

# Response to referee interactive comments on “A mid-latitude stratosphere dynamical index for attribution of stratospheric variability and improved ozone and temperature trend analysis” by William T. Ball et al

## General comments relevant to both referees:

We thank both referees for very helpful guidance and suggestions in the background, justification and statistical analysis performed. This has led to significant improvements in the quality of the manuscript (which are already apparent in our current revisions and in our response below). Major changes in the revised manuscript have been highlighted in bold text.

We begin by highlighting major updates that both reviewers should be aware of:

- i) Figures 10, 11, 12 and 13 have been updated to reflect the use of AR1 instead of ARO. Figure 13 in particular has been completely changed, and numbers added, to clarify the error improvement and a change in the mean values. Figure 12 has been updated to reflect similar style and information as provided in Figure 11.
- ii) The point made in the manuscript about no aliasing between regressors being shown by the relative importance plots has been modified. Due to the use of AR1, for temperature, there is a redistribution of the relative importance from the original regressors without the new index to the new one, in addition to the increase in total variance accounted for. However, the fact it does not change the mean value of the regression coefficients in the trend still supports the claim that it does not alias the derived signal. Modifications to the text in the manuscript have been made to reflect this change, and discussion on this point has been added.
- iii) A significant amount of background have been added to the introduction.
- iv) We have renamed the MLSD index to the Upper-branch Brewer Dobson Circulation (UBDC) index, to reflect a more direct interpretation of what it represents, and for which it should be more easily understood; this changes the title of the manuscript.

## **L. Hood (Referee)**

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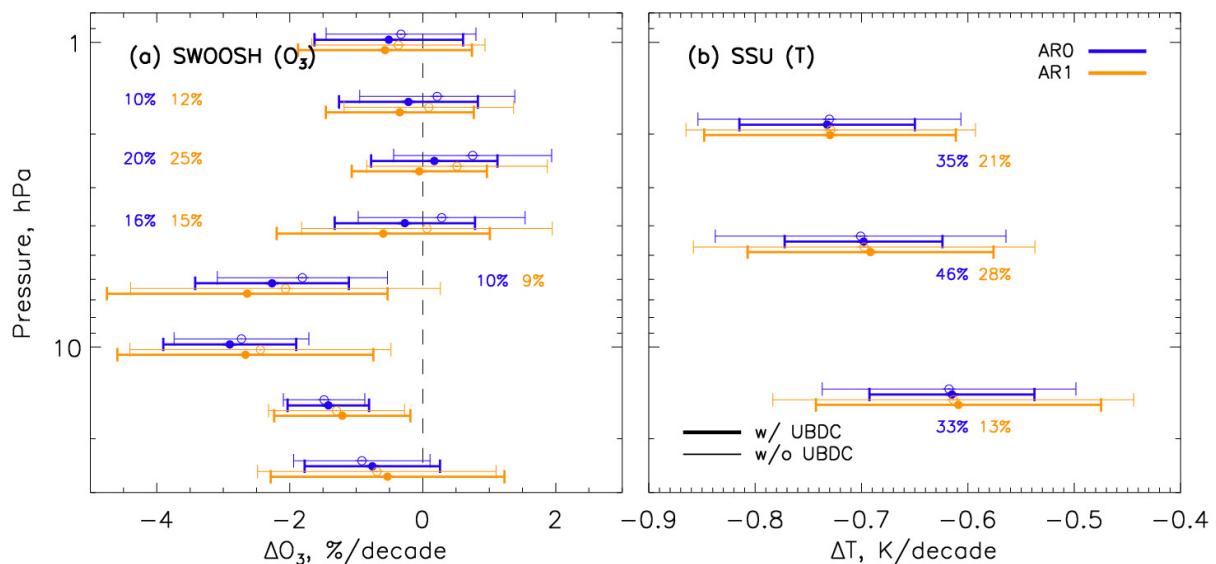
Overall, this is a useful effort to improve statistical estimation of stratospheric ozone and temperature trends and interannual variability by accounting for a source of shortterm (month-to-month) dynamical variability in tropical stratospheric data sets. The presentation is excellent and the figures are state-of-the art. However, the value of the adopted technique for trend estimation and its ability to “explain” a larger fraction of the variance in the observations is somewhat overstated, in my opinion. Some important revisions are needed prior to publication.

## **Main comments:**

(1) A major claim of the paper is that inclusion of the mid-latitude stratosphere dynamical (MLSD) index can reduce the uncertainty “on all multiple linear regression coefficients ... up to 45% and 25% in temperature and ozone, respectively.” First of all, the accuracy of these reduction estimates is questionable because, as mentioned on p. 11, line 12, “we do not consider use of any autoregressive modeling.” In other words, serial correlation (autocorrelation) of the residuals of the MLR analysis is not accounted for. It is possible that serial correlation of the monthly residuals is increased when the MLSD index is used because the month-to-month variability is reduced. Have the authors tested

whether this is the case? Accounting for any increased serial correlation would increase the uncertainty estimates. For example, application of a “pre-whitening” technique (e.g., Tiao et al. [1990]; Garny et al. [2007]) would ensure that the residuals are approximately white noise thereby yielding more reliable uncertainty estimates. Please re-do the analysis in this manner to provide such a test and yield more accurate (larger) uncertainty estimates.

These are indeed important points. Serial correlation is important, and you are correct that their consideration does indeed puff-up error bars. However, it does not change the main result and the usefulness of the index. To make this point clear we have produced a new plot, which we include in the paper, to emphasize that auto-regression will have an effect and should be considered. Further, we will replace Fig 13 with this one, since the point of Fig 13 is to show clearly how the reduction (and any effect on mean value) works in practice – this also addresses the second main point below. The plot is shown below: ARO (blue) and AR1 (yellow) are shown for cases with (thick lines) and without (thin) the MLSD index for SWOOSH (ozone, left) and SSU (Temp., right). We see the percentage change (in respective colours) at heights where we see the largest changes. In both cases, we still have a maximum improvement of up to 30% in the errors. Not shown here, but will be in the final manuscript, is that the index now increases  $R^2$  from a maximum increase of 40% (fig 10), to nearly 60% in temperature, and between 30 and 55% for ozone (depending on the dataset used). In the 1998-2012 periods, ozone error improvements are essentially unaffected at a 25% reduction in uncertainty, but the earlier 1983-1997 period is affected, on average reducing uncertainties by around 10% to a maximum of around 15%; some regions show a small increase in error, but this likely reflects the fact the datasets show different variability on all timescales (see response to question from the other reviewer on this point). We will update the manuscript to reflect this. [Additional note: we also considered AR2, but AR1 was sufficient to account for partial correlation at 1-month]



Second, even without accounting for serial correlation, the difference in the ozone and temperature trend results with and without the MLSD term shown in Figure 13 is not very impressive. For the sake of clarity, consider only the yellow curves in the figure. The error bars for with (thick curves) and without (thin curves) the MLSD cases overlap. These are presumably  $2\sigma$  error bars, right? If not, then the overlap is even larger. The error bars are roughly the same size at most levels. At 2.5 hPa, the ozone error bar appears to be about 25% smaller for the with MLSD case, which is consistent with the authors’ statement. But it is not a very significant difference considering the sizes of the error

bars and the large variation in the trend estimates from one pressure level to the next. For most of the other levels, the difference in size of the error bars is hard to discern.

We agree with these comments (the error bars are 2-sigma). In fact, we tried to make this clear with the grey shading in the old version of Fig 13a to highlight the altitudes where we see the largest improvement. In hindsight, the plot has such a large absolute range of profiles, that seeing this improvement is difficult. Figure 10 already shows similar results, that is the reduction in uncertainty as a function of altitude (right panels of each sub-plot) – the idea of Figure 13 was to show how it appeared in practice. By accounting for an attributable source of variability (or at least being able to show that it is not simply noise, but a clear dynamical factor) we make a step closer to better understanding those variables we are trying to determine (e.g. trend and solar cycle) – see point below. The new figure (above) reduces the absolute range and focuses in on one of the datasets. We consider this a more useful plot, and discuss and refer to other articles that do show the profiles.

(2) The other major claim of the paper is that use of the MLS index in a regression analysis can “explain much larger fractions of the total variability.” I am not sure that the word “explain” is appropriate. The dynamically induced variability is being accounted for in the MLR analysis but it is not really being explained. For example, the see-saw temperature and ozone variations between the tropics and extratropics are in many cases associated with minor and major polar stratospheric warmings in the winter hemispheres. The latter are modulated by a number of external forcings including the QBO and the solar cycle. A true explanation of the variability would therefore need to account for the external forcings that are controlling the rate of wave absorption events, which in turn produce the ozone and temperature fluctuations. I also disagree with the terminology “total coefficient of determination”, which is used in place of explained variance ( $R^2$ ) in the text. The words “determination”, “explained”, and “attribution” are all misleading if the sources of the dynamical fluctuations are not identified. Please revise the introduction and conclusions section to make this clear.

We are happy to clear up terminology. As the other reviewer also pointed out, the use of temperature mixes potential sources of the variance that correlates with temperature, but is actually the underlying driver, and we have added additional text to the introduction to account for this. The point we are trying to make is that we can ‘account’ for variability that is physical, and not simply noise that, unconsidered, would lead to higher uncertainty in quantities we wish to determine. It is true, the index itself doesn’t necessarily represent the underlying driver of the changes in the meridional flow, but it does act as a proxy and is related to a real variance in the system (which we relate through the EPFD to wave driving, as shown in the manuscript). We disagree about the use of the coefficient of determination,  $R^2$ , and would argue it is a useful quantity with which to test how much better our regression model, with the index, improves the amount of variability we can account for. By applying the bootstrapping (examples in Figs 11 and 12), we can also account for further statistical uncertainties to ensure that the improvement from the additional dynamical index is robust.

#### **Minor comments:**

(3) I agree with the other referee that the history of the ozone and temperature variations that are discussed in the paper and their application to trend analyses is not adequately summarized in the paper. The first report of the existence of such global stratospheric temperature oscillations with a change in phase between low and middle to high latitudes was by Fritz and Soules [1970]. Some stratospheric dynamicists still refer to these oscillations as the “Fritz-Soules effect”. See also, e.g., Andrews et al. [1987] for general discussions of their dynamical origin. Another observational study by Chandra [1986] could also be referenced.

We have added additional discussion and references as suggested by both referees (see response above to the first referee on this point). The reference by Chandra [1986] was particularly enlightening; our findings also confirm, and expand upon, the results from that study.

(4) In Figure 1 (and maybe other figures), the definitions of the diamonds in the upper right corner seem to be incorrect and are opposite to those given in the caption.

You are correct: the legend in the figure was wrong; this has been fixed; we also checked the other figures, which did not have this problem.

(5) P. 7, line 7. Adiabatically

We have corrected this.

#### References:

1. Andrews, D. G., J. R. Holton, and C. B. Leovy, *Middle Atmosphere Dynamics Academic Press*, 489 pp., 1987.
2. Chandra, S., *The solar and dynamically induced oscillations in the stratosphere, J. Geophys. Res.*, 91, 2719-2734, 1986.
3. Fritz, S., and S. D. Soules, *Large-scale temperature changes in the stratosphere observed from Nimbus-3, J. Atmos. Sci.*, 27, 1091, 1970.
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5. Tiao, G., et al., *Effects of autocorrelation and temporal sampling schemes on estimates of trend and spatial correlation, J. Geophys. Res.*, 95, 20507-20517, 1990