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

Anonymous Referee #2

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Review of "Seasonal stratospheric ozone trends over 2000-2018 derived from several merged data sets" by M.E. Szelag et al.

This manuscript describes an analysis of seasonally-varying long-term trends in stratospheric ozone, focusing on the period from 2000-2018. The authors analyze and compare data from four high vertical resolution merged ozone data sets. The analysis follows well-established procedures using multiple linear regression to isolate long-term trend from other sources of variability. The paper is well organized and concise. The authors discuss the consistency of the ozone results with previously published seasonally-dependent stratospheric temperature trends. The temperature and ozone trends have comparable seasonal characteristics, which the authors reasonably suggest might be due to seasonal changes in the Brewer Dobson circulation in time (with

changing climate), such that in the upper stratosphere the mean ozone trend (positive) is due to photochemistry and decreasing ODS's, while the seasonal variability is dynamical. Highlighting the seasonal component of the trends in profile ozone is an important addition and is appropriate for publication in ACP. I recommend publication after the following comments, mostly minor, are addressed.

Of the four data sets, two have native units of volume mixing ratio on pressure, and two have native units of number density on altitude. If I understand correctly, each data set is analyzed and plotted in its native units. Could the authors say more about whether these should be directly comparable? They would not be if the pressure surfaces are changing in time relative to the altitude surfaces, that is, in the presence of a temperature trend. (see McLinden and Fioletov, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2010GL046012). I believe the answer is the temperature trends over the period are small, but this should be stated. Also, the seasonally-varying temperature trends mean this effect will differ by season. Do the authors see differences in the number density vs VMR trends that are consistent with seasonal temperature trends?

In the tropics, the authors show that both the trend and uncertainty can vary substantially between their method 1 (use seasonal time series only to fit and remove natural variability) and their method 2 (use full time series to remove variability). The authors state that fitting the natural variability proxies with insufficient number of points will cause larger uncertainties (P4 L17), and in P4 L30 that the two-step approach provides for a sufficient number of points. However, is this true for Method 1? In Method 1, only seasonal points are fit, and although three months of each year are fit, the proxies are all slowly varying such that they are highly correlated over the three-month segments of each year, so I question the assumption that there is a sufficient number of points to accurately fit to the proxies. The authors find that the uncertainty is generally less using method 1, but I'm not sure this is a true reduction in uncertainty. With the smaller number of points being fit, there may be higher correlation between proxies that is arbitrary (i.e. not physical). This would allow more cross-talk between the proxies in the regression, and thus a better fit, but not a physically meaningful fit. For example if there was correlation between the seasonal QBO and PWLT, the best fit in the regression might be a high coefficient on the QBO term, and an equally high but opposite sign coefficient on the PWLT, which when added allow the regression to better fit some short-term variability while the large-scale changes cancel. This would lead to smaller short-term variability in the residual (after QBO fit removed), but a larger PWLT term left behind, which then affects the trend segment fits in step 2. This is not necessarily the case, and the correspondence with the temperature trends lends support to the results being physical, but these caveats should be discussed. It would be ok to refer this to future work, but to investigate these possibilities, the authors might compare the derived ozone variability based on the full QBO proxy to that from the seasonal QBO proxies. Are the seasonal variations consistent with how we expect the ozone QBO to behave seasonally, and are they consistent in latitude and altitude, or generally noisy?

Minor Comments: P1 L36 Is there a specific reference for the Antarctic ozone hole recovery (more recent than 2015) in addition to WMO 2018?

P3 Table 1 Under Ozone Profile Representation, are the first two entries (deseasonalized anomalies) in units of number density (or concentration) or percent? I suggest adding the units to each column.

P4 L2-3 Suggest "For GOZCARDS and SWOOSH, the deseasonalized anomalies were computed relative to their 2005-2011 mean seasonal cycle." [These were not necessarily computed the same way as those initially provided as anomalies. I suspect that for those, the seasonal cycle was subtracted from each instrument individually before merging, which, if there are seasonal biases in the individual records, is different than using the seasonal cycle of the final merged record.]

P4 L9-10 Suggest re-wording slightly, "The two-step approach allows us to avoid fitting over the period when the ozone trends transition from negative to positive, and are not

well-represented by a linear function." I want to get across the idea that it is not only that we don't know the exact turn-around time (and this varies with latitude and altitude) but that the ozone change is not linear over this period anyway, so there may not be a well-defined turn-around time.

P4 L12-14 Suggest dropping the last sentence, as using dynamical linear modelling is not otherwise discussed.

P4 L27 certain period -> certain season

P4 L31-33 Similar to above, suggest slight re-wording of the last sentence. The authors do fit near the "trend turnaround point in 1997" if the decline segment fit is 1984-1997. It is not so much sensitivity to the turnaround time as it is that the ozone change in time does not look like the hockey stick representation (it is a curve rather than two intersecting slopes). What about "In the second step, fitting is only done during periods when the ozone change is approximately linear, thus avoiding the problem of how to properly model the ozone change in the trend turnaround period (such as sensitivity to trend turnaround time when using a hockey-stick representation)."

P5 L15 Related to above comments, with fewer points, the individual proxies may be more correlated, giving the regression more room to "play" (more degrees of freedom) and get a better fit, but that fit might not be physical.

P8 L1 Just clarifying, the analysis is redone for ozone averaged in the broad latitude bands, as opposed to averaging the trends in the smaller bands. This is how I read the text, I just want to be sure.

Editorial Comments/Typos: P1 L13, add comma after SWOOSH

P2 L11 -1 K dec-1 (add space)

P3 Table 1 Suggest some re-wording on the Merging Method: Median value of deseasonalized anomalies Average value of deseasonalized anomalies referenced to SAGE II Average value of original values referenced to SAGE II Average value of original values referenced to SAGE II

P4 L4 Trend analyses are usually performed using a multiple linear regression. . .

P4 L8 . . . two steps, by detecting and removing natural cycles in the first step, and estimating bulk changes over specific periods in the second step . . .

P4 L30 In the two-step approach, *a* sufficient number . . .

P4 L 34 Figure 2 illustrates each step of our analysis . . .

P4 L35 Seasonal data (3-months, method #1)

P5 L1 estimated for the period 2000-2008

P5 L9 like in -> as in

P6 Figure 2 Caption Last sentence, Right panel shows. . .

P6 L16 Figure 2 -> Figure 3

P7 Figure 3 GOZCARDS and SWOOSH titles give altitude range, but shouldn't these give pressure ranges. The caption also says pressure bands. I would suggest changing the word bands to ranges in the caption, but also include the pressure range in the plot titles rather than the altitude range.

P9 L8 are dominating over -> dominate in

P10 L2 Figure S4 does not match Figure 6.

P10 L10 larger positive ozone trends

P10 L14 due to greenhouse gas (remove 'the')

P10 L30 so do -> as are

P10 L31 additional confirmation of our hypothesis

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