\Theta(\theta)$ would "distribute" the regressed time series to different latitudes. That is clearly not what the authors did. Rather, the authors probably used

$$\eta(\theta, t) = \sum_i \beta_i(\theta) X_i(t) \tag{1}$$

where $\beta_i(\theta) = \sum_j b_{i,j} L_j(\theta)$ is a sum over Legendre-Polynomials $L_j(\theta)$, $X_i(t)$ is the i th predictor time series, and the coefficients $b_{i,j}$ are determined from the entire dataset(?) by the fitting procedure?

This should be clarified/ corrected. The authors should clearly explain what is actually fitted.

In Sections 4 and 5.1, I am missing a few simple statements explaining what the total, correlated and uncorrelated residuals really are. I am assuming that they are from Equation B7 (pg. 17689, line 25). If I understood it right: The total residuals (top panels of Fig. 2) give the total residuals R_i in Eq. B7, and the uncorrelated residuals (bottom panels of Fig. 2) give the $\sigma_i \epsilon_i$ in Eq. B7. The total residuals are obtained in the 1st regression fit, the uncorrelated residuals are obtained only after iterative corrections / Cochrane-Orcutt transformations. Did I get that right? It would certainly help to add some clarification to Sections 4 and 5.1.

Section 7: I feel that the authors tend to over-emphasize differences between monthly zonal mean (MZM) and simultaneous spatial and temporal (STS) regressions. To me, the different panels in Figs. 12 and 13 look very similar. I wonder if the minor differences are really significant. I realize that the MZM must lead to granularity in the latitude direction, different from STS which should be smooth in latitude direction. But why are the STS results not granular in altitude direction, like the MZM results? Why are MZM results not stipled as insignificant, even when the trends are close to zero? I think the authors need to check that the MZM and STS results are really plotted in the same way. The should also be careful and not over-emphasize the differences. The authors may disagree, but my feeling from their results is that the old MZM method is actually doing quite a good job, and produces overall results that are comparable to results from the STS method. Of course the STS method is more advanced, does a better job in a few respects, and, particularly, gives better confidence that some possible sources of error are avoided, and results are more reliable.

3 Minor comments

Abstract: The abstract should give numbers for the results: What are the SAGE uncertainties? How big are the sunrise-sunset differences? How big are the QBO, ENSO,

solar cycle effects? How large are the pre-1997, post-1997 trends? How do these results compare to previous studies. This also applies to Section 8, which needs to put results into better perspective with respect to previous studies, e.g. summarized in the series of WMO-UNEP ozone assessments.

pg. 17682, line 21: An introduction should provide wider context from existing publications. Damadeo et al., 2013 is not a wide-context reference for SAGE II that has been providing good data and many papers since 1984. Some key papers from previous decades should be cited here. This criticism applies throughout much of the paper, where only papers from 2009 to 2013 are cited and the extensive body of work done with SAGE data in the 1980s, 1990s and 2000s is largely unreferenced / ignored. The authors should please make the effort and provide more scientific context, especially in Sections 1, 5.2.x, 7, and 8!!

pg. 17683, line 6: What is meant by swath? Define / be more explicit.

pg. 17683, line 8 (and many other places in the text): "based off of" → "based on" ??

pg. 17685: It remains unclear what QBO proxies are used. Two orthogonal equatorial EOFs only? Sidebands with annual modulation? This could also be achieved by allowing for annually varying amplitude of the QBO fit. As suggested above, it would help to show and discuss the final proxy time series.

pg. 17686, lines 7-11: I think a plot showing a typical daily sampling pattern would help a lot here (or a reference to an existing plot).

pg. 17694, lines 6 to 10: This ambiguity between solar-cycle and volcanic aerosol signals (both peaking near 1983 and 1992) is a very old problem. There is an old Susan Solomon paper from 1996(?) that should be cited here!!

pg. 17695/6, Section 6: Here (and in a few other places, e.g. pg. 17698, lines 1-2), the comparison between MZM and STS is not always fair: MZM does not differentiate between sunrise and sunset events, STS does. However, MZM could also split between sunrise and sunset events. Then only the spatial/temporal biases between STS and MZM would show up! Sunrise / sunset differences would not alias into the MZM vs. STS comparison. I think this should be stated clearly here, and in several other places (e.g. pg. 17698, lines 1-2).

pg. 17697, lines 14 to 25: Kyrölä, 2013 and many other studies consider data up to 2013. Here, however, only SAGE data up to 2005 are considered. This should be stated very clearly!!

pg. 17698, lines 3 to 23: This "cherry picking" should be avoided: The two "orthogonal" EESC terms (I'd like to see them!) happen to pick a plausible altitude dependence of the turnaraound year in the Northern Hemisphere, but not in the Southern Hemisphere. Why? So are the orthogonal EECS terms good or not? The piecewise trend turnaraound date could also be changed /fitted. What would happen then? Statements based on spurious or unclear evidence should be avoided!

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 17681, 2014.