Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-1144-AC1, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Seasonal stratospheric ozone trends over 2000–2018 derived from several merged data sets" *by* Monika E. Szeląg et al.

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Received and published: 16 April 2020

We would like to thank Reviewer #1 for the comments and the effort that the reviewer has put into this paper. Please see our response below.

MAJOR COMMENTS

Reviewer: Pg. 04, Ln. 10: "The two-step approach is equivalent to the one-step regression . . ." The two approaches are not equivalent. While the results of the fitting coefficients tend to be very close to the same (it depends on the nature of the data and the regression model), the truth is that the uncertainty analysis becomes less robust, because covariance information between dependence on the different proxies is lost with each step. The multistep procedure tends to result in smaller uncertainties than

a single step procedure, but this is because those uncertainties are artificially biased low from the lost information, not because it is necessarily a better process. On that note, technically this is a three-step regression since it is performed on deseasonalized anomalies (i.e., the first step). In lieu of doing things different, I would merely state that the multi-step approach is meant to avoid the use of more complicated regression models applied to a reduced amount of data at the expense of potentially biasing the uncertainties low.

Authors: In the revised version, we will write "nearly equivalent". We would like to note that the autocorrelations of residuals are taken into account in our regression method also in the second step, therefore the method should not bias the uncertainties low. In the revised version, we will add that autocorrelations are taken into account in both steps.

Reviewer: The authors describe their "two-step approach" on page 4 (Lns. 15–33) where the data is fit with Eqn. 1 with more data to derive the natural cycles before removing them and then fitting two different trends over two different time periods. This is intended to offer sufficient data to properly fit the natural variability. Generally, the wording here is fine (e.g., "thus providing more accurate fitting of these proxies" and "thus providing smaller sensitivity to defining turnaround point") but I would add a caveat. Namely, Eqn 1 allows for the use of the PWLT term to potentially alias into some of this natural variability such that its removal for step 2 will affect the residuals when the trends are fit.

Authors: Of course, any method for detecting cycles includes assumptions about the trend function (linear, piecewise, smooth), which might affect the estimated amplitude of the cycles. This is not a caveat for our method, but for multiple linear regression in general.

Reviewer: Pg. 05, Ln. 15: The paragraph that starts here (and goes onto the next page) discusses potential reasons why Method 1 would result in smaller uncertainties than Method 2. The seasonal dependence of the QBO, for example, is a likely culprit but multiple potential reasons exist. One is simply that using data from all seasons would require a more complicated QBO model in the regression and so the residuals are greater and potentially seasonally dependent. This is easily analyzed by looking at the amplitude of the QBO in the regression from the coefficients between the different Methods.

Authors: Indeed, the QBO amplitude is different in different seasons.

Reviewer: However, another is that the uncertainties in the trends computed in step 2 are done without any consideration of the uncertainties in the residuals from which they were fit.

Authors: No, uncertainties at the 2nd step are estimated from the fit residuals. We will state this in the revised version (PG. 05, Ln. 35). The uncertainties are smaller in method #1 because of better fitting the cycles (smaller residuals).

Reviewer: Pg. 10, Lns. 35–41 discuss the hemispheric asymmetry of the summer trends in the middle stratosphere at mid-latitudes (25-35km). This made me think to compare this work with what was shown in the last ozone assessment, as both use the same data sets. While WMO (2018) does not attempt to look at the seasonal trends, it does look at the overall trends. As such, I would expect the black lines in Figs. 5 and S3 to agree with what is shown in Fig. 3-19 of the Assessment. While much of the results are in agreement, I see some clear offsets and discrepancies, particularly between CCI in the Southern/Northern Hemisphere and SOO in the Northern Hemisphere. This makes me wonder if the term "All" in the figures in this paper are using all of the data at once in the analysis or if they are instead some sort of average of the results from different seasons. If the former, I would expect better agreement between this work and the Assessment. If the latter, then each value should be weighted by its seasonal mean to better represent the former, otherwise it does not really carry any meaning. This does not necessarily mean that the results from the seasonal regressions are

incorrect, but it does make me wonder if double checking that all of the results shown here are accurate is needed. If the data presented are accurate, then I would want to know why they disagree with those results from the Assessment.

Authors: In our analysis, yearly trend estimates ("all") are obtained using all the data at once. We would like to note that:

(1) slightly different latitude zones are used in the WMO (2018) report;

(2) different time periods are used in WMO assessment (2000-2016) and in our paper (2000-2018);

(3) different regression methods are used in WMO-2018 report (independent linear trend regression) and in our work (two-step regression).

And, despite these differences, the trends in WMO-2018 report and yearly trends in our paper are close to each other. We compared directly the trends presented in Figure 3-19 of WMO-2018 report (V. Sofieva produced this figure) with the yearly trends from our analyses, they are shown in Figures 1 and Figure 2 below.

The most pronounced difference is the reduced trends in Northern Hemisphere. Most probably, the main impact is due to two-year extension. This reduction of trends due to 2 years extension is also observed using traditional trend analysis.

MINOR COMMENTS

Reviewer: Pg. 01, Ln. 35: "The Antarctic ozone hole is showing some signs of recovery, and the first signatures of global recovery . . ." I would recommend adding a reference to the first part of this sentence such as Solomon et al. (2016) (DOI: 10.1126/science.aae0061)

Authors: We now add the reference as suggested by the Reviewer.

Reviewer: Pg. 04, Ln. 01: "The CCI and SOO datasets provide deseasonalized ozone anomalies. For GOZCARDS and SWOOSH, the deseasonalized anomalies were computed in the same way. The seasonal cycle was evaluated using the data from 2005-2011." Was the seasonal cycle computed as averages of data in each month or using some sinusoidal fit? I am assuming the former but please specify.

Authors: Yes, the seasonal cycle is computed via averaging the data in each month. We will clarify this in the revised version.

Reviewer: Pg. 04, Ln. 25: ". . . 2-month lagged ENSO proxy . . ." Why two months?

Authors: The MEI ENSO index is defined as a 2-month average. It was used with 2-month lag in several trend analyses, for example, (Bourassa et al., 2014; Randel and Thompson, 2011; Sofieva et al., 2017). The reason for the lagging is somewhat smaller residuals in the stratosphere. However, the sensitivity of the fit to lagging by 1-2 months is small (Petropavlovskikh et al., 2019). In the revised version, we clarify this.

Reviewer: Figs. 3 and 6 are very busy (and large) figures and have the same information content. Wouldn't it make more sense to only include one of them in the paper for discussion? The ratio of figure space to text space is quite large.

Authors: We think that Figure 6 is useful for discussion, and we would prefer to keep it in the manuscript.

References

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poe, N., Stiller, G., Laeng, A., von Clarmann, T., Walker, K. A., Sheese, P., Hubert, D., van Roozendael, M., Zehner, C., Damadeo, R., Zawodny, J., Kramarova, N. and Bhartia, P. K.: Merged SAGE II, Ozone_cci and OMPS ozone profile dataset and evaluation of ozone trends in the stratosphere, Atmos. Chem. Phys., 17(20), 12533–12552, doi:10.5194/acp-17-12533-2017, 2017. Petropavlovskikh, I., Godin-Beekmann, S., Hubert, D., Damadeo, R., Hassler, B. and Sofieva, V.: SPARC/IO3C/GAW Report on Long-term Ozone Trends and Uncertainties in the Stratosphere, edited by M. Kenntner and B. Ziegele, [online] Available from: https://elib.dlr.de/126666/, 2019.

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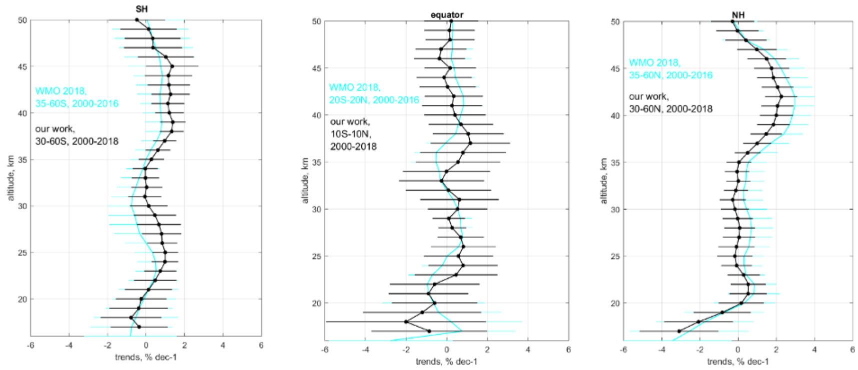

Fig. 1. Comparison of post-2000 ozone trends for SAGE-CCI-OMPS datasets. Cyan lines: WMO 2018 assessment (2000-2016), black lines: yearly trends in our work (years 2000-2018).

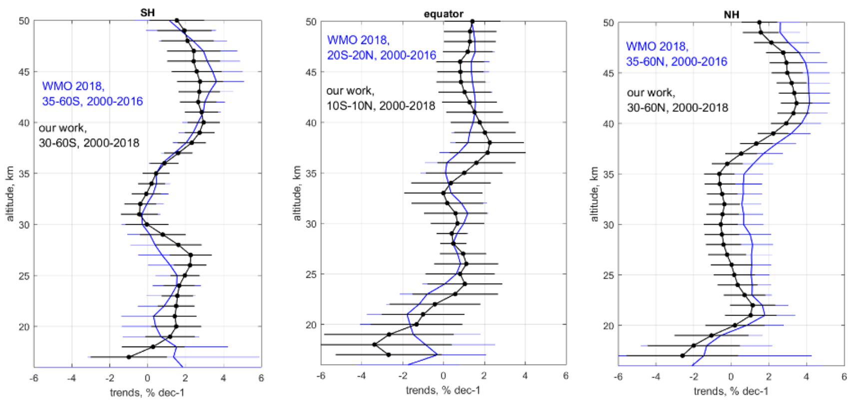

Fig. 2. As Figure 1, but for SAGE-OSIRIS-OMPS dataset (SOO). Blue lines: trends from WMO-2018 assessment, black line: our analysis.