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Atmospheric Chemistry and Physics Discussions

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Interactive comment

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

Anonymous Referee #1

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This work uses four composite data sets used in the last ozone assessment as part of a regression analysis to determine the seasonal trends in stratospheric ozone. The methodology is similar to what has been used in previous works and is simple and straightforward. However, applying this methodology to what amounts to much smaller data sets requires some modifications to avoid very large uncertainties. These modifications require some caveats to be stated to avoid potential misunderstanding the limitations of the significance of the results. Otherwise, the results are interesting and appear mostly consistent with previously published work on temperature trends in the stratosphere, though there are some discrepancies with other published work that need to be addressed to ensure accuracy of the work performed here.

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Discussion paper

Major Comments

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.

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.

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. 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. This could be artificially biasing those trend uncertainties lower in Method 1, which ties back into my first Major Comment. I would worry about what the influence of a few potential outliers are on the trends without any of the covariance information between proxies getting captured in the uncertainty analysis. In general I do not argue with the notion of picking a Method and sticking with it, simply that the caveats about the different methods need to be mentioned/discussed in the paper. This methodology has its uses, but it has its limitations as well.

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.

Minor Comments

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)

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.

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

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.

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