Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-1144-AC2, 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.

Monika E. Szeląg et al.

monika.szelag@fmi.fi

Received and published: 16 April 2020

We would like to thank Reviewer #2 for the comments and the effort that the reviewer has put into this paper. Please see our response below.

MAJOR COMMENTS

Reviewer: 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, doi: 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?

Authors: We agree, the ozone trends in different representations and vertical coordinate can be different due to temperature trends. However, since the temperature trends after 2000 are small, we expect also a minor difference in ozone trends in different ozone representations. In the revised version, we note this and add the reference to the paper by McLinden and Fioletov, 2011. We have plot the difference of seasonal trends for mean(CCI, SOO) minus mean(GOZCARDS, SWOOSH), similar way to Figure 4 in the manuscript. The difference in the upper stratosphere is consistent with the pattern on temperature trends and predictions by McLinden and Fioletov (see Figure 1 below). In the revised version, we include this figure into the Supplement.

Reviewer: 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?

Authors: In the revised version, we add caveats that the correlation between proxies can be different for Method 1 and Method 2, thus the reduction of uncertainty can be not fully physical. The detailed analyses of proxy correlations can be subject of future studies.

MINOR COMMENTS

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

Authors: We added the reference (Solomon et al., 2016).

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

Authors: We now add the units to each column in Table 1.

Reviewer: 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 be- fore merging, which, if there are seasonal biases in the individual records, is different than using the seasonal cycle of the final merged record.]

Authors: Corrected as suggested by the Reviewer.

Reviewer: 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.

Authors: Corrected as suggested by the Reviewer.

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

Authors: Corrected as suggested by the Reviewer.

Reviewer: P4 L27 certain period -> certain season

Authors: Corrected as suggested by the Reviewer.

Reviewer: 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)."

Authors: Corrected as suggested by the Reviewer.

Reviewer: 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.

Authors: Please see our replies above, in the major comments.

Reviewer: 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.

Authors: That is correct.

EDITORIAL COMMENTS/TYPOS

Reviewer: P1 L13, add comma after SWOOSH

Authors: Corrected as suggested by the Reviewer.

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

Authors: Corrected as suggested by the Reviewer.

Reviewer: P3 Table 1 Suggest some re-wording on the Merging Method: Median value of desea- sonalized 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

Authors: Corrected as suggested by the Reviewer.

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

Authors: Corrected as suggested by the Reviewer.

Reviewer: 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 . . .

Authors: Corrected as suggested by the Reviewer.

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

Authors: Corrected as suggested by the Reviewer.

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

Authors: Corrected as suggested by the Reviewer.

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

Authors: Corrected as suggested by the Reviewer.

Reviewer: P5 L1 estimated for the period 2000-2008

Authors: Corrected as suggested by the Reviewer.

Reviewer: P5 L9 like in -> as in

Authors: Corrected as suggested by the Reviewer.

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

Authors: Corrected as suggested by the Reviewer.

Reviewer: P6 L16 Figure 2 -> Figure 3

Authors: Corrected as suggested by the Reviewer.

Reviewer: 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.

Authors: Corrected as suggested by the Reviewer.

Reviewer: P9 L8 are dominating over -> dominate in

Authors: Corrected as suggested by the Reviewer.

Reviewer: P10 L2 Figure S4 does not match Figure 6.

Authors: The part with Supplementary Figure S4 is now moved to correct place in the manuscript (Description of the Figure 4). Note, that the order of Supplementary Figures has changed.

Reviewer: P10 L10 larger positive ozone trends

Authors: Corrected as suggested by the Reviewer.

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

Authors: Corrected as suggested by the Reviewer.

Reviewer: P10 L30 so do -> as are

Authors: Corrected as suggested by the Reviewer.

Reviewer: P10 L31 additional confirmation of our hypothesis

Authors: Corrected as suggested by the Reviewer.

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

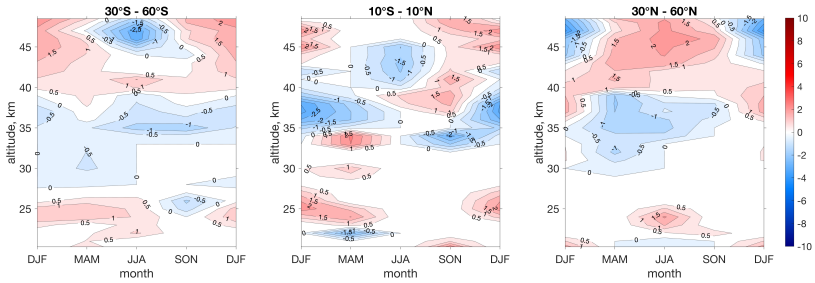

Fig. 1. Altitude-seasons variation of the difference of the seasonal trends (mean[CCI, SOO] minus mean[GOZCARDS, SWOOSH]), calculated over 2000-2018 for three selected latitudinal bands.